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published in the U.S.S.R. although frequent references are also made to the transactions of the Red Banner Military Communications Academy in Leningrad.

The term, microwave antenna, as used in the book, refers only to diffraction type radiators and thus excludes wire antennas. Specifically, six types of microwave antennas are discussed in the book. They are horn, lens, slot, dielectric, reflector, and surface wave. The first chapter contains an interesting chart illustrating this classification on the various subclassifications. For example, under horn antennas we find sectoral, pyramidal, bi-conical, and conical types. This first chapter also contains a very readable introduction and enunciation of basic principles.

The second chapter contains an introduction to the electromagnetic theory of microwave antennas. And this is followed, in the third chapter, by a well-written treatment of approximate diffraction theory for various shaped apertures. The remaining chapters examine, in detail, individual types of antennas. These include waveguide radiators, horn antennas, and helical antennas.

Of some mathematical interest is a rigorous solution of the problem exciting a horn antenna by a waveguide. The method is based on a modified form of conformal transformation. Furthermore, using this method, it is not necessary that the axis of the horn coincide with the axis of the guide. The author also draws on exact solutions of a number of other varied boundary valve problems. Some of them are based on the work of the brilliant Soviet mathematical physicist, L. A. Weinstein, who has made great use of factorization methods.

The book is indispensable to anyone seriously interested in the antenna field. It would also be very suitable as a supplementary text in a first year graduate course in fields and waves.

The translator (M. Nadler), with the assistance of the translator-editor (R. C. Glass), has managed to preserve much of the clarity and smoothness of the Russian version. The publisher is

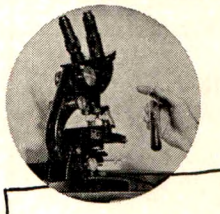
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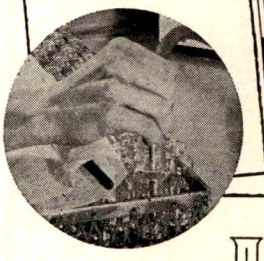
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also to be commended for using the economical photo offset process which also reduces typographical errors, particularly in the equations since these appear in their original form.—*James R. Wait*

The Cell and the Organism (Essays Presented to Sir James Gray), edited by J. A. RAMSAY and V. B. WIGGLESWORTH; 350 pages; $9.50; New York: Cambridge University Press, 1961.

Even more than in the case of symposia, this elegantly printed book is impossible to review in any proper sense because the subjects of the essays extend over an exceptionally wide range: from problems of cell growth to those of animal behavior; from lug worms to sperm energetics, bird flight, and electric fish. Yet largely because of this diversity of contents, the book is interesting and will make profitable reading for all those who wish to see into different aspects of experimental biology.

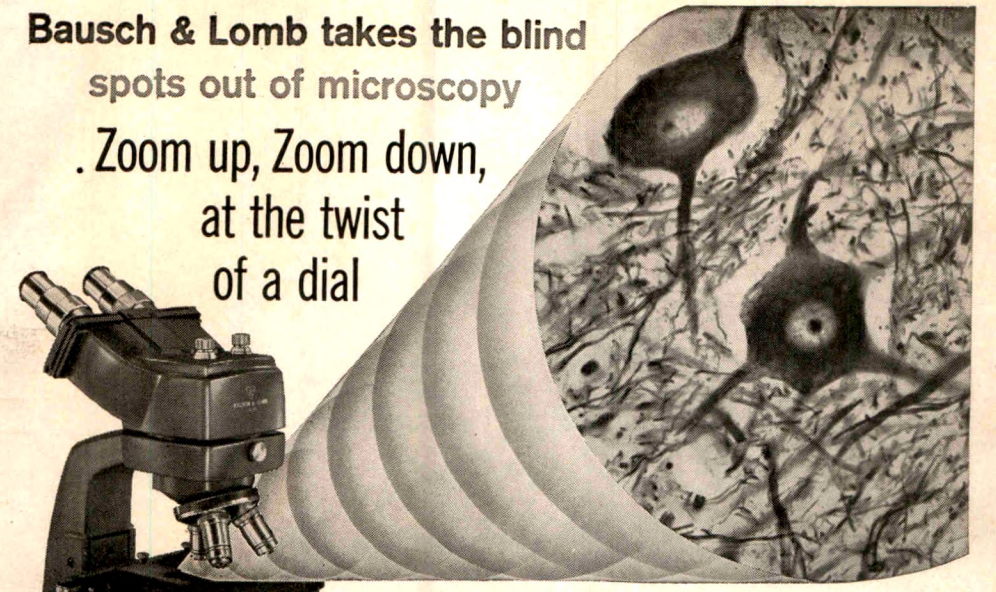
It should be added that the breadth of the articles, as well as their high quality, could not possibly be a more fitting tribute to Professor Gray. His colleagues have managed to put together a book possessing the spirit of his own admirable work which has always been rich in ideas and imagination, rich in insight and understanding, willing to point to the real problems and not just to the outer layers, and always finely and delicately expressed so that he has made it possible for all of us to see his large store of gifts.—*John Tyler Bonner*

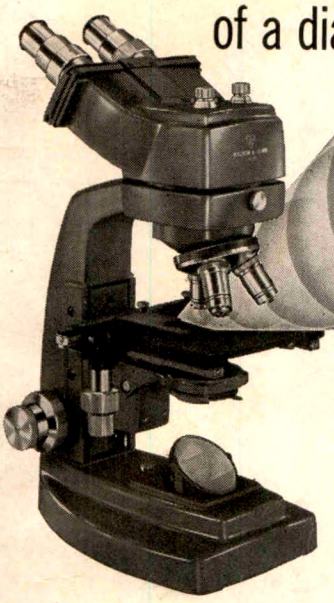
Advances in Morphogenesis, Volume 1, edited by M. ABERCROMBIE and JEAN BRACHET; 445 pages; $12.50; Academic Press, 1961.

This is a new series that should prove to be of considerable interest to all those concerned with problems of development. Perhaps the best way to indicate briefly its approach and its scope is simply to list the chapter titles and the authors. This should be prefaced with the remark that the papers have been meticulously edited

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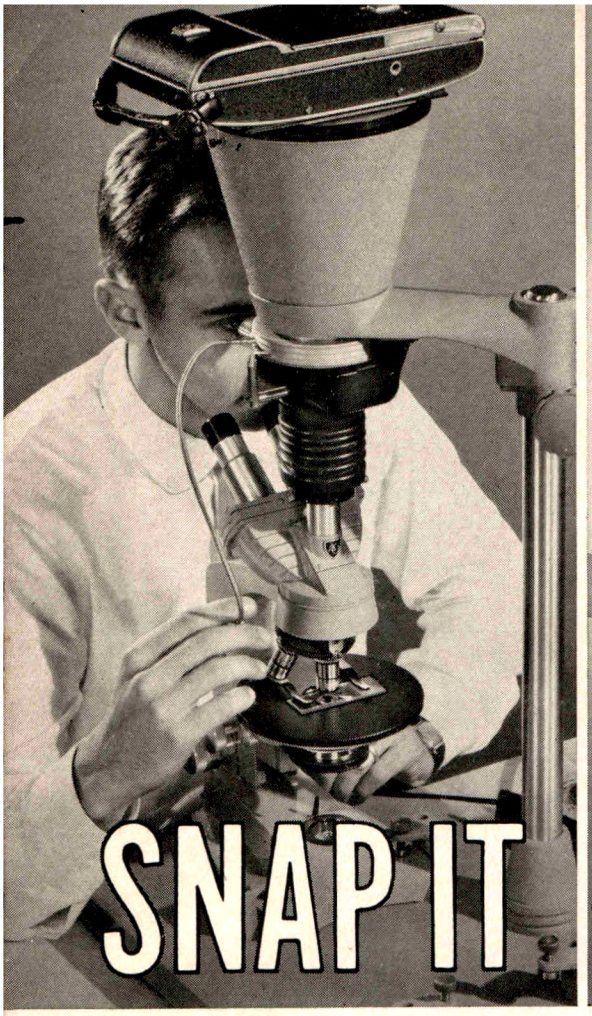
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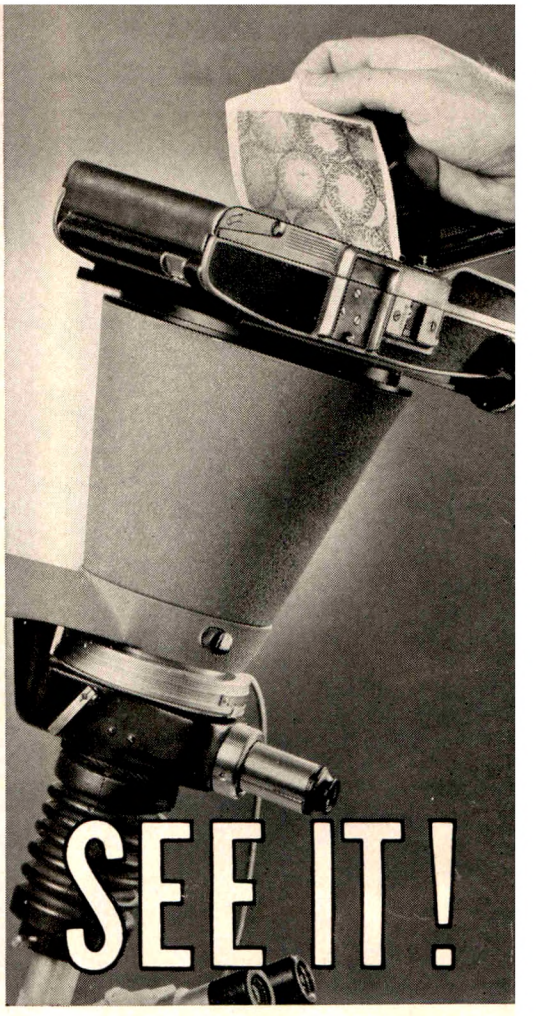
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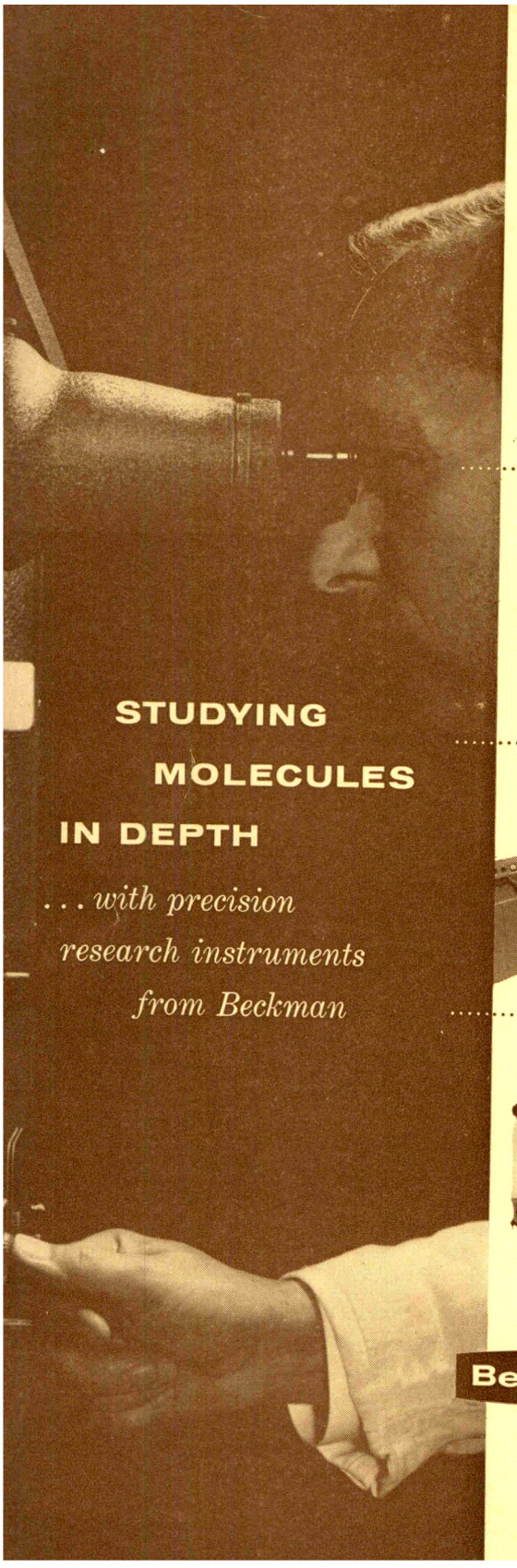
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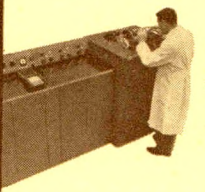
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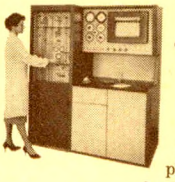
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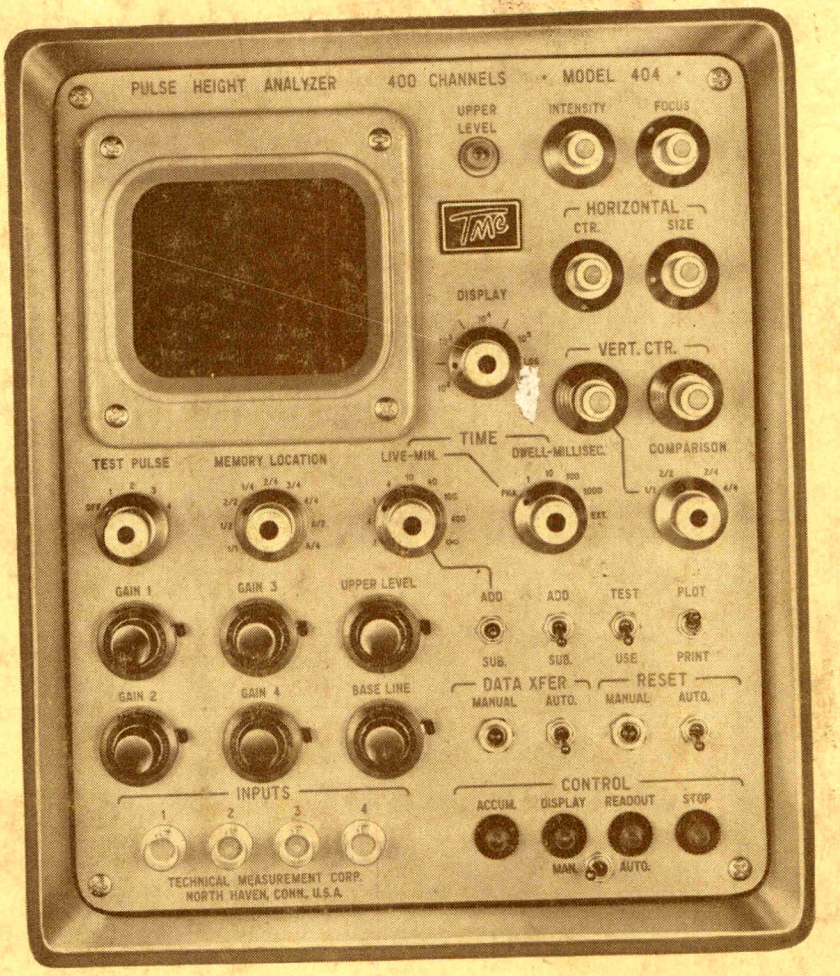
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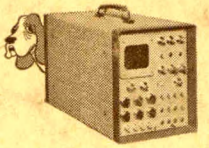
THE SCIENTISTS' BOOKSHELF 62A

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CONTRIBUTORS

WALLACE R. BRODE, *The Growth of Science and a National Science Program* 1

The President of the Society of Sigma Xi here expresses, out of a broad range of experience, his views on the growth of science and the national problems involved. In academic life, as Associate Director of the National Bureau of Standards, and, most recently, as Scientific Adviser to the Secretary of State in the Department of State in the Eisenhower Administration, he brings to his task the points of view of the educator, the research scientist, and the administrator in government. Dr. Brode has also accepted responsibility to the Society as Editor of *Science in Progress* which will probably be issued annually henceforth. Address: 3900 Connecticut Avenue, Washington 8, D. C.

EDWARD R. WEIDLEIN, *Cooperation—A Responsibility of the Scientists* 29

A past-President of RESA and the recipient of the Procter Prize Award for 1961, closely identified with the Mellon Institute from its beginnings in Kansas in 1909, as Director of the Institute in Pittsburgh from 1921 to 1951, and as President until his retirement in 1956, Dr. Weidlein emphasizes the role of such institutes in service both to industry and basic science. Through cooperation, he insists, "the endless frontiers of science now stretching to the stars can provide rich opportunities for the best creative minds." Address: The Kenmawr, 401 Shady Avenue, Pittsburgh 6, Pa.

GEORGE GAYLORD SIMPSON, *The Status of the Study of Organisms* 36

Professor George Gaylord Simpson, formerly of the American Museum of Natural History and now of the Museum of Comparative Zoology at Harvard, is a man of vast talents. He is not only one of the foremost vertebrate paleontologists of our day but one of the foremost biologists as well. A discussion of the nature and the future of biology could not be in more competent hands.

CLIFFORD GROBSTEIN, *Levels and Ontogeny* 46

While there is a general feeling today that developmental biology is one of the most important areas of research, there is some confusion as to what to do about it. One of the handful of biologists who has made really significant advances in this area is Professor Clifford Grobstein of Stanford University. Perhaps one of the reasons for his success has been his broad view of problems of biology which is evident in this article.

SANBORN C. BROWN, *Plasma Physics* 59

The National Lecture Committee of Sigma Xi fully recognizes the potential importance of the physics of plasmas. An article that suggests the potency of controlled thermonuclear reactions as sources of energy for man's peaceful pursuits, this National Lecture presents a broad picture of these potentials. Dr. Brown is Professor of Physics at the Massachusetts Institute of Technology. Last September he was a United States delegate to the International Atomic Energy Agency's Conference on Plasma Physics and Controlled Thermonuclear Reactions in Salzburg, Austria.

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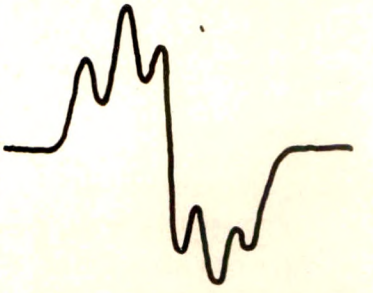
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N. B. HANNAY, *Chemical Equilibria and Reactions in Semiconductors* 75

The physical effects of semiconductors have monopolized the interest of scientists and engineers in the past decade. The Chemical Director of the Bell Telephone Laboratories at Murray Hill, New Jersey, here emphasizes some aspects of chemical equilibrium in such solid systems, showing the analogies that exist between these equilibria and the more familiar ionic equilibria found in aqueous systems and well-known even to the modern freshman chemist.

MARTIN RYLE, *Radio Astronomy and Cosmology* 92

With the permission of the Royal Institution of Great Britain, we reproduce from the Proceedings of the Royal Institution a lecture by the Professor of Radio Astronomy in Cambridge University. The Cambridge interferometer is described and the significance of radio observations to test particular cosmological models, such as the Steady-State Model, is discussed. It is suggested that the Steady-State Theory in its original form does not correspond to the actual Universe.

EUGENE M. SHOEMAKER, *Exploration of the Moon's Surface* 99

Dr. Shoemaker is Chief of the Branch of Astrogeology established as recently as 1961 in the U. S. Geological Survey. After a detailed study of the geology of Meteor Crater in Arizona where his work reinforced the hypothesis that it was due to meteorite impact, he studied craters due to nuclear explosions and to volcanism, craters produced elsewhere on earth probably by meteorite impact, and was finally led to consideration of the origins of the surface features of the moon. In this paper he presents some stimulating new material and ideas based on a geologist's way of dealing with this kind of a problem. Address: U. S. Department of the Interior, Geological Survey, 345 Middlefield Road, Menlo Park, California.

R. G. W. NORRISH, *Kinetic Spectroscopy and Flash Photolysis* 131

Professor of Physical Chemistry at Cambridge University and President for 1961 of Section C (Chemistry) of the British Association for the Advancement of Science, Dr. Norrish presents a summary of his Presidential Address made at Norwich, England, in September 1961 on the technique developed by him and Professor G. Porter on the flash photolysis of a wide variety of chemical systems, and the significant contributions that have resulted therefrom in the direct demonstration of short-lived intermediates in photochemical reactions. Reprinted by permission.

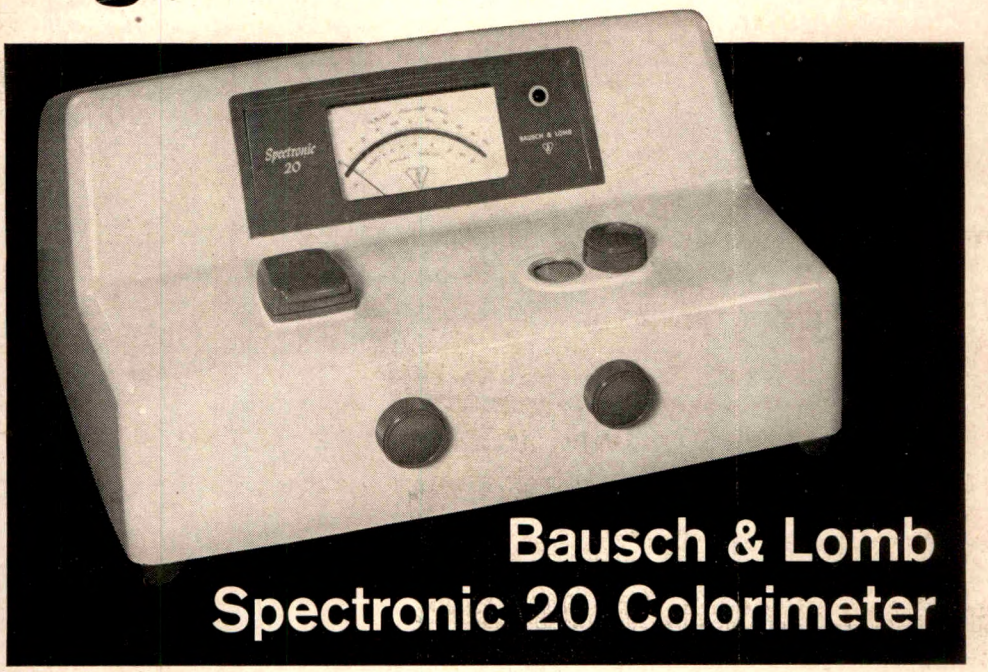
JACQUES R. FRESCO and DAVID B. STRAUS,
Biosynthetic Polynucleotides: Models of Biological Templates 158

Two biochemists in the Frick Chemical Laboratory of Princeton University describe the production of synthetic polynucleotides as models of the naturally occurring nucleic acids, "the master molecules of living systems." They point out that "the code which has been deciphered since the beginning of life by the simplest cell is now intelligible even to *Homo sapiens*" and emphasize also the significance of synthetic products in elucidating these exciting developments.

CLARENCE D. CONE, JR., *Thermal Soaring of Birds* 180

All who have been fascinated by the apparently effortless soaring of certain birds will find one aspect of this problem dealt with thoroughly. Dr. Cone is an aerodynamicist with the National Aeronautics and Space Administration's Research Center at Langley Air Force Base, Virginia. He has been asked to represent the United States at the International Technical Congress of the Organisation Scientifique et Technique Internationale du Vol à Voile in 1963.

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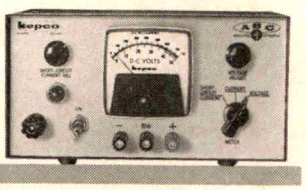
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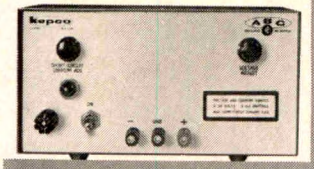
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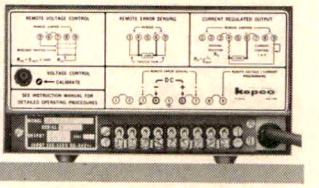


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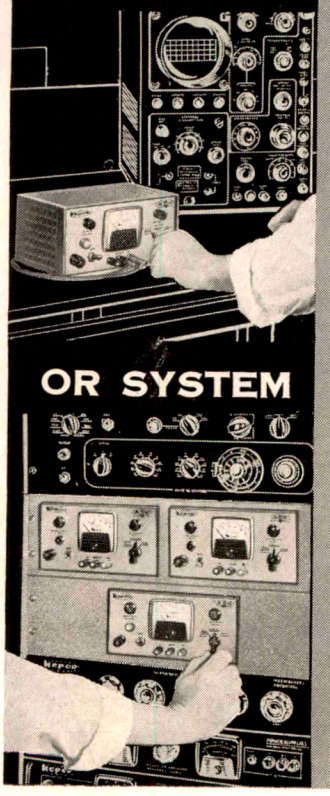


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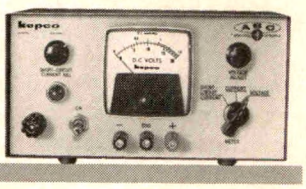
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STEPHEN BRUNAUER, *Tobermorite Gel—The Heart of Concrete* 210

A Hungarian-born scientist who obtained his scientific education the hard way in New York on emigrating to this country after World War I, he served his adopted country for many years in the Fixed Nitrogen Research Laboratory and as a Naval officer and Civilian Director of Research in the U. S. Navy during and after World War II. Decorated for his services by the Navy and by the British, he has gained distinction as a scientist for his researches on *Adsorption of Gases and Vapors*, a title of a classical book of which he is the author. He is now Manager of the Basic Research Section of the Portland Cement Association Research and Development Laboratories at 5420 Old Orchard Road, Skokie, Illinois.

H. M. WORMINGTON, *A Survey of Early American Prehistory* 230

A native of Denver (from indestructible French ancestors), Mrs. George D. Volk, as Curator of Archaeology in the Denver Museum of Natural History, has created pre-eminent educational exhibits of the cultures of prehistoric Amerindians. She has become a foremost educator and author through her museum researches and has scoured many parts of the world for pertinent tools, bones, and data. Her devotion and skill have greatly enriched her multiple-edition book, *Ancient Man in North America*.

SIMON MARCSON, *European Scientists at Work* 243

The author of a recent book, *The Scientist in American Industry*, writes, in a *Perspectives* article, an account of visits to a government scientific research organization in Britain, an industrial research laboratory in Holland, and a semi-governmental research and educational institute in France to ascertain West European attitudes toward the scientific research man. Dr. Marcson is on the staff of Rutgers University, a Research Associate in the Industrial Relations Section of Princeton University, and has served as consultant to industry, government, and the United Nations and in research capacities at numerous universities here and abroad.

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PHILIP L. WALKER, JR., *Carbon—An Old but New Material* 259

This Sigma Xi-RESA National Lecturer for 1961–1962 was educated first at The Johns Hopkins University and received his Ph.D. degree at The Pennsylvania State University in 1952. Now, Chairman of Pennsylvania State's Mineral Technology Division, which includes Metallurgy, Ceramic and Fuel Technologies, Professor Walker's research interests are concerned with the physical and crystallographic properties of carbon and their relation to reaction with oxidizing gases. This lecture brings the reader abreast of the characteristics of an element which has acquired a new significance in the nuclear age as well as enjoying tried characteristics in a variety of technologies. Address: Pennsylvania State University, University Park, Pa.

SALOMON BOCHNER, *The Role of Mathematics in the Rise of Mechanics* 294

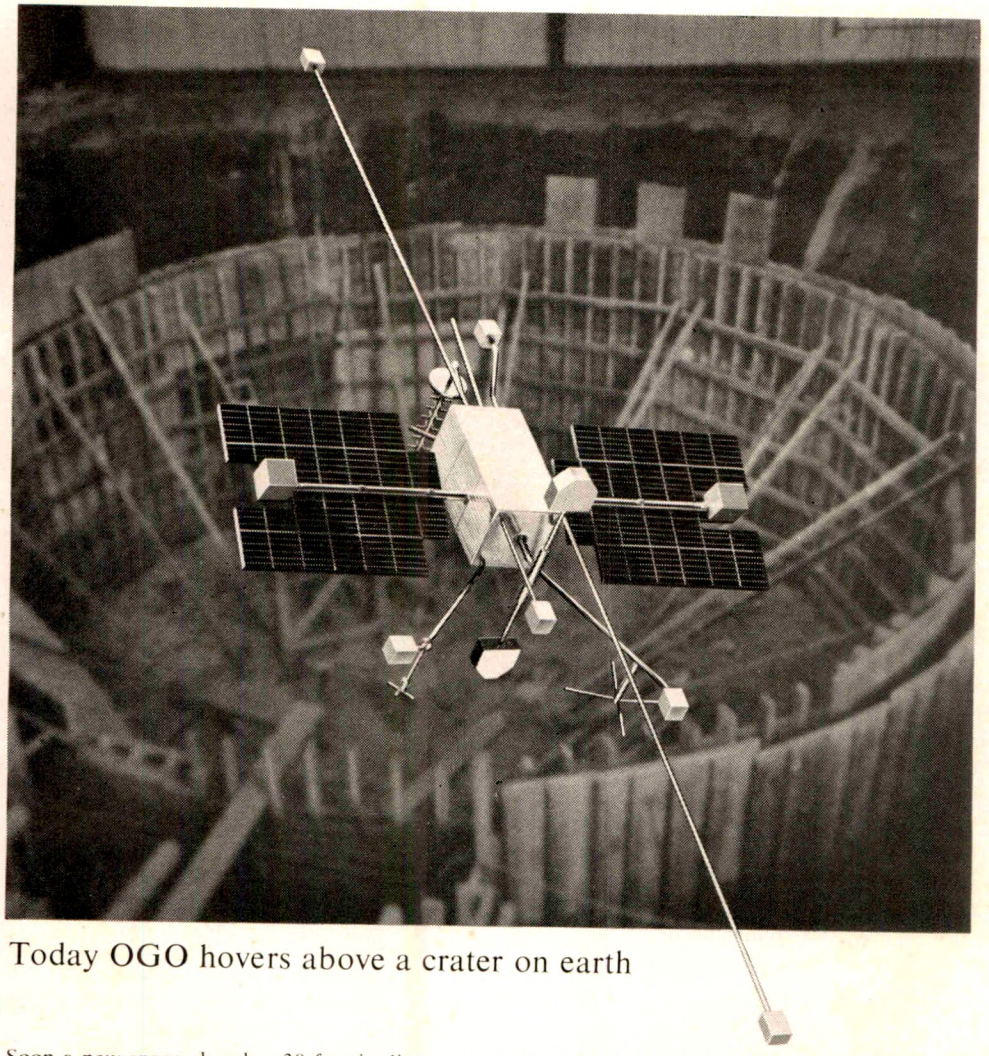
Born in Poland at the turn of the century, Professor Bochner arrived in Princeton, via a Ph.D. in Berlin and a Lectureship in Munich, in the uneasy year of 1933. He has passed right up the academic ladder to become, in 1959, the Henry Burchard Fine Professor of Mathematics. Numerous assignments in Berkeley, Copenhagen, Oxford and Cambridge, and his membership in the National Academy of Sciences are witnesses to his command of probability theory, complex functions, and manifolds. His scholarly recreation has long been to study the reciprocity of physical concept and mathematical expression in historical development. This article is his first publication of those interests. Address: Fine Hall, Princeton University, Princeton, N.J.

ALLEN NUSSBAUM, *Tellurium* 312

The Division Head of the Solid State Division of American Electronics Laboratories, Inc., here reports on a study of the electrical properties of single-crystal tellurium. The work was carried out at the Honeywell Research Center. As a review article it includes some historical material and background on solid state theory to explain the objectives of such a program to non-specialists in the field of semi-conductors. Address: Box 552, Lansdale, Pa.

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IRE Delegates: NYC Interviews March 26–29. Call F. C. Nagel, PLaza 2-8968. Visit STL Booth 1435.

PATRICK L. MCGEER, *Mind, Drugs and Behavior* 322

Now an Associate Professor in the Kinsmen Laboratory of Neurological Research, Department of Psychiatry, University of British Columbia, Vancouver, Canada, the author came to medicine by a devious route. Graduating from Princeton University with a Ph.D. in physical chemistry and some experience in microwave absorption in liquids, he worked with polymers for three years with the DuPont Company before returning in 1954 to his Canadian homeland to begin medicine. His interest in the biochemistry of the brain and its effect on behavior was aroused during his first year in medicine and has since developed into a fascination which he shares in this article.

CYRIL L. COMAR, *Biological Aspects of Nuclear Weapons* 339

As nuclear testing has now been resumed, it is well to have the Professor of Physical Biology at the New York State Veterinary College of Cornell University (Ithaca) give the readers of AMERICAN SCIENTIST a sober appraisal of the biological effects of nuclear explosions through weapons. As Head of the Department of Physical Biology and Director of the Laboratory of Radiation Biology at his Veterinary College, he is a pioneer in research on the passage of fission products through the food chain. He is an expert consultant in all national and international committees dealing with this problem. The author of texts on *Radioisotopes in Biology and Agriculture* and on *Mineral Metabolism*, he was excellently equipped to contribute this lecture to Cornell University's Symposium on *Nuclear Peril and Disarmament* on January 11, 1962.

ARTHUR B. PARDEE, *pU: A New Quantity in Biochemistry* 130A

Our contributor to News and Views is Professor of Biology and Chairman of the newly founded Biochemical Sciences Program at Princeton University. He has written numerous original articles and reviews in the area of enzymology and metabolic regulation.

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NEWS AND VIEWS

By the Board of Editors and the Membership of the Sigma Xi-RESA Societies

THE SCIENTIFIC RESEARCH SOCIETY OF AMERICA: PROCEEDINGS OF THE THIRTEENTH ANNUAL CONVENTION

Denver, Colorado

December 29, 1961

The thirteenth Annual Convention of the Scientific Research Society of America was held in the Terrace Room of the Hilton Hotel, Denver, Colorado, on December 29, 1961. The Chairman, Dr. W. J. Coppoc, called the meeting to order at 10:30 A.M.

The roll call, checked by replies from branch officers received in response to the formal notice of the Convention, showed the following delegates present from six branches and one club:

Boulder: L. A. Matheson, R. B. Scott, and Frank E. E. Germann
Butler-Indianapolis: Donald B. Meyers
Fort Detrick: Charles R. Phillips
Frankford: Abraham L. Korr
Roche Research: Franklin A. Smith
Whirlpool: Everett F. Cox
Colorado Springs Club (to be installed January 18, 1962): Hans Reich and Richard M. Pearl

In addition to these delegates there were present: Wallace R. Brode, Frederick W. Brown, and Thomas T. Holme of the Board of Governors, Frederick D. Rossini, President-elect of Sigma Xi, and Hugh S. Taylor, Editor-in-Chief of AMERICAN SCIENTIST.

At the twelfth Convention in New York City in December 1960, twenty-three branches were represented. Obviously, the location of the meeting strongly affects the attendance. Denver was the choice of the AAAS with which RESA customarily meets. The Convention in 1962 will be held in Philadelphia.

No corrections nor additions having been reported, it was

VOTED: to approve the Minutes of the 1960 Convention, held in New York, December 29, 1960, as circulated to the branches and published in the AMERICAN SCIENTIST for March 1961.

The Chairman reported that he had installed the Ford Motor Company Branch at Dearborn, Michigan, on May 10, 1961, and that the enthusiastic meeting indicated a very successful branch. The Chairman discussed the work of the Special Committee on the objectives and policies of RESA, of which Blaine McKusick of the DuPont Branch is Chairman. This Committee has presented two progress reports to the Board of Governors which have been carefully considered and debated, and it is expected that a final report will be presented at the March meeting of the Board. If any actions by the branches are proposed, suitable recommendations will be presented to the 1962 Convention. The Chairman stated that the 1961 Procter Award would be announced at the luncheon to follow the Convention.

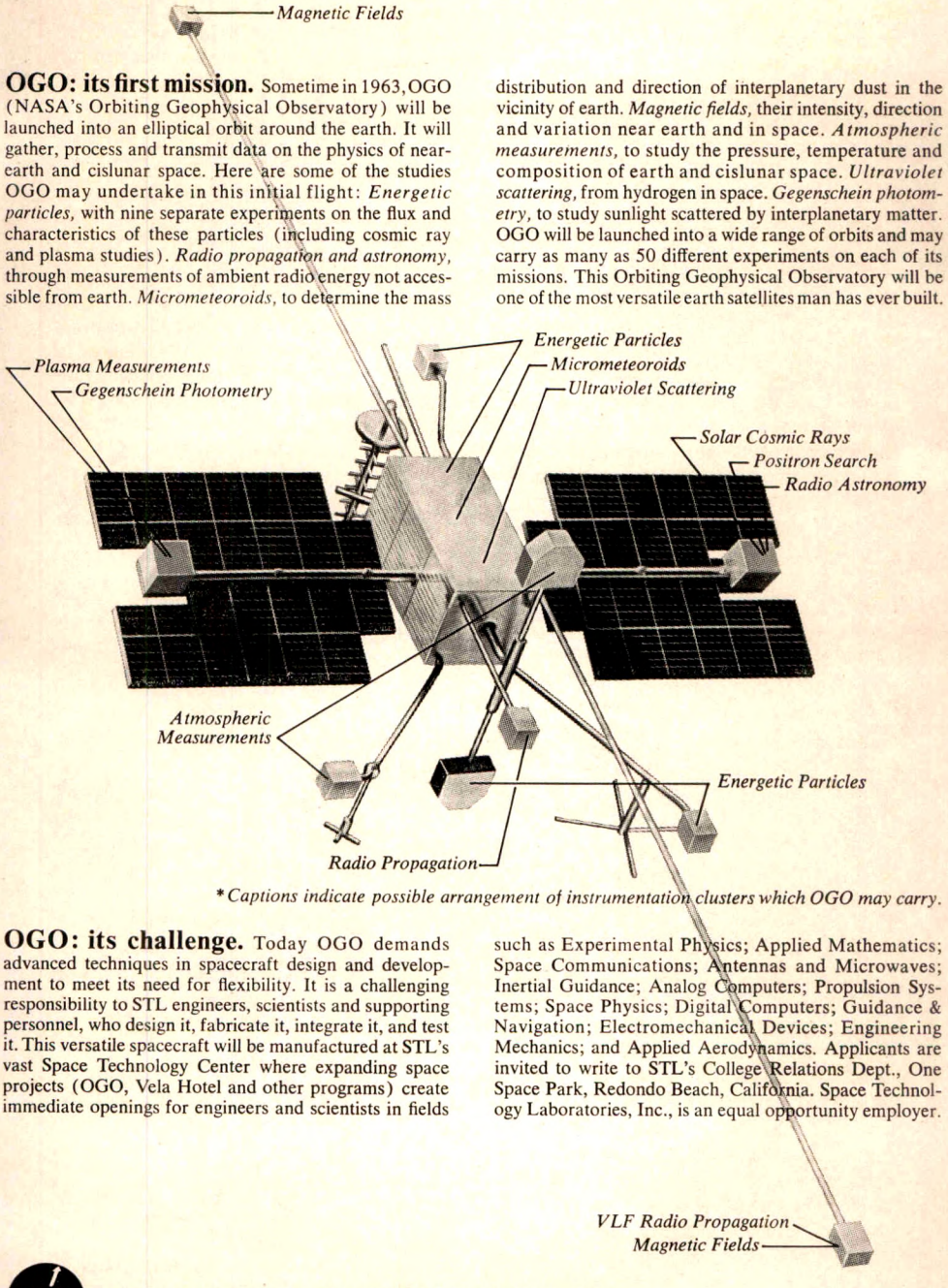
The Director reported the following installations in addition to the Ford Motor Company Branch since the 1960 Convention:

Aerojet-Sacramento on November 29 by Dean Boelter
Atomics International on September 20 by Doctor Starr
Avco on October 17 by Doctor Prentice

Branches have been approved for Owens-Illinois Glass Company at Toledo and United Aircraft at Hartford and a

(*Continued on page 14A*)

OGO: its first mission. Sometime in 1963, OGO (NASA's Orbiting Geophysical Observatory) will be launched into an elliptical orbit around the earth. It will gather, process and transmit data on the physics of near-earth and cislunar space. Here are some of the studies OGO may undertake in this initial flight: *Energetic particles,* with nine separate experiments on the flux and characteristics of these particles (including cosmic ray and plasma studies). *Radio propagation and astronomy,* through measurements of ambient radio energy not accessible from earth. *Micrometeoroids,* to determine the mass distribution and direction of interplanetary dust in the vicinity of earth. *Magnetic fields,* their intensity, direction and variation near earth and in space. *Atmospheric measurements,* to study the pressure, temperature and composition of earth and cislunar space. *Ultraviolet scattering,* from hydrogen in space. *Gegenschein photometry,* to study sunlight scattered by interplanetary matter. OGO will be launched into a wide range of orbits and may carry as many as 50 different experiments on each of its missions. This Orbiting Geophysical Observatory will be one of the most versatile earth satellites man has ever built.



**Captions indicate possible arrangement of instrumentation clusters which OGO may carry.*

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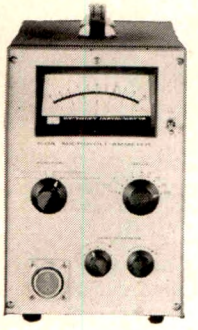
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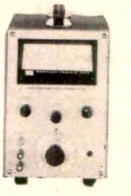


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MODEL	RANGES	ACCURACY	STABILITY	POWER SENSITIVITY
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150A/150AR* Voltmeter	13 overlapping in 1x, 3x steps, 1 μv to 1 v.	2% full scale on all ranges.	After warmup, within 0.1 μv per day.	Greater than 4 x 10^{-18} watt.
Ammeter	15 overlapping in 1x, 3x steps, 10^{-16} to 10^{-3} amp.	3% full scale on all ranges.	After warmup, ± 2 x 10^{-11} ampere per day.	
151/151R* 151RC†	11 linear, 1x, 3x steps, 0.1 mv to 10 v; 5 non-linear, 0.001 to 10 v; three decades each.	Linear $\pm$3% full scale. Non-linear $\pm$10% of input.	After warmup, below 10 μv per day.	10^{-17} watt.

*Models for rack mounting †Rack-Contact meter model, same as standard unit except for addition of a contact meter relay.

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NEWS AND VIEWS

By the Board of Editors and the Membership of the Sigma Xi-RESA Societies

At the March meeting of the Executive Committee of the Society of the Sigma Xi, largely on the initiative of the Committee on Membership-at-Large, consideration was devoted to the general problem of making membership in the Society more meaningful. Especially is this true in the case of members-at-large particularly when these members are not placed near to one of the Sigma Xi chapters. It was felt, too, that the chapters themselves bore a measure of responsibility for ensuring that the best relations were established between the Chapters and the Membership-at-Large. Otherwise expressed, what can Chapters do to create a good "image" of Sigma Xi in their respective communities? One member of the Executive Committee expresses it in the form of a query: Is the image of Sigma Xi in an educational community equal, superior or inferior to that of Phi Beta Kappa? On the answer to that question the actions of the incoming officers of the Chapters and Clubs might well be determined. What can be done to increase, for example, the prestige attaching to the gaining of one of the Sigma Xi-RESA Grants-in-Aid? What can local officers of the National Office do to secure greater publicity for the awards made in these areas? Some $50,000 a year is being expended by the joint societies in such grants. The scale is small compared with the scale of grants of governmental and private foundational programs. But it should be insisted that the grants made in the Sigma Xi-RESA program do occupy an area of support for scientific research that is both necessary and valuable, support which is not forthcoming from most of the alternative programs. As of the first weeks in March, eight grants had been made in December 1961, thirty-seven would be processed during March and there was already a group of seventy applications which were being prepared for the June meeting of the Committee on Grants-in-Aid of Research.

The spring Jubilee Issue of AMERICAN SCIENTIST had a print order of 103,000 copies, the largest in the history of the two Societies. The consumption of paper alone for this issue was in excess of sixty short tons. In the light of these circumstances, the officers of the Society have been considering with Professor W. J. Cunningham, the Business Editor, and with the Advertising Representative, Mrs. William A. James, the advertising rates for the Journal. It seems certain that an upward revision of the rates should be made as of August 1962. It was emphasized that the AMERICAN SCIENTIST ratio of $s per page per 1000 readers, even with the tentative advances in rates, would be approximately one half of those obtaining in journals with similar objectives. It should be remembered, nevertheless, that quarterly journals are at a disadvantage with respect to advertising rates compared with technical journals appearing more frequently.

The Secretary of the Society of Sigma Xi reported for Dr. H. A. Neville, the Director of the Lectureship Program. For the year 1962–63, the program of eight lectures follows.

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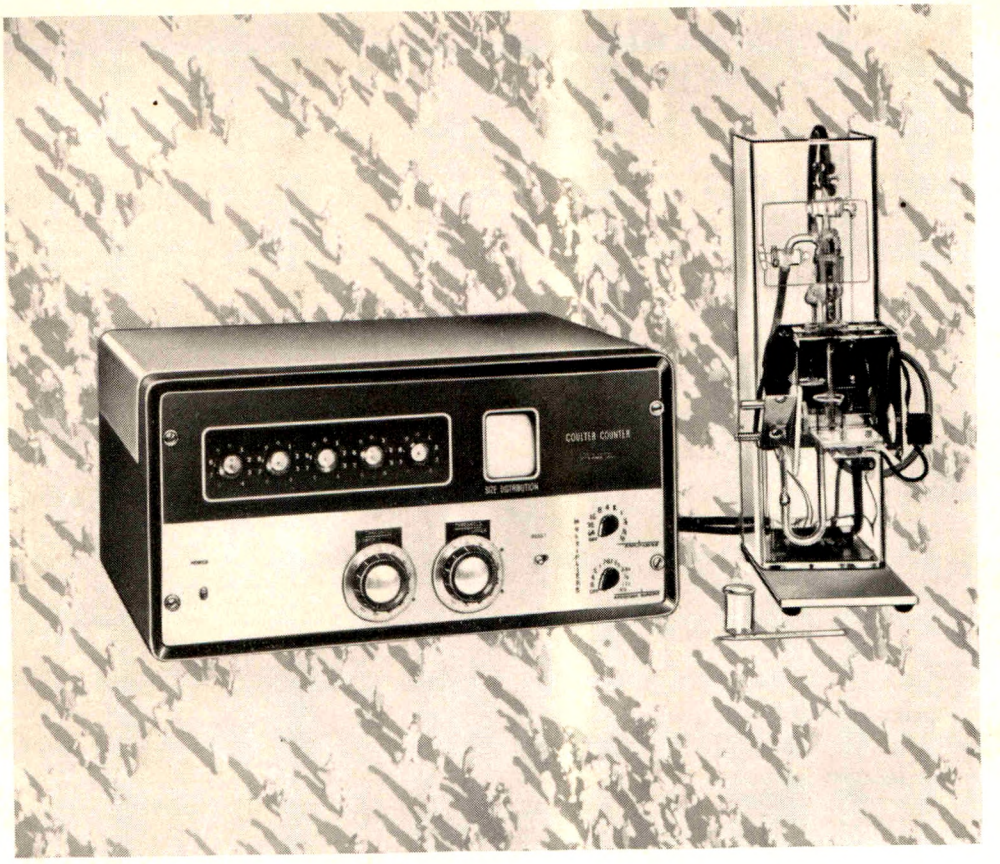
"The Prehistoric Great Lakes of North America"

Dr. Jack L. Hough

of the University of Illinois, Urbana, Ill.

October 1–27, 1962

(*Continued on page 130A*)



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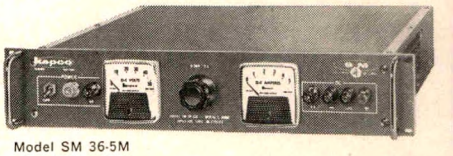
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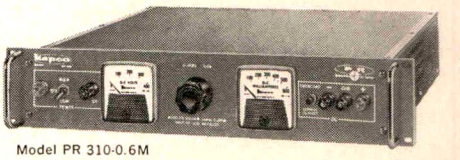
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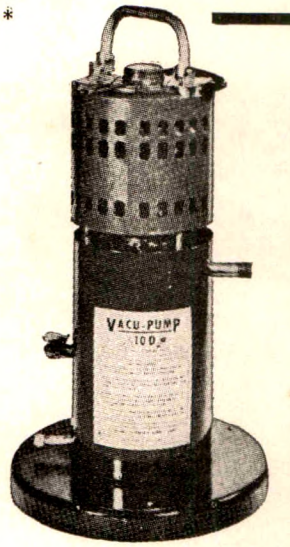
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(*Continued from page 10A*)

club for the Colorado Springs area to be installed by Dr. F. W. Brown on January 18. A petition for an IBM-Poughkeepsie Branch has been received but not yet voted upon. If approved, this will be the seventy-first group. The Director stated that the Board of Governors had held two meetings in 1961, as required by the Constitution, in March and October.

During the year current editions of both RESA booklets have been printed; and a list of lecturers, reported as very satisfactory by branch officers, has been assembled, mimeographed, and mailed to all branches. It is hoped that the latter will be especially helpful to program chairmen. Travel accident insurance has been taken out for speakers on the Sigma Xi-RESA lecture program for a number of years. At a very small additional cost, members of the Boards of RESA and Sigma Xi could be covered also while traveling on Society business, and this is now in effect.

The Director reported, also, that a recent ruling of the Internal Revenue Department requires tax-exempt organizations to provide in their constitutions or by-laws for tax-exempt disposition of their assets in case of termination of operation. The ruling is not retroactive and does not require RESA to comply. (RESA was declared tax-exempt a dozen years ago.) However, the Board of Governors feels that such provision should be made, is studying the question, and will make a suitable recommendation, probably to the 1962 Convention.

The Treasurer reported that payments due for 1962 had been made by all the branches except Houston, that the branch was apparently inactive at present because of a change of employment by an officer and lack of interest on the part of members, and that Doctor Coppoc and he were endeavoring to get the branch reactivated through the Shell Laboratory where the petition for the branch originated. (The Houston Branch was reactivated as of January 24, 1962.)

The financial report for 1961 will be published in the March 1962 issue of AMERICAN SCIENTIST, and it is expected that it will show a favorable balance of income over outgo—a little more perhaps than in 1960. The report will show satisfactory growth in branch membership and membership-at-large and payments of $7000 for the AMERICAN SCIENTIST, some $2300 for Grants-in-Aid of research, and the usual $1000 for the Procter Prize.

The Board of Governors recommends that the assessment per member for 1962 be $2 as at present. It was

VOTED: that the annual assessment per member for 1962 be $2.

The Board of Governors recommends that the initiation and charter fees be continued as at present for 1962. It was

VOTED: that initial individual fees and charter fees be continued as at present for 1962.

The Director explained the great advantage in operation if the Board of Governors could take final action on petitions for group and nominations for individual membership. Interested research scientists who desired to establish a branch at their laboratory would not be required to wait, perhaps many months, until the next Convention could take action as specified in the Constitution. At the same time, the Director stated the Board felt that the Constitution should not be amended but that the members through their Convention delegates should review the situation yearly, thus retaining control of the procedure.

After discussion, it was

VOTED: to continue for one year the grant of authority to the Board of Governors to take final action on group and individual petitions for membership.

The Chairman stated that, at its October meeting held after the RESA Board meeting, the Executive Committee of Sigma Xi voted to recommend to the 1961 Convention that the assessment for 1963 to be voted by the 1962 Convention be raised from the present $2 to $3. The RESA Board took a mail ballot and voted to make a similar recommendation to this Convention. Doctor Coppoc explained that RESA is operating in the black and presumably will not need the extra $1 but that it

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No. S4150—Beckman Model DB Spectrophotometer, for 320 to 770 mu wavelength range, with two silica rectangular cells. 115 volts, 50/60 cycle.....**$1,970.00**

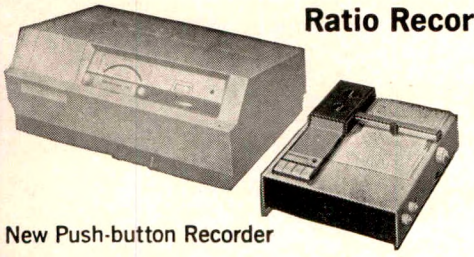
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No. S4180—Wavelength Drive Accessory.....**$ 83.00**

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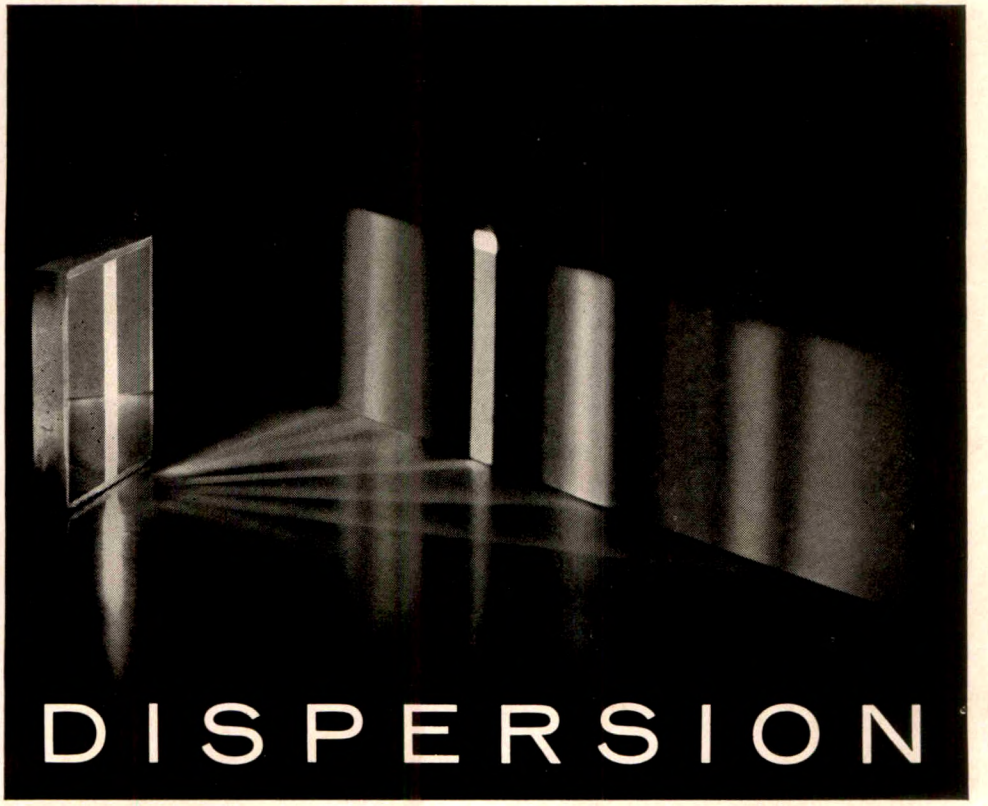
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could and probably should make a larger contribution to the cost of the traveling lecturer program, could increase its contribution to the research fund, and could build up its own operating reserve. There might be an increase in the cost of the AMERICAN SCIENTIST, although a statement from the printer indicated that charges would not increase much in the next two years.

However, in view of the close connection between RESA and Sigma Xi and the fact that members-at-large of the two societies receive identical service, it would be desirable to have the same assessments. After discussion it was

VOTED: that the assessment for 1963 be $3 contingent on similar action being taken by the Sigma Xi Convention. (The Sigma Xi Convention at a later meeting took such action.)

A proposed amendment to the Constitution in regard to the wording of branch charters had been approved by the Board of Governors at its October meeting and submitted to the branches by mail more than the required sixty days before the Convention.

No objections or suggestions had been received from the branches; it was

VOTED: that Section 3 of Article V of the Constitution be amended by substituting the following wording after the sentence "The Charter shall be issued in the following form:

"Be it hereby known that a charter for the establishment of a branch of the Scientific Research Society of America to be known as the ———— Branch is hereby granted in accordance with the provisions of the Constitution of the Society conveying all the powers, privileges and responsibilities to which a branch of the Society is entitled.

"In witness whereof the signatures, etc."

The Director reported that at its October meeting the Board of Governors re-elected Messrs. Coppoc and Prentice to one-year terms beginning July 1, 1962, as Chairman and Director-Treasurer respectively as proposed by the Nominating Committee.

For Mr. J. F. Weiffenbach, Chairman of the Nominating Committee (other members: Messrs. Leo Flexser and W. Hanford), the Director presented the nominations of D. L. Benedict, W. J. Coppoc, and J. W. Copenhaver for three-year terms on the Board of Governors beginning July 1, 1962. Doctor Benedict is a senior scientist at the Stanford Research Institute, and Doctor Copenhaver is Director of Research at the Minnesota Mining and Manufacturing Company. The Chairman asked if there were any nominations from the floor. There being none, on motion it was

VOTED: to elect Messrs. Benedict, Copenhaver, and Coppoc members of the Board of Governors for three-year terms commencing July 1, 1962.

Dr. Hugh S. Taylor, Editor-in-Chief, reported on plans for the AMERICAN SCIENTIST for 1962 including the anniversary issue in March, commemorating the seventy-fifth anniversary of Sigma Xi, the fiftieth anniversary of the AMERICAN SCIENTIST, and the fifteenth anniversary of RESA. Doctor Taylor stated that the Editorial Board always welcomed the opportunity to consider papers by RESA members.

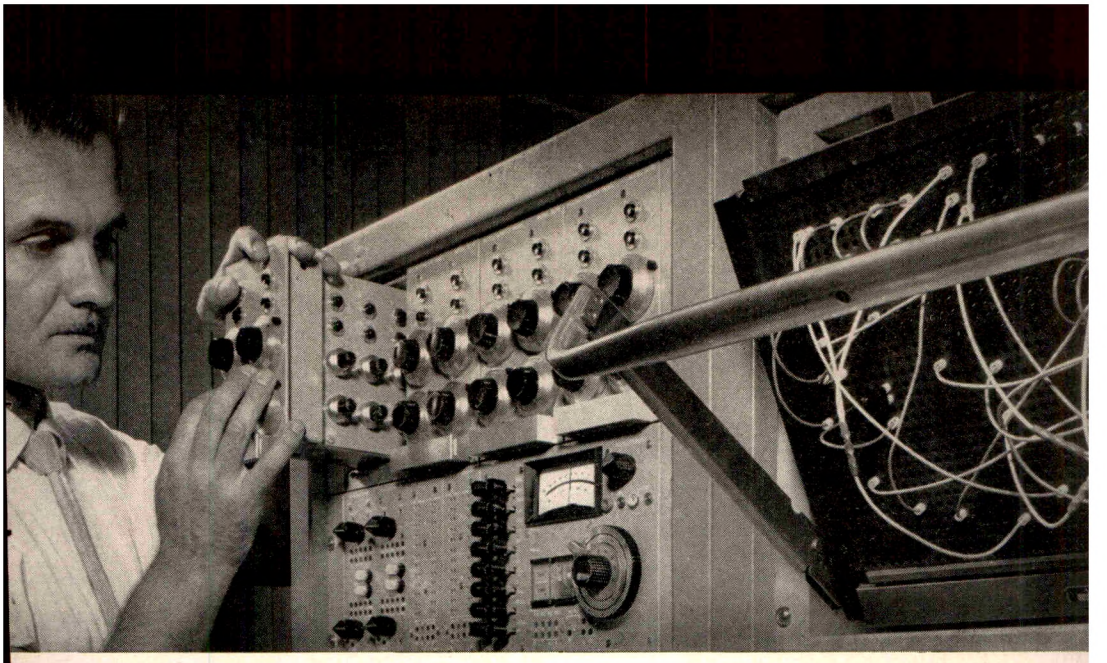
In the absence of Doctor Neville, Chairman of the Committee on Lectureships, the Director reported the plans under which this program operates and stated that the eight lecturers would deliver approximately 160 addresses this season.

For Doctor Shapley, Chairman of the Grants-in-Aid of Research Committee, the Director reported that grants this year would make a record of approximately $50,000, 25 per cent higher than in the previous year. In spite of this increase, requests exceed the available funds and additional or increased contributions will be most welcome.

Dr. W. R. Brode, Editor, discussed the forthcoming volume of "Science in Progress" and stated that it was planned to bring out a volume yearly instead of biennially so that the articles could more nearly present the current status of research in the various fields covered.

Mr. Russell Scott, President of the Boulder Branch, gave a most interesting talk on the activities of his branch. In addition to serving as judges at the high

(*Continued on page 20A*)



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• **NEW AMPLIFIER** Bandwidth, 1 mc; drift, 20 microvolts per day; gain, 10^8; noise, 500 microvolts; output, ± 100 volts @ 20 milliamps.

• **NEW DUAL MULTIPLIER** Solid state 0.05% quarter-square multiplier—squares, divides and extracts square root.

• **NEW FUNCTION GENERATOR WITH STORABLE PROGRAMS** Each module contains 2 independent 12-segment silicon diode function generators.

• **NEW POTENTIOMETER MODULE** Each compact module contains 20 fused precision potentiometers mounted on the smoothest drawer slides we've ever found.

• **NEW CONTROL CENTER** Pushbuttons select these modes of operation: compute, hold, reset, automatic recycle, slave, audible overload indication, and automatic hold. A new reference potentiometer allows null voltage measurements accurate to 0.02%.

OTHER FEATURES The amplifier construction incorporates a built-in jack field on every module which helps the user get a quality computer for minimum cost and later, when he adds the standard removable patch board, he can use the amplifier patching bay as a built-in simulation board. Several sizes of removable patch boards are available both shielded and unshielded.

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(*Continued from page 124A*)

PLAINS TOUR
"Glacier Fluctuations Since the Ice Age in Southeast Alaska"
Dr. Richard P. Goldthwait
of Ohio State University, Columbus Ohio
October 15–November 9, 1962

MID-ATLANTIC TOUR
"Static and Dynamic Buckling of Thin Shells"
Dr. William A. Nash
of The University of Florida, Gainesville, Fla.
October 22–November 17, 1962

CENTRAL TOUR
"Investigation of Crystalline Imperfections and Properties of Materials"
Dr. Sigmund Weissman
of Rutgers, The State University, New Brunswick, N. J.
October 29–November 24, 1962

SOUTHEAST TOUR
"On the Role of Eddies in Fluid Motion"
Dr. Hunter Rouse
of The State University of Iowa, Iowa City, Iowa
February 4–March 2, 1963

PACIFIC TOUR
"The Energy Environment in Which We Live"
Dr. David M. Gates
of The National Bureau of Standards, Boulder, Colo.
March 11–April 6, 1963

SOUTHWEST TOUR
"The Sense of Taste"—Its Sensory and Motivating Properties"
Dr. Carl Pfaffman
of Brown University, Providence, R.I.

MID-WEST TOUR
"Geochronology of Rock Systems"
Dr. John Laurence Kulp
of Lamont Geographical Observatory, Torrey Cliff, Palisades, N.Y.
March 1963

pU: A NEW QUANTITY IN BIOCHEMISTRY

By ARTHUR B. PARDEE

In the course of our observations in Biochemistry (in a broad sense) it appears to us that a new law is emerging. It might be called the Law of Inverse Relationship Between the Uniqueness of an Observation and the Number of Investigators who Report it Simultaneously. (Or, for simplicity and in modesty—Pardee's Law.) Extensive perusal of the scientific literature appearing in journals known to some as "Bio-Quickies" or the "Racing Forum," and also in leading daily newspapers and weekly newsmagazines, reinforced by discussions in learned circles, and finally from rather extensive personal experimentation [1], leads the author to suggest an empirical equation of the form

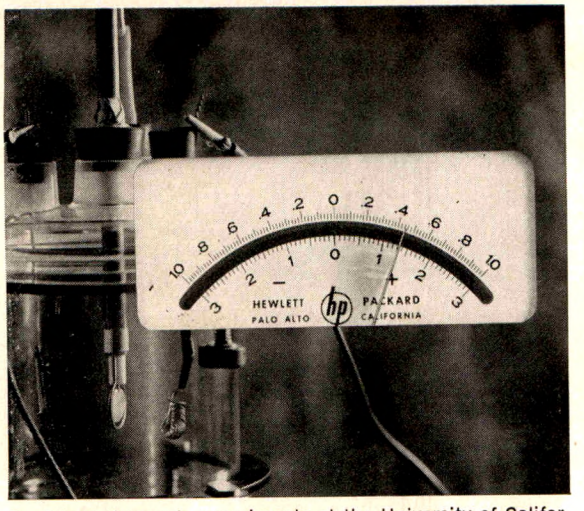
$$pU = N \qquad (1)$$

where pU, equal to $-\log_{10} U$, is defined as the "Uniqueness Coefficient" and serves as a measure of Uniqueness (U) of a contribution. N is the number of other independent investigators who publish the result simultaneously. A logarithmic index of uniqueness, pU, is justified for two reasons: first, it is familiar by virtue of its similarity to pH. Second, it is less likely to wound the pride of the investigator; a pU of 2 is perhaps more easily borne than the flat statement that a contribution published by three authors at the same time is only $^1/_{100}$ as unique as if it were truly unique.

Two subsidiary comments should be appended. Some colleagues feel that the term "simultaneous" is ambiguous. We are developing a more refined equation, of the form

$$pU = Ne^{-qt} \qquad (2)$$

where t is the time, in years, between one's own, first publication and the second. The "priority coefficient," q, is not a constant but rather is some complex function of the interactions between authors and journals—including such factors as publication times, relative incompleteness of the articles, and the number of cocktail parties jointly at-



Dr. Albert Krueger's experiments at the University of California are part of vast research seeking to determine the effect of the air's ion content on man.

You are looking at the measurement of ion level in air . .

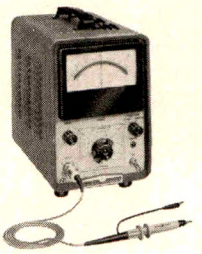
a current measurement of .000000000045 ampere . . .

significant to the health and comfort of Man.

Progress in the physical sciences is dependent on precise measurement of infinitesimally small voltages and currents, electrical energy such as that involved in the measurements of ions in air.

Hewlett-Packard engineers, through revolutionary application of photoconductors in the hp 425A Microvolt-Ammeter, have developed circuits capable of making these minute measurements directly, quickly, easily, accurately. Imaginative techniques overcame traditional characteristics of photoconductors to permit their use as high-speed switches, virtually eliminating the difficulties caused by electrical noise and drift in minute measurement. Overcome are the hazards of contact "bounce," wear, synchronization and noise associated with mechanical switching devices in meter circuits. And the 425A offers unusually long life and high reliability at moderate price.

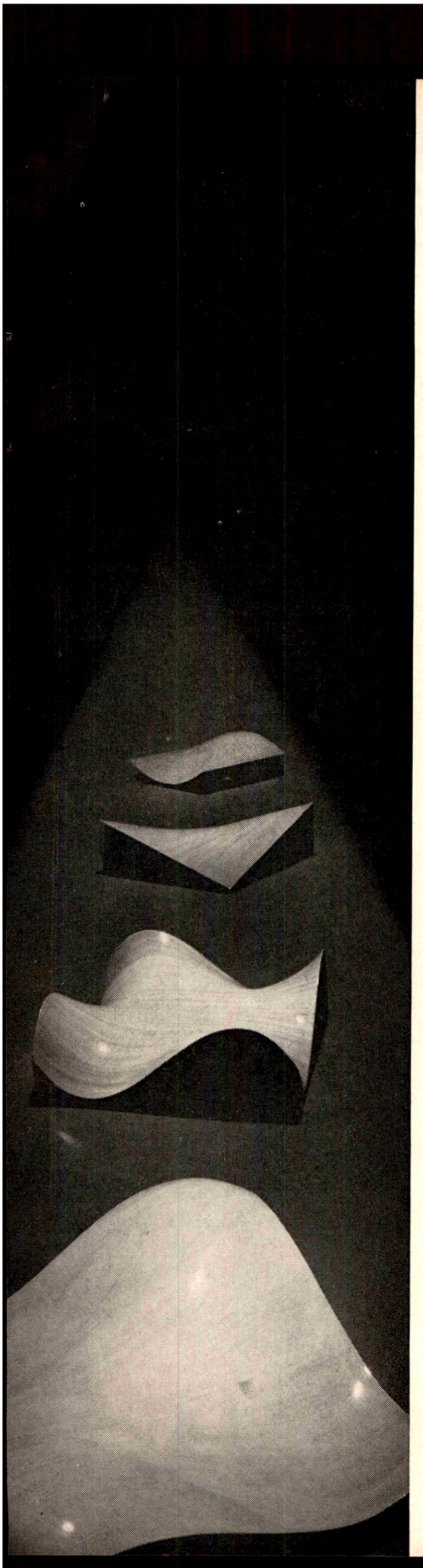
Model 425A is but one of some 400 hp instruments serving the needs of science, industry and the military. It is another example of Hewlett-Packard's continuing effort to engineer into each instrument a genuine contribution to the art of measurement.



hp 425A Microvolt-Ammeter for making these difficult measurements: Ionization levels—grid, photomultiplier currents—thermocouple potentials—voltaic currents in chemicals—minute dc potentials, difference voltages, nulls—voltages in living cells—nerve voltages—voltages in plants, seeds.

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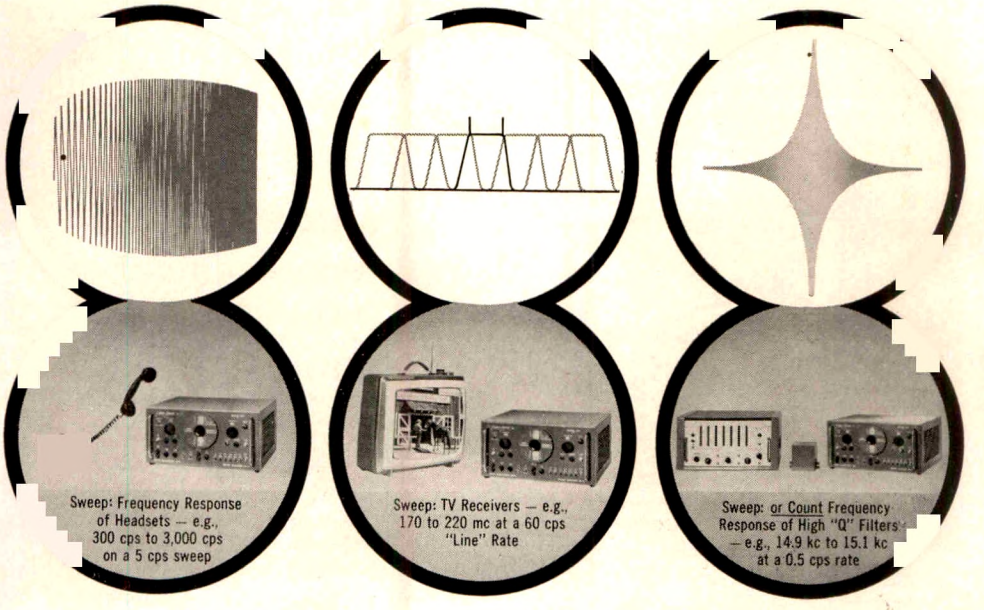
Recently mathematicians at the GM Research Laboratories have been looking at new ways to interpolate in three-dimensional space. As a result they've come up with a fresh approach to the mathematical representation of surfaces. It is called *smooth surface interpolation.*

Their new interpolation formula is the sum of twelve terms. Each term corresponds to a way you can deform an elastic plate by bending or twisting its corners.
(Four of the ways are shown in our illustration.)

To develop a surface, those isolated points in space are first fitted with a crisscross network of curves using a 3-D extension of shipbuilders' spline interpolation. Each rectangular element of the network can then be interpolated with the new formula so it will join smoothly and without a ripple to adjacent elements.
Result: a completely smooth surface.
A continuously differentiable surface.

A distinct contribution to mathematical theory, this work has suggested a host of related techniques *and* potential applications to our new advanced mathematics group. We think it typical of General Motors' constant effort to seek and find—A BETTER WAY.

General Motors Research Laboratories
Warren, Michigan



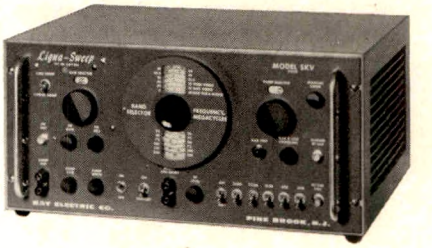
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Check Radar IF's — e.g., 25 to 35 mc at 30 cps


Check Audio Bandpass — e.g., 50 cps to 20 kc at 1 cps Log Sweep

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tended by members of the two laboratories. Exact specification of q is being deferred, awaiting a Grant which will permit purchase of digital and analog computers as well as providing the services of a Staff Psychologist. We are confident that in the full communication, which will shortly follow, this concept will be clarified. A tentative value of q is unity (all else being equal).

pU might be thought identical to the Unimportance Coefficient (pUg). We feel that as N approaches infinity the two coefficients become identical. However, variables enter into the calculations of pUg which can be disregarded with respect to pU.

Another phase of this investigation, on which work is actively in progress, is on the control of pU and qt. Studies on the more or less well understood forces that activate the pressure-to-publish and on the positive-feedback relationship between Grant numbers (G), average Grant size ($\bar{\$}$) and publication rates are under way, and will be pursued as soon as an adequate staff is obtained. We are also considering investigation of the problem of rates of proliferation of Journals and Meetings; these are tentatively believed to depend on the double integral of $\$\overline{G}(pU)dN$, over-all time and space.

REFERENCE

1. Pardee, A. B. (Unpublished work).

LETTERS TO THE EDITORS

The Editors are pleased to report the receipt of a number of favorable letters from home and abroad concerning the Jubilee Issue of American Scientist for March 1962. It would embarrass both writers and editors to publish many such letters. We content ourselves with one or two extracts which raise specific points that may be of interest. Thus far, the Editors have not received any "brickbats" about the issue. They are still expectant and, as said many times before, welcome constructive criticism.

I was very much pleased to receive the anniversary number of the American Scientist. I am sure that all of the readers of this fine publication will be delighted with this excellent anniversary issue. The Editor-in-Chief and his able assistants deserve the hearty congratulations and thanks of the membership of Sigma Xi for their enormous efforts that have gone into this work. I know that many of us are inclined to receive our Journal without much consideration of the problems associated with its publication. The prestige of the Society has undoubtedly increased greatly in recent years because of the improved quality of the American Scientist.

I liked very much the Jubilee Issue of American Scientist. In keeping scientists and engineers informed in fields outside their own specialities, your publication does an excellent job, together with other magazines of a different style like *Scientific American* (which I also greatly enjoy). Noting in the report of the Editor-in-Chief that either a reduction in the size of the publication or an increase in dues will be necessary, I for one would much prefer an increase in dues in order to preserve the quality of American Scientist.

I was impressed by the report on the number of books you received and the number of reviews you were able to print. I gained needed new respect for your space problem and shall make an extra effort to write concise reviews and certainly stay below the three hundred word limit.

ERRATA

To keep the Editors humble, errors crept into the Jubilee Issue as often before. Dr. H. M. Wormington's note explains what happened to her article, *A Survey of Early American Prehistory:*

This manuscript was completed during the period immediately following the fire at the Denver Museum of Natural History, which severely damaged the wing occupied in part by the Department of Archaeology. In the confusion, a page from a rough preliminary draft was substituted for one destined for the final version. Paragraph 4, Page 239 should read as follows:

In the eastern United States there are many complexes that are commonly referred to as an "Archaic" stage of development. Although there is very little stratigraphic evidence, it is generally assumed that the first developmental stage was characterized by fluted points, and that the next stage was the Archaic, with some unfluted lanceolate forms and many notched and stemmed points. The early part of the Archaic was characterized by tools produced by flaking and by implements and utensils shaped by use rather than design. In the late Archaic, techniques of grinding and polishing, as well as flaking, were used. Throughout there was an emphasis on wood-working tools and those used in the preparation of vegetal foods. There was a dependence on a greater variety of game

(*Continued on page 138A*)

(*Continued from page 16A*)

school science fair and providing prizes and a science award, members of the branch sponsor a troop of Explorer Scouts who are especially interested in science, attending weekly meetings of the troop, providing science programs, and taking the boys on plant and laboratory visits.

The Convention was greatly indebted to Mr. Scott for this valuable and suggestive report.

It was announced that the 1962 Convention would be held in Philadelphia at the time of the AAAS sessions.

The Director reported that, in connection with the presentation of the petition for a charter from United Aircraft, RESA had been investigated by an agency of the Federal Government and pronounced non-subversive.

There being no New Business the Convention adjourned at 12:30 for a joint luncheon with Sigma Xi delegates, who were to meet in convention at 2:00 P.M. Attendance at the luncheon was 128 and reached nearly 150 for the annual RESA address on "Cooperation—A Responsibility of the Scientist," by Dr. Edward R. Weidlein, formerly Director of the Mellon Institute.

The 1961 Procter Award was presented by Doctor Coppoc on behalf of the Society to Doctor Weidlein.

DONALD B. PRENTICE
Director

THE SOCIETY OF THE SIGMA XI: PROCEEDINGS OF THE SIXTY-SECOND ANNUAL CONVENTION
Denver, Colorado—December 29, 1961

The Sixty-second Annual Convention of The Society of the Sigma Xi was held at the Denver Hilton Hotel in Denver, Colorado, on December 29, 1961, in accordance with the official announcement mailed to all chapters and clubs on October 27, 1961.

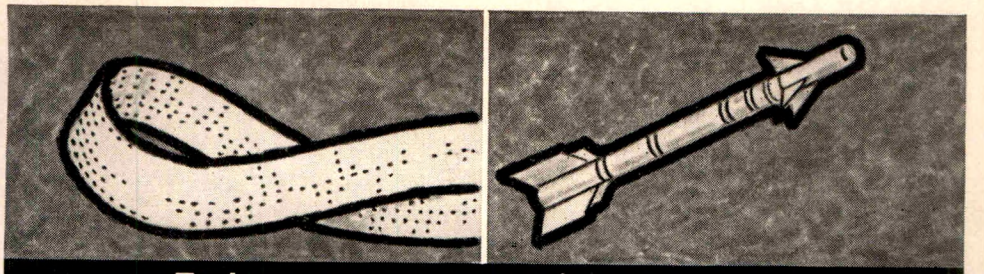
Call to Order

The Convention was called to order at 2:15 P.M., by the President, Dr. Wallace R. Brode, who appointed the following Committee on Credentials:

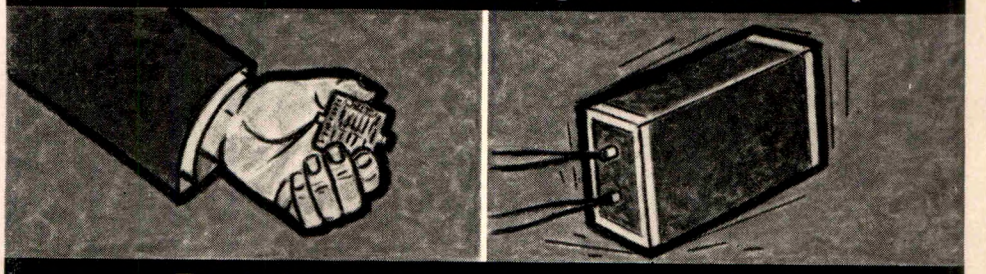
Dr. Richard Bradfield, Cornell University
Dr. Harriet L. Frush, District of Columbia
Dr. Donal D. Johnson, Colorado State University, Chairman

The Committee examined the credentials and, later, the Chairman, Dr. Donal D. Johnson, reported to the Convention that 61 delegates representing 52 chapters, and 31 delegates representing 25 clubs, and 1 delegate representing the Membership-at-Large, were in attendance as follows:

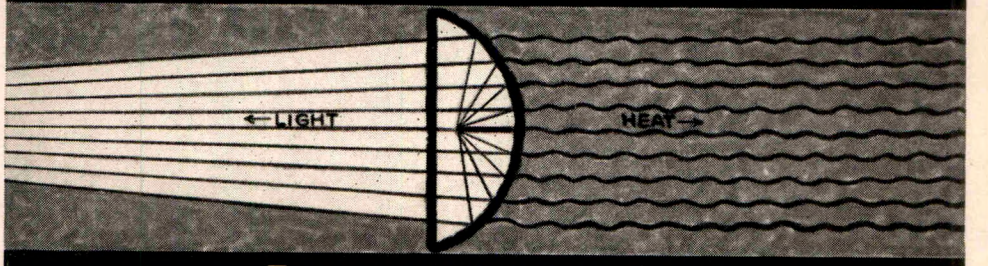
Chapter Delegates: Arkansas, University of—Perry Max Johnston; Auburn University—Reid David; Brigham Young University—John H. Gardner; Bryn Mawr College—John R. Pruett; California Institute of Technology—John H. Maxson; California, University of (Berkeley)—Richard M. Eakin; California, University of (Davis)—Milton Hildebrand; Colorado, University of—Warren Longley; Colorado State University—Robert P. Martin, Donal D. Johnson, Walter R. Benson; Columbia University (Kappa)—Theodosius Dobzhansky; Cornell University—Richard Bradfield; District of Columbia—Harriet L. Frush; Florida, University of—E. Ruffin Jones, Jr.; Florida State University—Charles B. Metz; Fordham University—Douglass J. Hennessy; Indiana University—Henry W. Hofstetter; Iowa, State University of—Eugene Spaziani, Chesley J. Posey; Kansas, University of—Parke H. Woodard, Dennis R. Dahl, Nancy Ann Dahl; Kansas State University—Robert Robel; Kentucky, University of—Morris Scherago; Mayo Foundation—Wendell M. Swenson; Michigan, University of—W. S. Benninghoff; Michigan State University—David T. Clark; Minnesota, University of—Marion A. Brooks; Missouri, University of (Columbia)—Arthur W. Merrick; Missouri, University of (Mines and Metallurgy)—Donald L. Frizzell; New Hampshire, University of—George Moore; New Mexico State University—Marlowe G. Anderson, James E. Weiss; North Carolina State College—Stanley G. Stephens; North Dakota, University



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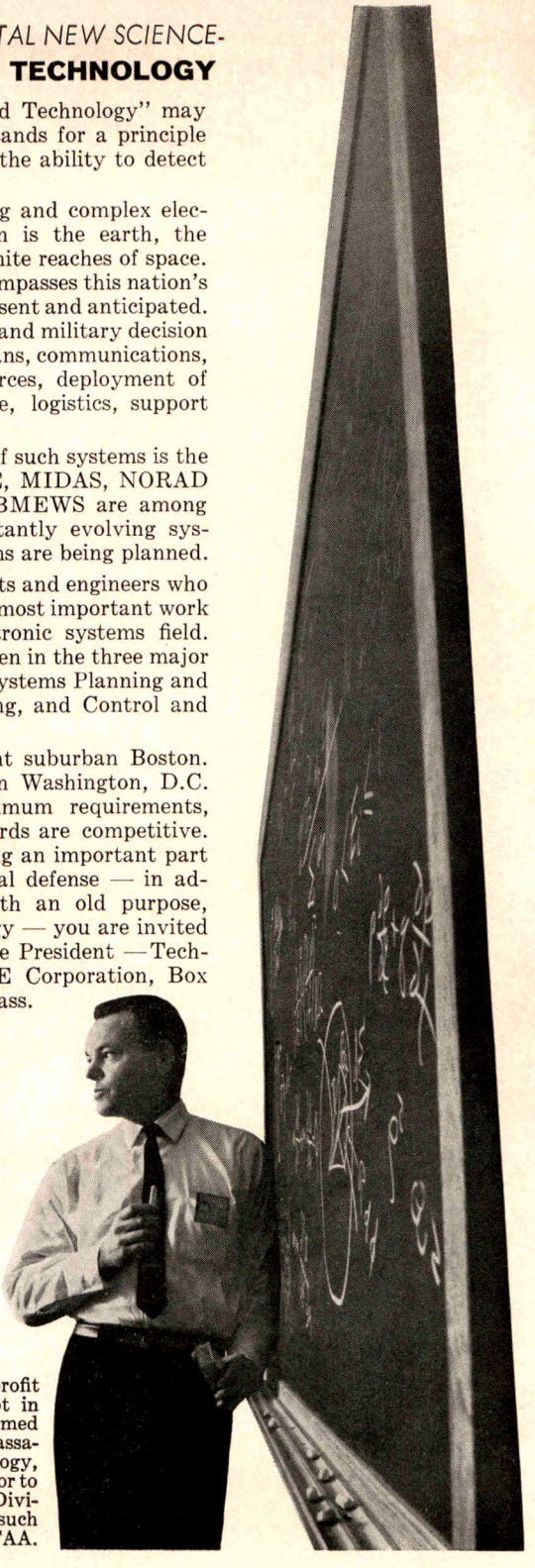
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of—David Williams; Notre Dame, University of—R. C. Gutschick; Ohio State University—Frank C. Croxton; Oklahoma, University of—J. Teague Self; Ohio University—William G. Gambill, Jr.; Pennsylvania State University—Howard B. Sprague; Rice University—Roy V. Talmage; Smith College—Amelia J. Polnik; South Dakota State College—C. Wendell Carlson; South Dakota, State University of—Charles Vaughn, Roger T. Davis; Southern California, University of—Glenn Hamor; Syracuse University—Paul M. Ruoff; Tennessee, University of (Medical Units)—Clinton L. Baker; Texas, University of (Austin)—William H. P. Emery; Texas, University of (Medical Branch-Galveston)—John G. Sinclair; Texas A. & M., College of—Wayne C. McCully; Texas Technological College—Earl Camp; Tulane University—Karlem Riess; Utah State University—Herman H. Wiebe; Virginia, University of—Frank W. Finger; Washington, University of—Allan J. Kohn; Washington, State University of—Herber Eastlick; Wyoming, University of—Kenneth L. Diem, E. R. Schierz, John Hitchcock.

Club Delegates: Arizona State University—Wallace R. Brode; California, University of (Riverside)—H. Homer Aschmann; Chicago Medical School—Ben B. Blivaiss; Colorado School of Mines—Paul F. Bartunek, John S. Rinehart; Creighton University—Don C. Fitzgerald; Downstate Medical Center (State University of New York)—Leonard Procita; DePauw-Wabash—Willis Johnson; Houston, University of—John F. Oro; Idaho State College—Albert E. Taylor, Allen D. Linder, Christina M. Richards; Indiana University-Medical Center—Joseph S. Ingraham; Manhattan College—Brother Conrad William; Miami, University of—L. C. Gilman; Michigan College of Mining and Tech.—Robert T. Brown, Ira H. Horton; Mississippi, University of (Medical Center)—William C. Holland; Montana State University—Ludvig G. Browman, Robert S. Hoffman; New Mexico Highlands University—Lora M. Shields, Robert C. Tripp; North Dakota State University of (Agriculture & Applied Science)—K. L. Lebsock; Ohio Wesleyan University—William D. Stull; Rollins College—Dan A. Thomas; St. John's University—Daniel M. Lilly; San Jose State College—Kenneth E. Hutton; South Dakota School of Mines—George Rapp; Southern Illinois University—Howard G. Applegate; Texas Western College—A. H. Berkman; Wooster, Ohio—Donald L. Wise.

Membership-at-Large Delegate: C. M. Jansky, Jr.

Approval of Proceedings

The Proceedings of the Sixty-first Annual Convention held in New York City on December 29, 1960, were approved as published in the March 1961 issue of AMERICAN SCIENTIST and distributed to the chapters and clubs.

Report of the President

President Brode reported the following installations for 1961:

Chapters: Ohio University—May 18, 1961, Dr. Wallace R. Brode; South Dakota State College—May 30, 1961, Dr. Wallace R. Brode.

Clubs: Indiana University-Medical Center—January 1, 1961; Texas Christian University—January 6, 1961—Dr. Donald Wiggans; Louisiana State University in New Orleans—January 11, 1961—Dr. Clayton C. Brunso; University of Puerto Rico—January 31, 1961—Dr. Truman P. Kohman; U.S. Naval Radiological Defense Lab.—March 10, 1961—Dr. Nello Pace; Connecticut College—April 18, 1961—Dr. W. Jack Cunningham; Florida Presbyterian College—May 11, 1961—Dr. Harry H. Sisler; Jefferson Medical College of Phila.—May 23, 1961—Dr. Harold S. Ginsburg; Lafayette College—May 25, 1961—Dr. Harvey A. Neville.

Dr. Brode also announced anniversaries of chapters at the following institutions:

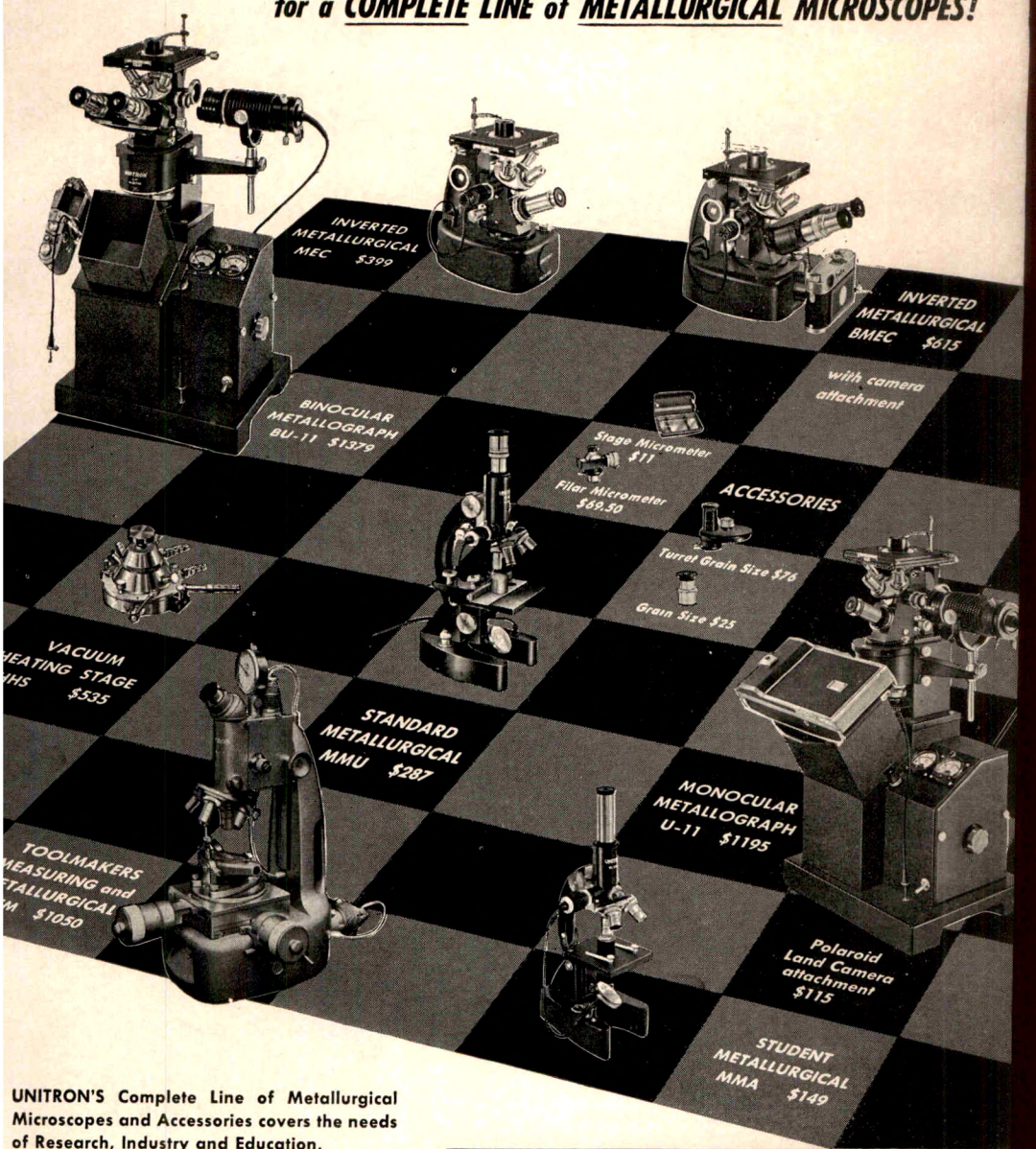
25th Anniversaries in 1961: Carleton College, University of Buffalo

25th Anniversaries in 1962: George Washington University, University of Utah, Carnegie Institute of Technology, Oregon State University

(*Continued on page 26A*)

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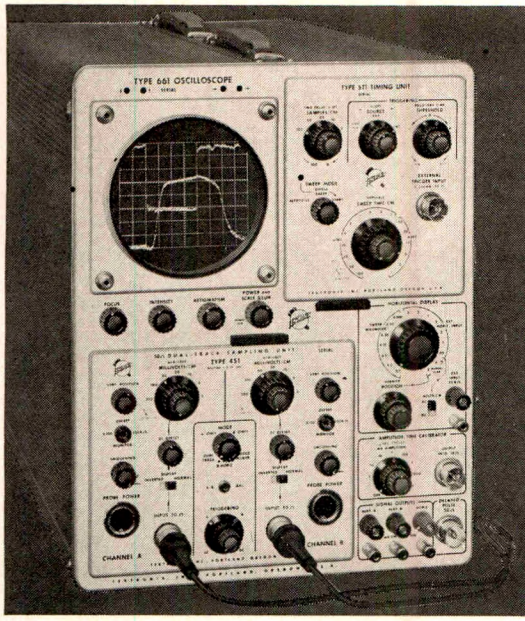
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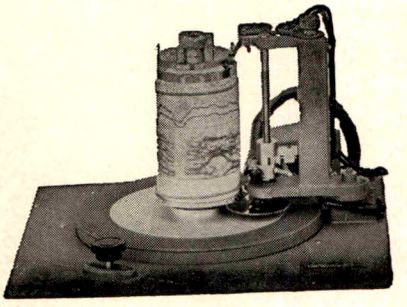
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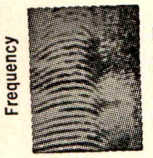
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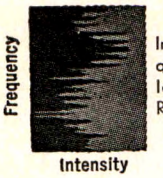
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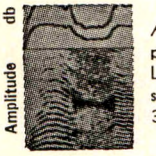
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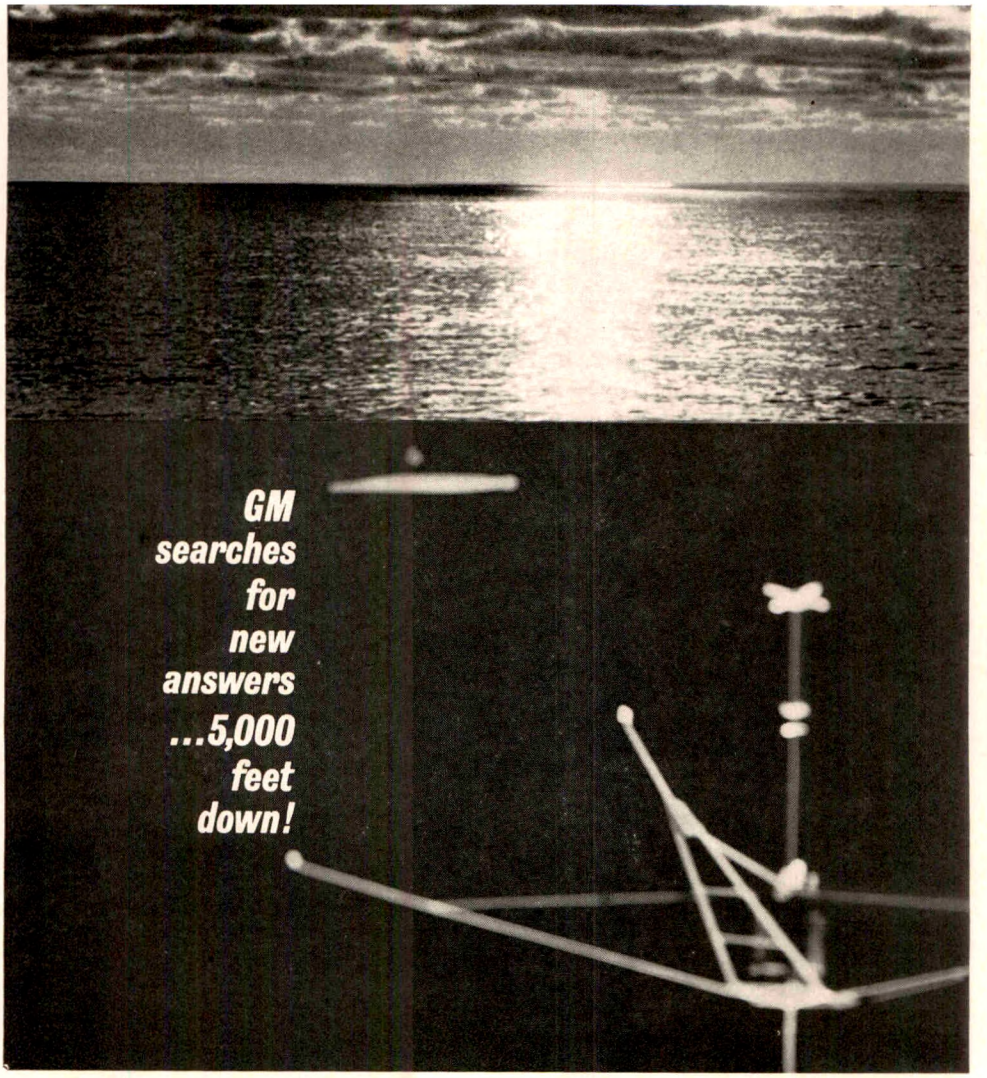
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(*Continued from page 132A*)

including small forms, and a greatly increased emphasis on the gathering of wild plant foods. Some groups made an extensive use of shellfish. Similarities between Archaic lithic assemblages and those of the gatherers and hunters of the Desert Culture suggest a possible relationship of undetermined nature. Radiocarbon dates indicate Archaic occupation as early as 7000 or 8000 B.C. in Alabama, Illinois, and Missouri.

We apologize to another contributor, Dr. Jacques R. Fresco and the American Heart Association.

A dagger in the authors' line "By Jacques R. Fresco and David B. Straus" (page 158) got misplaced in the transition from page proof to published issue, suggesting that Dr. Straus, rather than Dr. Fresco, was the "Established Investigator of The American Heart Association." This necessitated a letter of apology to Dr. Fresco and to the Heart Association which has contributed so generously to the support of Dr. Fresco's work.

We also greatly regret that the name of Professor Carl Bowin of the Woods Hole Oceanographic Institution appeared as Carl Brown in his book review on page 68A.

(*Continued on page 144A*)

REGISTER OF SCIENTISTS INTERESTED IN OVERSEAS ASSIGNMENTS

American participation in the educational and economic development of other countries is likely to continue on an increasing scale during the next few years. In anticipation of the growing need for personnel under international programs in which it cooperates, the National Academy of Sciences-National Research Council is compiling a register of American scientists and other specialists who are interested in the possibilities of assignments abroad for periods ranging from several weeks to two years.

Assignments become available irregularly throughout the year, and they vary greatly with respect to location, duration, stipends, and responsibilities. Some programs sponsored by private foundations require scientists of high competence and reputation for short-term lecturing and consultative duties in a single country or in several countries. A number of government-sponsored projects call for specialists who are available for two-year periods, have had previous experience in certain geographical areas, and are fluent in Spanish or French. Also, under the exchange program authorized by the Fulbright and Smith-Mundt Acts, younger as well as established science educators are welcomed as lecturers at many African, Asian, and Latin American colleges and universities.

Persons who wish to be considered for any of such assignments are asked to fill out a special form, available upon request from the *Committee on International Exchange of Persons*, 2101 Constitution Avenue N.W., Washington 25, D.C. Completion and return of the form will not constitute an application, but it would ensure a person's consideration for openings in his field.

The Register is intended specifically for specialists in the biological and physical sciences and related technologies. However, the Committee will be happy to receive inquiries from persons in other fields.

(*Continued from page 22A*)

It was *VOTED* that:

Those chapters observing 25th Anniversaries in 1962 be extended the congratulations of the Society.

President Brode stated that, during 1961, he had been able to visit a great number of chapters and clubs and he hoped that, during 1962, he would be able to increase that total considerably before his term of office expired in December.

Report of the Executive Secretary

Secretary Holme reported that 1961 had been a most successful year of operation for the Society. The activities of the AMERICAN SCIENTIST, National Lectureships, and Grants-in-Aid of Research had all been carried on most successfully. The one activity that the Secretary reported as needing improvement was that of processing address changes for the AMERICAN SCIENTIST. By reason of a physical relocation of National Headquarters and a major changeover in record keeping during the latter half of 1960, a considerable backlog was accumulated which had to be cleared up during 1961. Of course, the number of changes and additions (more than 20,000) had to be processed at the same time. This work load coupled together with personnel changes at National Headquarters had made 1961 a rather difficult year from the standpoint of the clerical operations. As of December 15, however, when the IBM chapter and club membership lists were mailed out at National Headquarters, work was "on schedule."

In commenting on the problems of address changes, Editor-in-Chief Hugh S. Taylor, of the AMERICAN SCIENTIST, called attention to a new blank form being printed in the December issue of the AMERICAN SCIENTIST for the first time. Dr. Taylor stated that it was hoped that this innovation would help facilitate address changes but, already, the Editorial Office in Princeton had received a number of these notices although the notice clearly stated that it was to be mailed to the Office of the Executive Secretary in New Haven, Connecticut.

Secretary Holme reported that the Executive Committee had approved changes and additions to the National Headquarters office which should enable it to function adequately under the increased load resulting from new chapters, new clubs, and expanded programs of National Lectureships and Grants-in-Aid of Research.

Report of the Treasurer

Dr. Donald B. Prentice, the National Treasurer, reported that the Society was in excellent financial condition and should have a comfortable balance at the end of 1961. At the moment all chapters and all but three clubs had paid their 1961 assessments in full. The Mount Sinai Sigma Xi Club appeared to be completely inactive as no assessments or records of any kind had been received for the current year despite repeated correspondence. Also, the Sigma Xi Clubs at Bucknell University and Lafayette College were in arrears on payment of assessments, and one chapter at Convention time, Georgia Institute of Technology, still owed fees for its June initiates.

Dr. Prentice stated that both income and expense had exceeded the budget for 1961 but, when the final audit was completed, he felt certain that the comfortable balance of income would permit an allotment to the Research Fund as well as an increase to the Reserve.

The Treasurer then presented recommendations of the National Executive Committee for Convention action as follows:

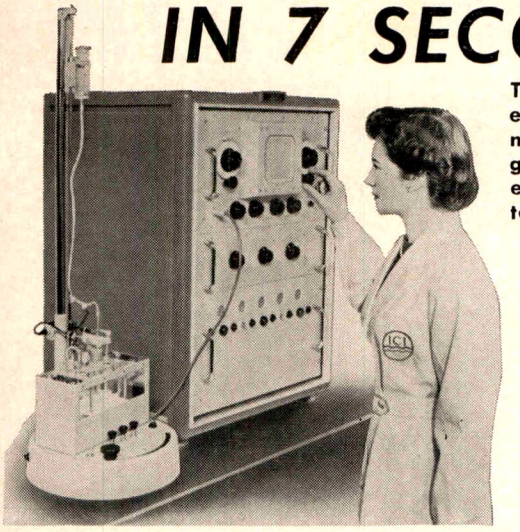
It was *VOTED* that:

1. The annual assessment on each chapter shall be due and payable on January 1, 1962, and that the amount of the assessment on each chapter shall be $2.00 multiplied by the number of Members and Associate Members on the active membership roll of the chapter as of January 1, 1962.

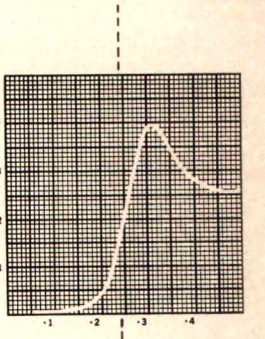
It was *VOTED* that:

2. The annual assessment on each club shall be due and payable on January 1, 1962, and that the amount of the assessment on each club shall be $2.00 multiplied by

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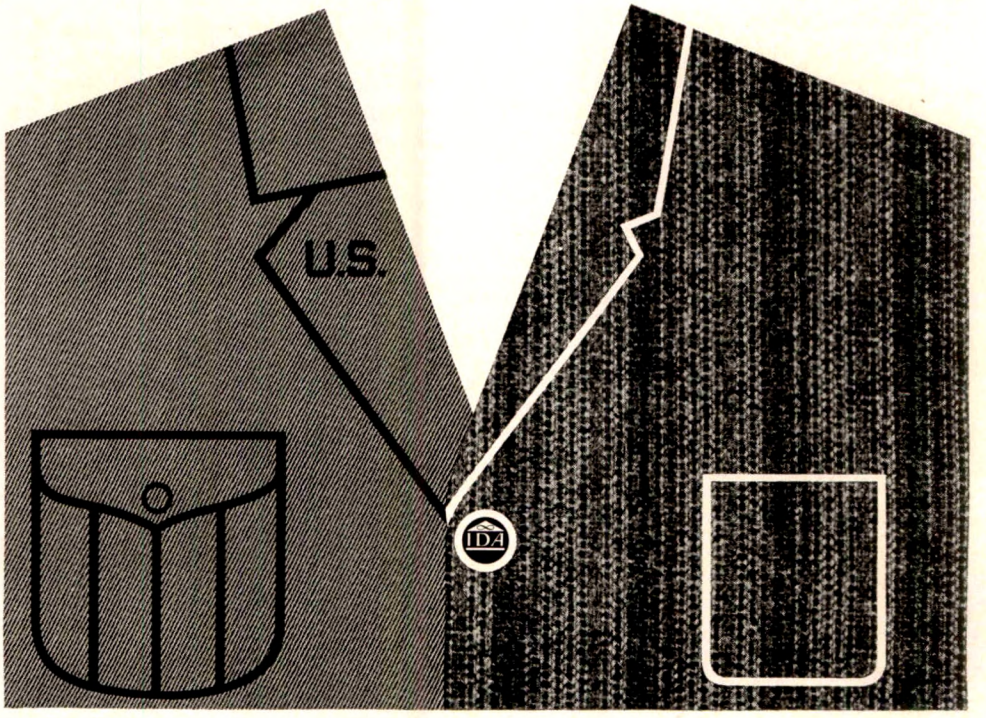
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the number of Members and Associate Members on the active membership roll of the club as of January 1, 1962.

It was *VOTED* that:

3. In sending notice of the 1962 assessment to chapter and club treasurers, the Treasurer of the Society be instructed to advise each chapter and club that the assessment is to be computed strictly on the number of Members and Associate Members on the active membership rolls as of January 1 without regard to whether Members and Associate Members have or have not paid current chapter or club dues, and to explain that this method of fixing the amount of the assessment on each chapter and club has been adopted by the Convention of the Society as the most equitable to all chapters and clubs.

   Further, that each chapter and club should be informed that "the active membership roll as of January 1" is defined as the IBM Membership List as mailed to the chapter or club on or about December 15, 1961, modified by additions and deletions reported to National Headquarters prior to January 31, 1962. If the edited IBM Membership List is not returned to National Headquarters by January 31, assessment will be due for all names on list as mailed by National Headquarters on December 15 and for all subsequent additions.

It was *VOTED* that:

4. The annual assessment on each Member-at-Large and Associate Member-at-Large shall be $2.00 and shall be due and payable on January 1, 1962.

It was *VOTED* that:

5. The initiation fee for 1961 be $4.00 per new Member or new Associate Member and include a subscription to the AMERICAN SCIENTIST, a certificate of appropriate grade of membership, and an initiate booklet.

It was *VOTED* that:

6. The initiation fee for each Promoted Member (from former Associate Member status) be $1.00 and include a membership certificate and a current initiate pamphlet.

Upon completion and discussion of the Treasurer's Report, it was

VOTED that:

The Treasurer's informal report be accepted subject to audit and publication upon completion of the fiscal year of 1961.

Reports and Recommendations from the Executive Committee

National Assessments and Fees for 1963: At the request of the President, Secretary Holme read and explained the meaning of the following action of the National Executive Committee:

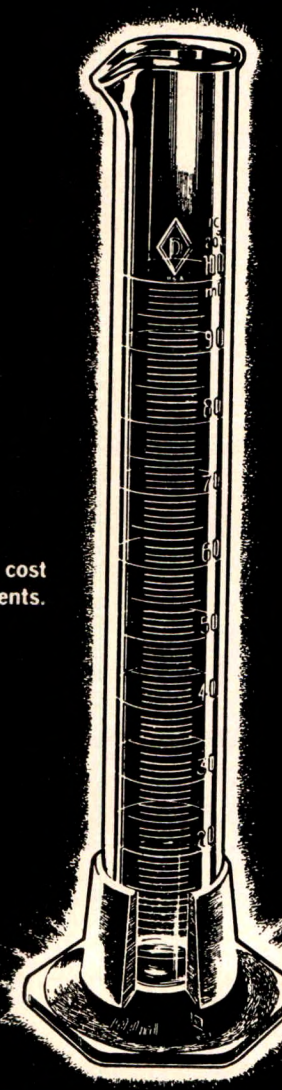
"*VOTED* that: The Executive Committee recommend to the 62nd Annual Convention that the National Assessments for 1963 be increased from $2.00 to $3.00 and that the initiation fees be increased accordingly. Also, that in presenting and justifying this recommendation to the Convention, the statement be made that this increase is the bare minimum and that it might be necessary to return with a recommendation for further increase even at the next Convention."

The Secretary pointed out that, under the rules of the Society, National Assessments and related fees could be increased only when made effective for a period one year hence. This requirement is made necessary in order that chapters and clubs, which ordinarily start the collection of their dues at the beginning of an academic year, would not find the Convention voting an amount in excess of that which had already been collected. It was therefore necessary for the 62nd Convention to look forward a bit to the fiscal year of 1963. Every activity of the Society was experiencing rising costs—the AMERICAN SCIENTIST had already been advised of rising costs in 1962 and 1963 for printing; in addition to this, it would most certainly be experiencing a rise in the cost of paper. The expenses of the National Lectureships were increas-

(*Continued on page 32A*)

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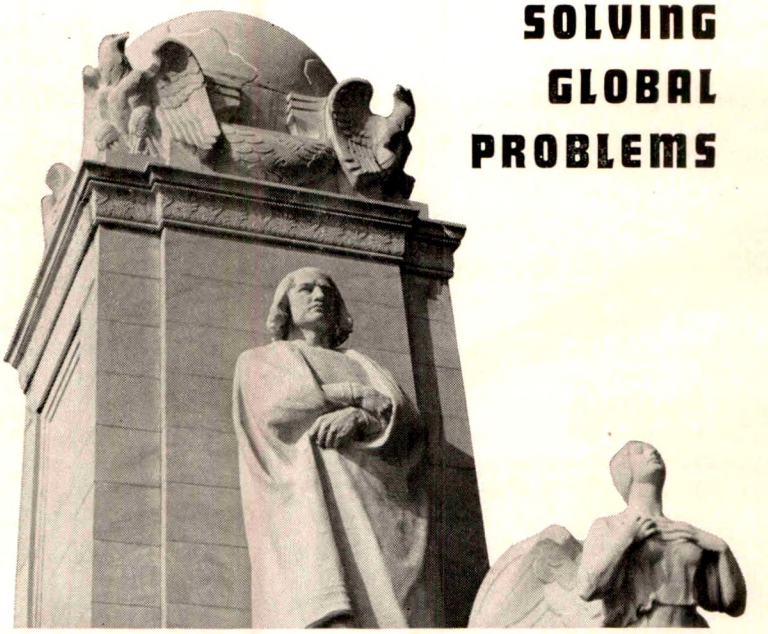
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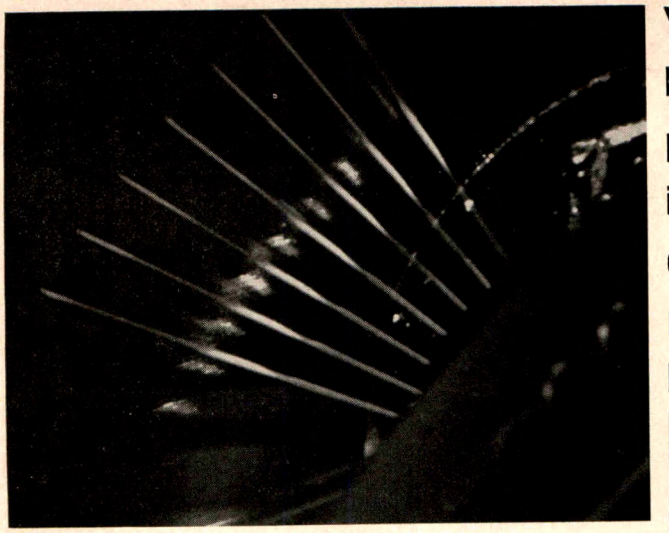
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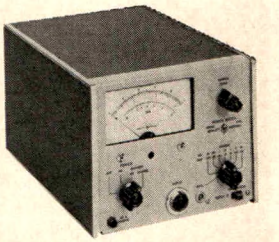
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Progress in scientific measurement is dependent on measuring tools of increasing accuracy. When today's scientist must compromise maximum measuring accuracy for the high cost of specialized measuring devices, technological progress is hampered or blocked.

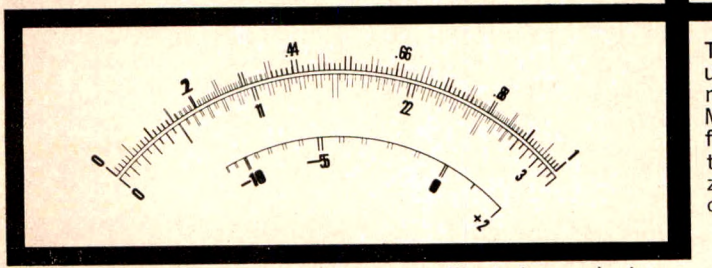
The universally accepted d'Arsonval movement used in modern electrical meters possesses an inherent flaw, "tracking deviation," which is the product of mechanical inconsistencies in the meter movement and which causes differences between the actual electrical quantity under measurement and the meter needle indication or deflection.

Most meters today use preprinted scales calibrated to the individual movement at only a few points—and hand calibration of each point on each meter scale is so time consuming as to make the cost prohibitive.

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142A

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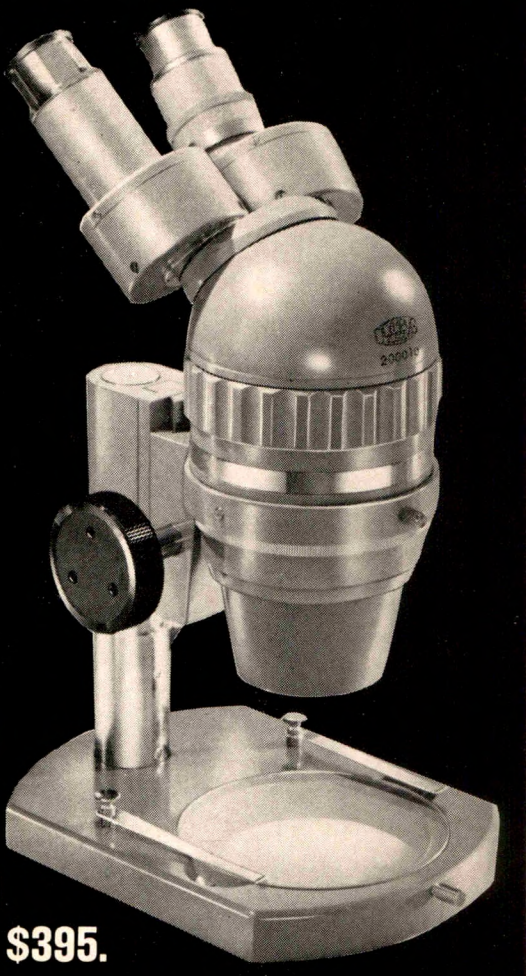
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TECH NEWS

For Scientists, Mathematicians

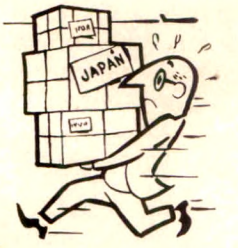
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"Operations research"—the term itself—has attained full status in the recently published Webster's Third International Dictionary. OEG takes particular pleasure in this recognition because of our background as the oldest military operations research organization in the country.



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OEG advises the Chief of Naval Operations, the Commandant, U. S. Marines, and certain Fleet and Force commanders regarding operational problems susceptible to quantitative analysis. A recent example is collected under the title, "The Selection of Cargo for Air Transport." Here the objective was to determine criteria for shipping the myriad replacement parts stocked by the Navy's Yokosuka (Japan) Supply Depot.



It was found, for example, that the items which are candidates for air transport from Oakland, California, to Yokosuka can be selected on the bases of dollar volume of annual shipments and value per pound. Under the assumptions in the study, $7.00 per pound was the break-even point. That is, there will be transportation savings if items with a higher value per pound go to Yokosuka by air.

The more sobering content of another study can be deduced from its title: "The Effects of Radiation on Populations," a two-part work considering (1) the effects on individuals exposed to radiation today and (2) the genetic consequences for future generations. One of many conclusions: The continued detonation of nuclear weapons in the stratosphere, at a 100-megaton-yearly rate, would result in reducing individual life expectancy by approximately 20 days. Although human survival would not be endangered by this testing rate, radiation from a nuclear war—involving the detonation of 100,000 to 1,000,000 megatons—*would* constitute a definite hazard to the mortality of the human race.

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GRANTS-IN-AID OF RESEARCH

The attention of the membership of the Society of the Sigma Xi is called to the fact that **ALL** correspondence with respect to Grants-in-Aid of Research should be addressed to the Executive Secretary, Professor T. T. Holme, at 51 Prospect Street, New Haven 11, Conn. Inquiries addressed to the Editorial Office of the American Scientist result inevitably in unnecessary delays.

Important Notice: The attention of all readers and publishers is called to the present address of the Editorial Offices of American Scientist
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If your name or address has changed from that on file at headquarters at Sigma Xi or RESA; please return the form below to:

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This is to notify you of the following change of address or name. (Please print or typewrite.)

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☐ Member-at-Large
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(*Continued from page 28A*)

ing as a result of increased transportation costs, increased number of lectures, and increased honoraria paid. *Science in Progress* would now be published annually rather than biennially and there would be increased expenses associated with this. The operation costs at National Headquarters certainly had been increased and all indications were that they would continue to increase as volume of work and salary scales continue to rise. The Secretary stated that although the Report of the Treasurer disclosed that 1961 would end with a comfortable balance he (the Secretary) in looking ahead to 1963 doubted that the Society would be able to continue its present level of contributions to Grants-in-Aid of Research let alone increase its activity without increased income. The Secretary stated that if The Society of the Sigma Xi was to continue to be an active organization, working in the interests of encouraging scientific research and rising to meet ever-increasing demands upon it, the increase in the National Assessment should be voted.

There followed considerable discussion during which President-elect Rossini pointed out to the Convention the actual narrow margin of income over expenses represented by the 1961 Budget and after stressing indications of further increases in expenses in 1962 and 1963 he urged approval. After questions concerning the operation of Grants-in-Aid of Research and the possibility of operating under differential assessments, it was unanimously

VOTED that: The Annual Assessment for 1963 be increased from $2.00 to $3.00 and that the initiation fees be increased accordingly.

Petition for Chapter Status: In accordance with the recommendation of the Executive Committee, it was

VOTED that: The Petition for a chapter to be known as the Chicago Medical School Chapter be *GRANTED*.

Proposed Statement for Article V of the Bylaws: President Brode announced that in accordance with the ruling of President Carpenter at the 61st Annual Convention he had appointed a Committee, consisting of Dr. Ralph E. Hefferline, Columbia University, Dr. R. William Shaw, Cornell University, Dr. George Zimmerman, Bryn Mawr College, Dr. Harold G. Cassidy, Yale University, Chairman, to draft wording for Article V of the Bylaws. This Committee had met several times with Dr. Brode in attendance and as a result of its work presented recommendations to the National Executive Committee at its meeting on October 13. President Brode then called upon the Executive Secretary to read the following vote of the Executive Committee:

"*VOTED* that: A recommendation be made to the 62nd Annual Convention for the adoption of the proposed Article V of the Bylaws as submitted by Dr. Cassidy and as edited by the Executive Committee."

Copies of this recommended Bylaw having previously been circulated to the chapters and clubs and distributed to the Convention delegates, it was then

VOTED that: The following be approved as Article V of the Bylaws:

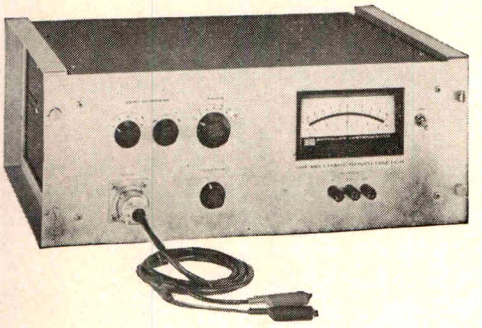
"*Conditions Essential for Election of Members and Associate Members*

"Evidence of suitability for election to Sigma Xi should comprise all those accomplishments expected of companions in zealous research that would normally be recognized by the independent award of degrees, by publication of research, or by other kinds of certification. However, membership in Sigma Xi is neither linked to the possession of any degree, nor contingent upon belonging to some other organization except as set forth in Article VII, Sections 2 and 3 of the Constitution. Responsibility for nominating persons for admission to Sigma Xi as Members and Associate Members, and thus for upholding the high ideals of the Society, rests with the Committee on Admissions of each chapter, and ultimately with the chapter itself. This responsibility includes periodic review of the roster of Associate Members with a view to

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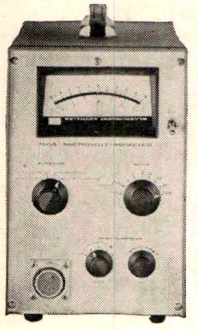
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149	13 overlapping in 1x, 3x steps, 0.1 μv to 100 mv.	2% full scale on all ranges.	After warmup, within 0.01 μv per day.	Greater than 4 x 10^{-18} watt.
150A/150AR* Voltmeter	13 overlapping in 1x, 3x steps, 1 μv to 1 v.	2% full scale on all ranges.	After warmup, within 0.1 μv per day.	Greater than 4 x 10^{-18} watt.
Ammeter	15 overlapping in 1x, 3x steps, 10^{-16} to 10^{-3} amp.	3% full scale on all ranges.	After warmup, ± 2 x 10^{-11} ampere per day.	
151/151R* 151RC†	11 linear, 1x, 3x steps, 0.1 mv to 10 v; 5 non-linear, 0.001 to 10 v; three decades each.	Linear $\pm$3% full scale. Non-linear $\pm$10% of input.	After warmup, below 10 μv per day.	10^{-17} watt.

*Models for rack mounting †Rack-Contact meter model, same as standard unit except for addition of a contact meter relay.

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In yellow, blue and black nu-ances.
The face however seems to me
To be that of the Chim-pan-zee:
A fact that makes the gentle Pansy.
Appeal no longer to my fancy.

At the time the June 1962 issue of AMERICAN SCIENTIST went to press, the roadstands and nurseries in the Princeton area were selling pansy plants for Spring and Summer gardens. This turned our thoughts back to Professor R. W. Wood's book, *How to Tell the Birds from the Flowers*, and his comparison of The Pansy and The Chim-pansy which we reproduce above from the reprint of the volume by Dover Publications, Inc., New York 14, N.Y. To them we were earlier indebted for The Gnu and The Newt, AMERICAN SCIENTIST, Vol. 48, Dec. 1960. Some readers will recall that this inspired a similar effort by Professor and Mrs. A. J. Kohn, AMERICAN SCIENTIST, 49, 75 (1961) comparing The Hare and The Haruspex, suggested in turn by the article of that title in the issue of AMERICAN SCIENTIST, 48, 415 (1960) by E. S. Deevey.

possible promotion. The following guides are offered to the chapters and their Committees on Admissions to assist in interpreting the conditions essential for election to the Society.

"Section 1. *Fields of Activity.* In the expanding field of Science, enumeration of disciplines is not entirely satisfactory as the sole basis for defining criteria for membership. For this reason, nomination of an individual working in a discipline related to those enumerated in Article I, Section 3 of the Constitution may be made provided that the person nominated is, by the quantitative emphasis of his work, combined with synthesis and with qualitative stature, developing his discipline into the kind of activities common to the disciplines listed in Article I, Section 3.

"Section 2. *Full Membership.* Noteworthy achievement as an original investigator in some branch of pure or applied science, specified as a criterion of eligibility for election to full membership, in Article VII, Section 2(a) of the Constitution, must be judged on performance.

"Section 3. *Associate Membership.* Marked excellence in studies as specified for eligibility for election to associate membership, in Article VII, Section 3 of the Constitution should not require as high a level of attainment as the noteworthy achievement specified for full membership. Election to associate membership would normally apply to an undergraduate, or a graduate student, and would rest upon grades in courses and evidence of an aptitude for research."

Report of Editor-in-Chief of AMERICAN SCIENTIST

"This report represents a glance backward through the pages of Volume 49 published in 1961 and a projection of what we may expect in the Jubilee Volume, Volume 50, of 1962.

"In Volume 49 there was a recession from 616 pages of text in Volume 48 to 538 pages in the present year. On the other hand, the double-column text, with advertising, increased from 416 pages in 1960 to 442 pages in 1961. In sum, therefore, we provided our readers with some 980 pages of interesting scientific material as compared with 1032 pages a year ago. Our annual subscription is $2.50 and the charge to members is only $2.00. We believe that this represents a record in scientific literature at the price charged. We have to note that, in spite of the decrease in over-all size, our printing costs have advanced and further increases in such charges are forecast. This gives point to the recommendations which are being laid before the Convention as to increases in national assessments in the future. What the Convention has to decide in this matter is whether to recommend a modest augmentation in assessments or a material cutback in the size of the journal, the cost of which, as the balance sheet shows each March, is more than one half of the total disbursements or receipts.

"We have received for review during the current year approximately 755 books exclusive of paperbacks and obvious discards. In the light of the data in the preceding paragraph it must be recorded that the pricing of these books has advanced phenomenally during the period, while our charges for AMERICAN SCIENTIST have remained stationary. We have reviewed about 189 books in 1961 or an average of 47 per each of the four issues. In 1960, exactly 200 books were reviewed, a further evidence of mounting printing costs confronting us. We have continually to hold down the number and the length of the reviews, we hope without sacrifice of quality.

"We have recommended to the officers of the Society this year an increase in the number of honoraria for articles offered to us by authors for publication. We exclude from such awards not only the National Lectures but also articles reprinted from other sources, many of which are outstanding in merit.

"From Volume 49 we have selected for a major honorarium the article by K. D. Roeder and A. E. Treat, 'The

(*Continued on page 38A*)

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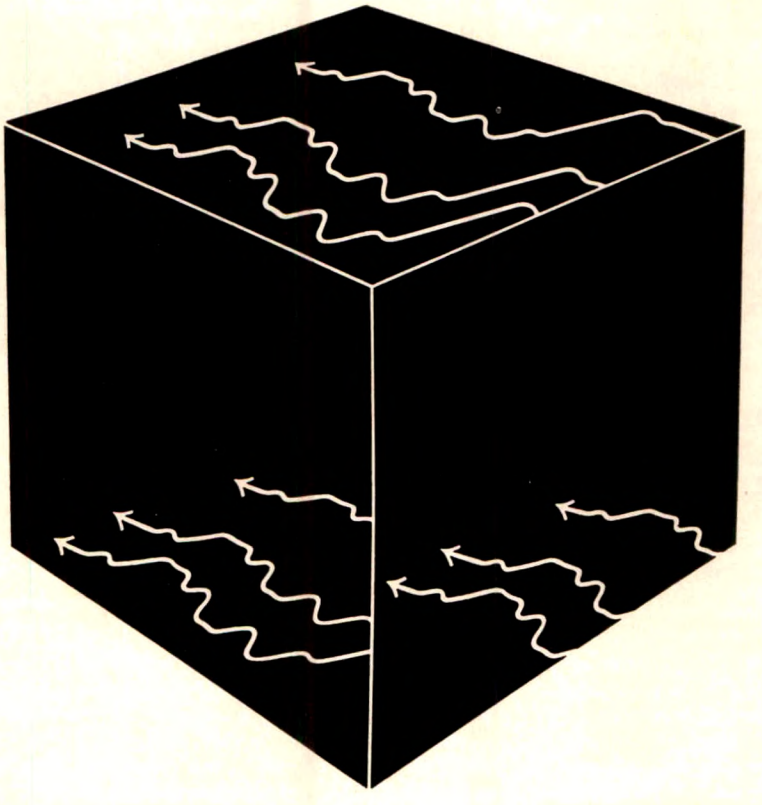
The best way to test new equipment is to put it into actual use. But how can the performance of advanced airborne guidance systems be evaluated without spending millions of dollars in production and flight test of equipment? How can the effect of possible design changes be determined? How much can systems and equipment be improved before over-all performance becomes subject to diminishing returns?

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BOOKS RECEIVED

TO THE MEMBERSHIP: *Our readers are reminded that this section of their journal is intended to be an information service listing first editions of the most important new scientific books received for review in our Princeton Editorial Offices. The listing of paperbacks is being discontinued. In general, the Board of Editors will not encourage the review of symposia.*

The Board of Editors wishes to point out again that members volunteering as reviewers are urged to do so but that a comprehensive statement of professional training and qualifications must accompany a new application.

Integrated Basic Science by S. M. BROOKS; 507 pages; $7.85; The C. V. Mosby Co., 1962.

The Wave Mechanics of Electrons in Metals by S. RAIMES; 367 pages; $13.00; John Wiley & Sons (Interscience Division), 1962.

Mathematics for Secondary School Teachers by B. E. MESERVE and M. A. SOBEL; 367 pages; $9.00 trade edition; Prentice-Hall, 1962.

Nuclear Reactor Fuel Elements (Metallurgy and Fabrication), edited by A. R. KAUFMANN; 739 pages; no price given; Interscience Publishers, 1962.

Mathematical Models of Economic Growth by J. TINBERGEN and H. C. BOS; 131 pages; $6.90; McGraw-Hill Book Co., 1962.

The Ovary, Vol. I by SIR SOLLY ZUCKERMAN; 619 pages; $22.00; Academic Press, 1962.

Elementary and Advanced Trigonometry by K. S. MILLER and J. B. WALSH; 350 pages; $5.75; Harper & Brothers, 1962.

Biographical Memoirs, Vol. XXXVI of The National Academy of Sciences; 336 pages; no price given; Columbia University Press, 1962.

Topics in Modern Algebra by C. P. BENNER, *et al.;* 144 pages; $3.75; Harper & Brothers, 1962.

Science and Salvation by R. S. DE ROPP; 308 pages; $5.00; St Martin's Press, 1962.

Differential Equations by C. W. LEININGER 271 pages; $6.00; Harper & Brothers, 1962.

Banach Spaces of Analytic Functions by K. HOFFMAN; 217 pages; $10.65 trade edition; Prentice-Hall, 1962.

Punch-Card Methods in Research and Documentation With Special Reference to Biology by M. SCHEELE, translated from the 2nd revised German edition by E. HOLMSTROM; 274 pages; 19.50; John Wiley & Sons (Interscience Division), 1962.

Basic Matrix Theory by L. E. FULLER 245 pages; $9.35; trade edition; Prentice-Hall, 1962.

Regular Solutions by J. H. HILDEBRAND and R. L. SCOTT; 180 pages; $7.00; Prentice-Hall 1962.

Fundamentals and Techniques of Mathematics for Scientists by M. M. NICHOLSON; 526 pages; $7.50; John Wiley & Sons, 1962.

Organic Reactions, Vol. XII, A. C. Cope, Editor-in-Chief; 538 pages; $13.00; John Wiley & Sons, 1962.

Digital Computers by R. K. LIVESLEY; 56 pages; $1.75; New York: Cambridge University Press, 1962.

Principles of Electronic Instrumentation by W. A. LYNCH and J. G. TRUXAL; 828 pages; $7.50; McGraw-Hill Book Co., 1962.

The World of Ice by J. L. DYSON; 305 pages; $6.95; Alfred A. Knopf, Inc., 1962.

Space Research, Vol. II (Proceedings of the 2nd International Space Science Symposium Florence 1961), edited by H. C. VAN DE HULST, *et al.;* 1241 pages; $29.50; John Wiley & Sons (Interscience Division), 1962.

Microbial Classification (12th Symposium of The Society for General Microbiology), edited by G. C. AINSWORTH and P. H. A. SNEATH; 483 pages; $9.50; New York; Cambridge University Press, 1962.

Absorption, Distillation and Cooling Towers by W. S. NORMAN; 477 pages; $11.50; John Wiley & Sons, 1962.

Genetics by R. C. KING; 347 pages; $7.50; Oxford University Press, 1962.

Evolution and Genetics (The Modern Theory of Evolution) by D. J. MERRELL; 420 pages; $6.00; Holt, Rinehart & Winston, 1962.

High Magnetic Fields (Symposium of International Conference held at M.I.T. November 1961), edited by H. KOLM, *et al.;* 751 pages; $15.00; The M.I.T. Press & John Wiley & Sons, 1962.

Inertial Guidance, edited by G. R. PITMAN, JR.; 481 pages; $18.50; John Wiley & Sons, 1962.

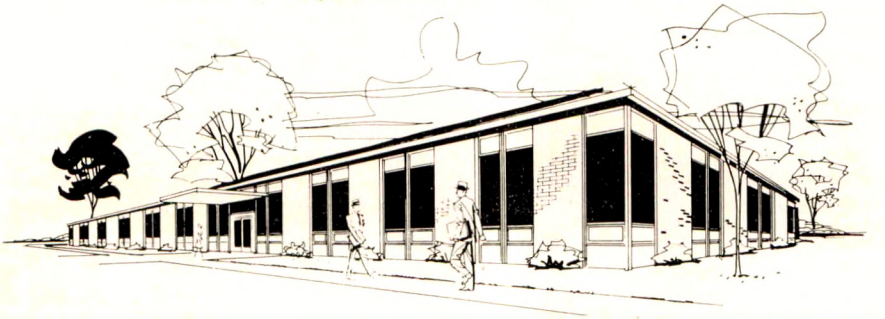
(*Continued on page 152A*)

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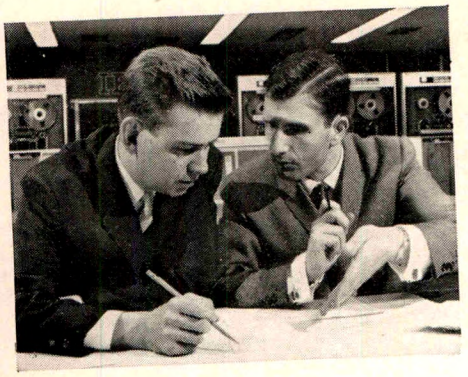
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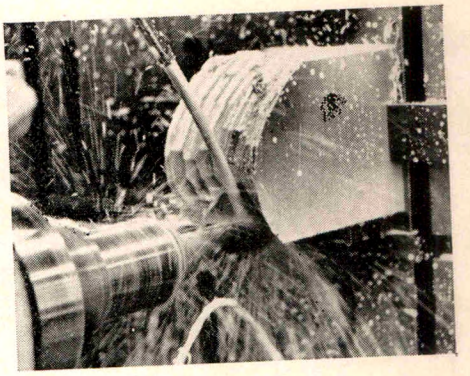
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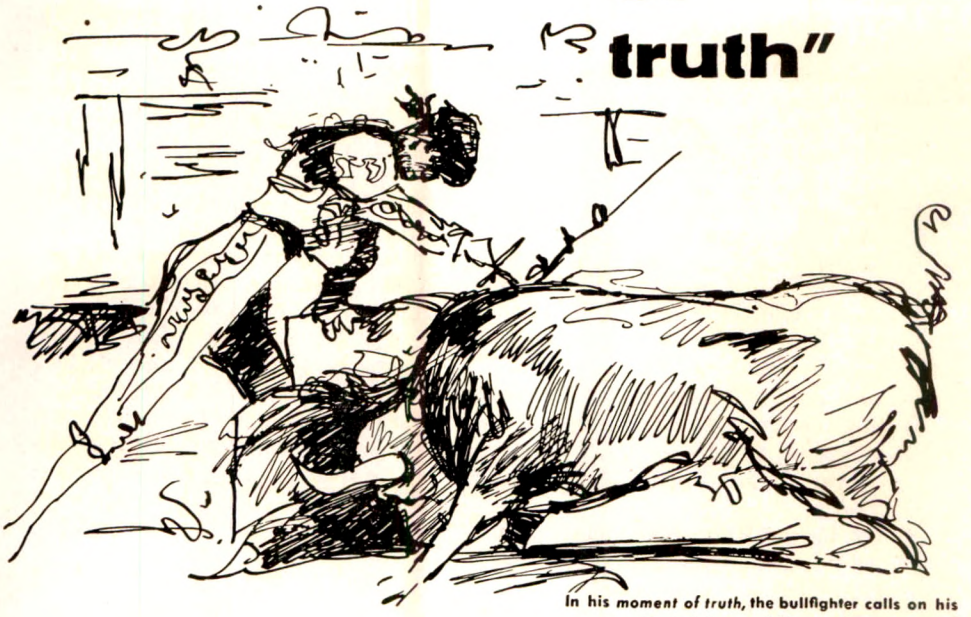
IBM programmers are simplifying communication with computers. Through careful selection and ordering of references to machine structure, they have developed programming systems that transfer a large part of the repetitive work in programming to the computer itself. These systems permit programmers to express their instructions in language resembling English. They also make different machines "look alike" so that programmers can state their problems with as little difficulty as possible. In addition, IBM programmers are experimenting with systems which use the computer's own capacity to construct new programming systems, such as assemblers or compilers.

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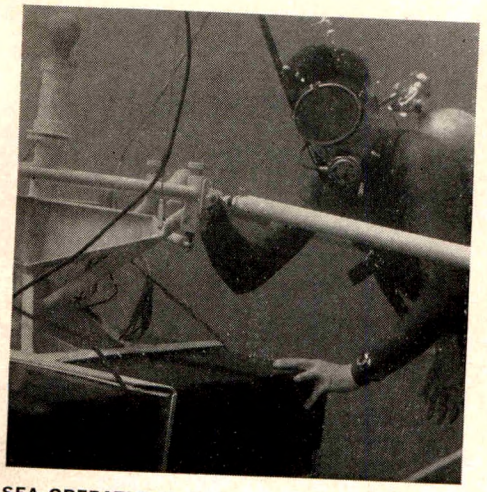
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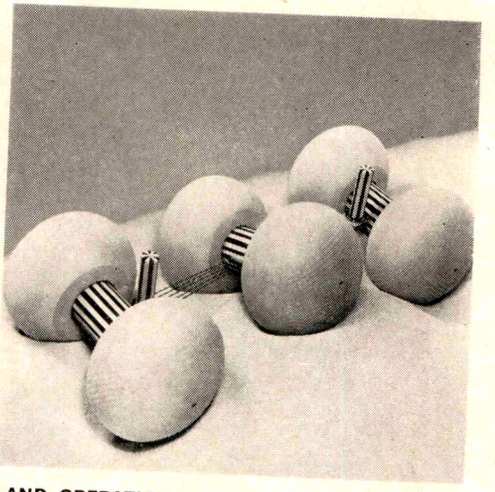
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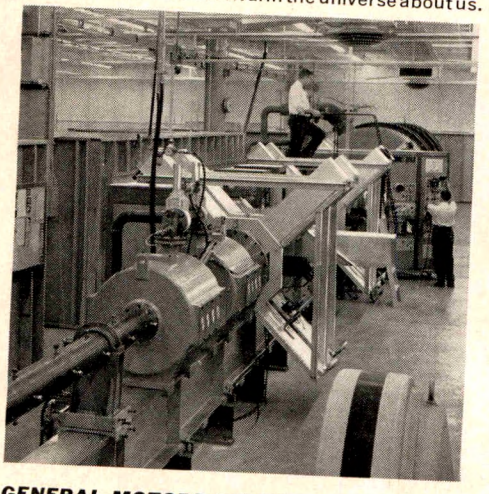


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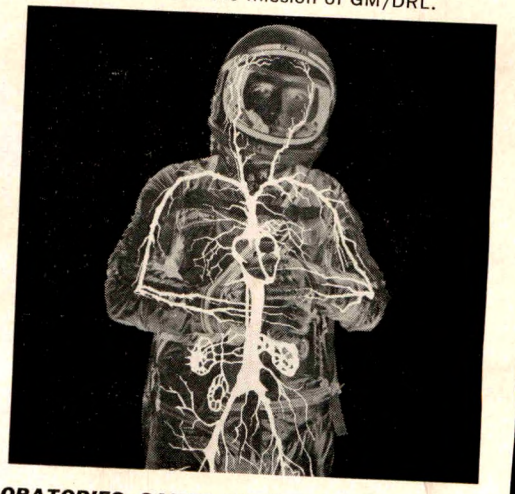


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(*Continued from page 34A*)

Detection and Evasion of Bats by Moths,' June 1961, an article which received through *Time* magazine a wide national and international distribution.

"Other honoraria will be awarded to: R. N. Colwell, 'Some Practical Applications of Multiband Spectral Reconnaissance,' March; A. Lemonick, 'The New Teaching in Physics—A Physicist Speaks with the Humanist,' Perspectives for March; E. W. Muller 'The Field Ion Microscope,' also in March; S. G. Brush, 'John James Waterston and the Kinetic Theory of Gases' which appeared in June; S. Corrsin for 'Turbulent Flow'; and R. E. Danielson for 'Project Stratoscope,' both in the September issue.

"In recognition of the 75th Jubilee Year of The Society of the Sigma Xi, the 15th of RESA, and Volume 50 of AMERICAN SCIENTIST, the Executive Committee has authorized the March 1962 issue, Vol. 50, No. 1, approximately fifty per cent larger than normal. Some thirteen articles are in process of development for this number, including a lead article by the President, and the Procter Prize Address. It is the objective of the issue to take the reader to the borders of scientific advance in biology, biochemistry, the physical chemistry of semiconductors, and to the core constituent of concrete, both of these latter being contributions from industrial research units. Aeronautical science is represented by an article on soaring of birds. Radio astronomy and cosmology, flash photolysis, plasma physics, archeology and geology are among other topics to be presented.

"The printers have collaborated with P. J. Conkwright, the noted typographer of the Princeton University Press, in recasting some aspects of the journal, retaining its present format but changing, one hopes for the better, certain characteristics of the departmental titles and the mode of presentation. We shall use for the whole of Volume 50 colored covers to mark the jubilees attained. We anticipate that the changes to be introduced, though not radical, will merit the approval of our readers. At the same time, the Editors would welcome constructive criticism looking towards an even higher standard of the journal, which has recently received considerable favorable comment, especially from abroad.

"With the cooperation of The Asia Foundation, The Society of the Sigma Xi plans to send 400 copies of Volume 50, free of charge, to a selected group of libraries and scientists in the Philippines, Taiwan, Okinawa, Japan, Korea, Hong Kong, Malaya and Singapore, Thailand, Burma, India, East and West Pakistan and Ceylon. The Foundation and the Society will bear, approximately equally, the cost of this venture among Asian libraries and confreres. Hugh Taylor, *Editor-in-Chief*"

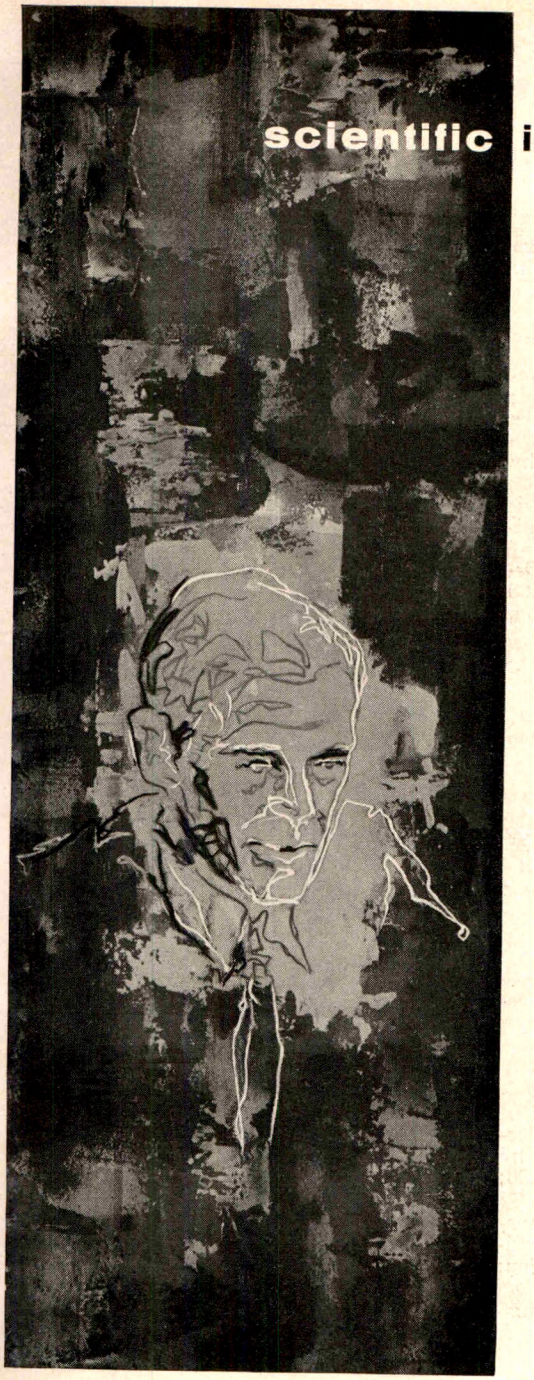
Report of Editor-in-Chief of Science in Progress

Dr. Brode as Editor-in-Chief of *Science in Progress* reported that Volume XII of *Science in Progress* was now being set in type and was expected to be published in April. He also reported that work had already started on Volume XIII as the National Executive Committee had voted to make *Science in Progress* an annual publication henceforth rather than biennial. This action was taken in view of the increase in the number of National Lecturers (double) and the desirability of publishing these in book form without as much delay as is experienced with the biennial issue of *Science in Progress.*

Reports of Other Committees

Membership-at-Large: Dr. C. M. Jansky, Jr., a member of the National Executive Committee and of the Committee on the Membership-at-Large, reported as follows:

"I regret that Dr. James H. Marks, Chairman of the Committee on Membership-at-Large, found it impossible to be present at this Convention. In his absence it becomes my privilege as a member of this committee to present a



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(*Continued from page 148A*)

Living with the Atom by R. CALDER; 275 pages; $5.95; The University of Chicago Press, 1962.

Management and the Computer of the Future, edited by M. GREENBERGER; 340 pages; $6.00; The M.I.T. Press and John Wiley & Sons, 1962.

Fact and Fancy (Seventeen Speculative Essays) by I. ASIMOV; 264 pages; $3.95; Doubleday & Co., 1962.

Biophysical Science by E. ACKERMAN; 626 pages; $13.50 trade edition; Prentice-Hall, 1962.

Quantitative Chemical Techniques of Histo- and Cytochemistry, Vol. I by D. GLICK; 470 pages; $14.50; John Wiley & Sons (Interscience Division), 1962.

Peroxide Reaction Mechanisms, edited by J. O. EDWARDS; 245 pages; $8.00; John Wiley & Sons (Interscience Division), 1962. (Based on Brown University Conference of June 1960.)

Psychological Principles in System Development by R. M. GAGNE, *et al.;* 560 pages; $9.00; Holt, Rinehart & Winston, 1962.

Atoll Environment and Ecology by H. J. WIENS; 532 pages; $15.00; Yale University Press, 1962.

Extraction of Signals from Noise by L. A. WAINSTEIN and V. D. ZUBAKOV; 382 pages; $14.00 trade edition; Prentice-Hall, 1962.

Progress in Polarography, Vol. I by P. ZUMAN with I. M. KOLTHOFF; 355 pages; $12.00; John Wiley & Sons (Interscience Division), 1962.

The Biochemistry of Intracellular Parasitism by J. W. MOULDER; 172 pages; $6.00; The University of Chicago Press, 1962.

Prehistoric Copper Mining in the Lake Superior Region (A Collection of Reference Articles), edited by R. W. DRIER and O. J. DU TEMPLE; 214 pages plus illustrations; $5.95; Published privately by R. W. Drier, 715 Pine St., Calumet, Michigan, 1961.

Electromechanics (A First Course in Electromechanical Energy Conversion) by H. H. SKILLING; 475 pages; $10.00; John Wiley & Sons, 1962.

Fourier Series by G. P. TOLSTOV, translated from the Russian by R. A. SILVERMAN; 336 pages; $13.00 trade edition; Prentice-Hall, 1962.

Elementary Vector Geometry by S. SCHUSTER; 213 pages; $4.95; John Wiley & Sons, 1962.

Eighth International Symposium on Combustion, preface by S. S. PENNER; 1164 pages; $31.00; The Williams & Wilkins Co., 1962.

Letters of the Lewis and Clark Expedition with Related Documents, 1783–1854, edited by D. JACKSON; 728 pages; $10.00; University of Illinois Press, Urbana, 1962.

Fundamentals of Psychology by F. A. GELDARD; 437 pages; $7.50; John Wiley & Sons, 1962.

The Nature of Parsitism (*The Relationship of Some Metazoan Parasites to Their Hosts*); Vol. II of Theoretical and Experimental Biology International Series of Monographs; by W. P. ROGERS; 287 pages; $7.50; Academic Press, 1962.

Use and Abuse of Statistics by W. J. REICHMAN; 336 pages; $5.00; Oxford University Press, 1962.

Animal Tissue Techniques by GRETCHEN L. HUMASON; 468 pages; $8.00; W. H. Freeman, 1962.

Historical Roots of the Principle of Conservation of Energy by ERWIN N. HIEBERT, 118 pages; $3.50; State Historical Society of Wisconsin, 1962.

Inert Atmospheres by P. A. F. WHITE and S. E. SMITH; 235 pages: $9.00; Butterworths, 1962.

The Ideas of Biology by JOHN TYLER BONNER; 180 pages; $4.95; Harper & Brothers, 1962.

The Thinking Machine by JOHN PFEIFFER; 242 pages; $5.95; J. B. Lippincott Co., 1962.

The Ecology and Life History of the Common Frog by R. MAXWELL SAVAGE; 221 pages; $4.25; Hafner Publishing Co., 1962.

Cilia, Ciliated Epithelium, and Ciliary Activity by JOSÉ A. RIVERA; 167 pages; $9.50; Pergamon Press, 1962.

Brain and Behavior, edited by M. A. B. BRAZIER; 433 pages; $6.75 to members and libraries: The American Institute of Biological Sciences, 1962.

Studies in Modern Analysis, Vol. I, edited by R. C. BUCK; 182 pages; $4.00; Prentice-Hall for The Mathematical Association of America, 1962.

Linear Signal-Flow Graphs and Applications by Y. CHOW and E. CASSIGNOL; 144 pages; no price given; John Wiley & Sons, 1962.

Mining Research, edited by G. B. CLARK; 859 pages in two volumes; $30.00 per set; Pergamon Press, 1962.

Cold Spring Harbor Symposia on Quantitative Biology, Vol. XXVI, Cellular Regulatory Mechanisms, edited by L. FRISCH; 408 pages; $8.00; Long Island Biological Association, Cold Spring Harbor, N.Y., 1962.

Dissemination of Cancer (Prevention and Therapy) by W. H. COLE, *et al.;* 462 pages; no price given; Appleton-Century-Crofts, 1961.

Statistical Mechanics by N. DAVIDSON; 540 pages; $14.50; McGraw-Hill Book Co., 1962.

Theoretical Inorganic Chemistry by M. C. DAY and J. SELBIN; 413 pages; $12.00; Reinhold Publishing Corporation, 1962.

(*Continued on page 158A*)

brief report of the committee's activities and viewpoint.

"The total active membership of The Society of the Sigma Xi is approximately 75,000. Of this number approximately 22,000 are not members of any Chapter or Club. It is this group which is represented by the Committee on Membership-at-Large.

"Up to the present, the activities of the Committee on Membership-at-Large have been directed primarily toward producing more money for Grants-in-Aid of Research. Between January 1 and December 27, 1960, the contribution of the Membership-at-Large to the Grants-in-Aid of Research Fund was $17,975.96. Between January 1, 1961 and December 27, 1961, the contribution to Grants-in-Aid of Research was $19,652.56. Since the total direct contributions to this fund for 1961 from all sources except the operating account amounted to $35,000, this means that the Membership-at-Large, comprising slightly less than 30% of the total membership, contributed slightly more than 55% of the total contributions for Grants-in-Aid of Research.

"The Committee desires to increase its communication and contact with the Membership-at-Large. Letters to members have been well received and the responses show a desire to help the Society over and above contributions of money. An objective which the Committee would like to emphasize is to contribute to the image of Sigma Xi in the eyes of the public. At the last meeting of the Executive Committee, Dr. Marks introduced for discussion a proposal that the Membership-at-Large Committee be empowered to nominate to the Executive Committee for election to membership in the Society, scientists of outstanding capability who, for some reason, had not been elected to membership. Dr. Coppoc, Chairman of RESA expressed the thought that RESA should be considered in relation to this problem. It was the consensus of the Executive Committee that this issue should be a subject of possible study by a joint committee of Sigma Xi and RESA.

"Since I am making this report and am not the Chairman of the Committee on Membership-at-Large, it may not be out of place for me as a member of the Executive Committee to pay a tribute to the outstanding leadership Dr. Marks has given the Committee on Membership-at-Large."

Upon the conclusion of Dr. Jansky's report, President Brode added his appreciation to that of Dr. Jansky's for Chairman Marks' services in behalf of The Society of the Sigma Xi.

National Lectureships: Secretary Holme, for Dr. Harvey A. Neville, Chairman of the Committee on National Lectureships, reported on the 1961–62 and 1962–63 Lectureship Series.

"Report on the Status of the 1961–62 Lectureship Program:

165 Number of groups requesting lectures:
- 24 Pacific
- 21 Northeast
- 23 Southeast
- 18 Mid-West
- 20 Southwest
- 19 Mid-Atlantic
- 18 Plains
- 22 Central

150 Number of lectures scheduled:
- 20 Pacific
- 17 Northeast
- 20 Southeast
- 18 Mid-West
- 19 Southwest
- 18 Mid-Atlantic
- 16 Plains
- 22 Central

165 Number of groups to have lectures:
- 24 Pacific
- 22 Northeast
- 22 Southeast
- 18 Mid-West
- 20 Southwest
- 19 Mid-Atlantic
- 18 Plains
- 22 Central

13 Number of joint lectures scheduled:
- 4 Pacific
- 4 Northeast
- 2 Southeast
- 0 Mid-West
- 1 Southwest
- 1 Mid-Atlantic
- 1 Plains
- 0 Central

54 Number of groups that did not want lecture:
- 12 Pacific
- 18 Northeast
- 2 Southeast
- 2 Mid-West

(*Continued on page 44A*)



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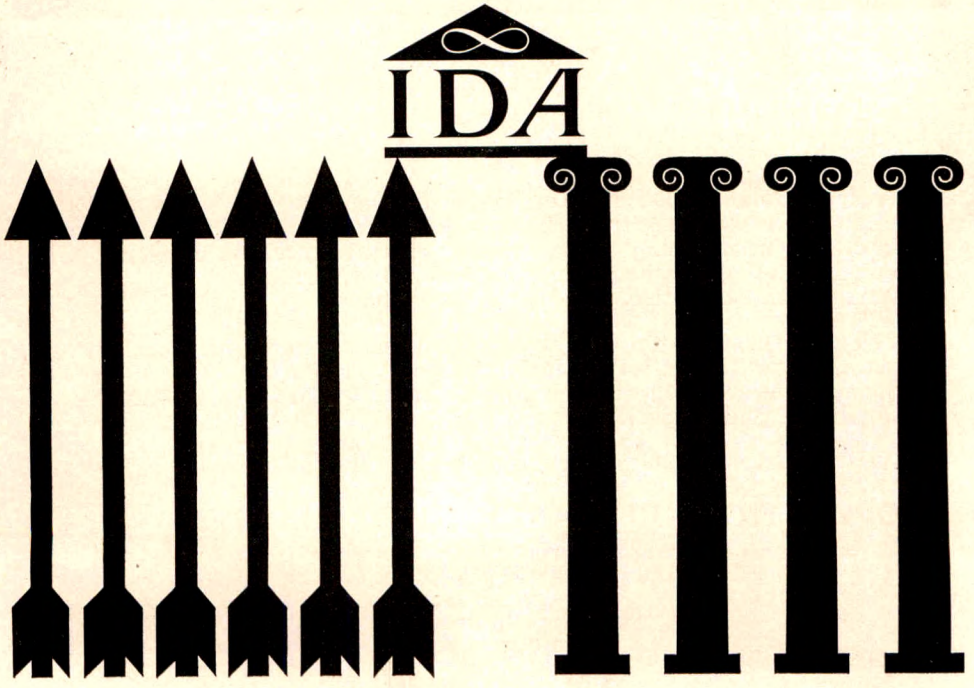
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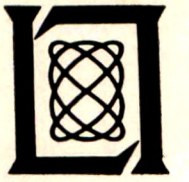
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The Department of Defense is required to make hundreds of decisions daily that affect the well-being and security of every one of us. It must have the highest possible quality of technical advice before making its far-reaching and consequential decisions. * For one major source of such advice, the Department has turned to the Institute for Defense Analyses, a non-profit corporate entity sponsored by nine great universities. The Institute has assembled a staff from the national sources of professional talent—from industry, from research and development laboratories, from university faculties and graduate circles. * Thus IDA is a bridge between two worlds, the Department of Defense on one side and the academic and professional scientific community on the other. In serving as a bridge for a traffic of ideas and recommendations on major national problems, IDA contributes to the development of the immediate and future posture of our national security. IDA employs particularly talented individuals, from all sources of such talent, who can contribute to this great challenge with the highest degree of competence and imagination. * IDA welcomes inquiries from qualified people of professional and academic backgrounds in the scientific and engineering disciplines who feel they can contribute at this level and want to do so. Professional people in the social sciences would also find an IDA career stimulating and challenging. Please communicate with us.

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RE-ENTRY PHYSICS

PENETRATION AIDS DEVELOPMENT

TARGET IDENTIFICATION RESEARCH

SYSTEMS:

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- Strategic Communications
- Integrated Data Networks

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SYSTEM ANALYSIS

COMMUNICATIONS:

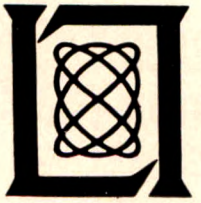
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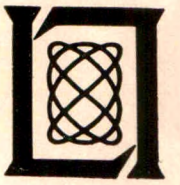
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RADIO PHYSICS and ASTRONOMY

RE-ENTRY PHYSICS

PENETRATION AIDS DEVELOPMENT

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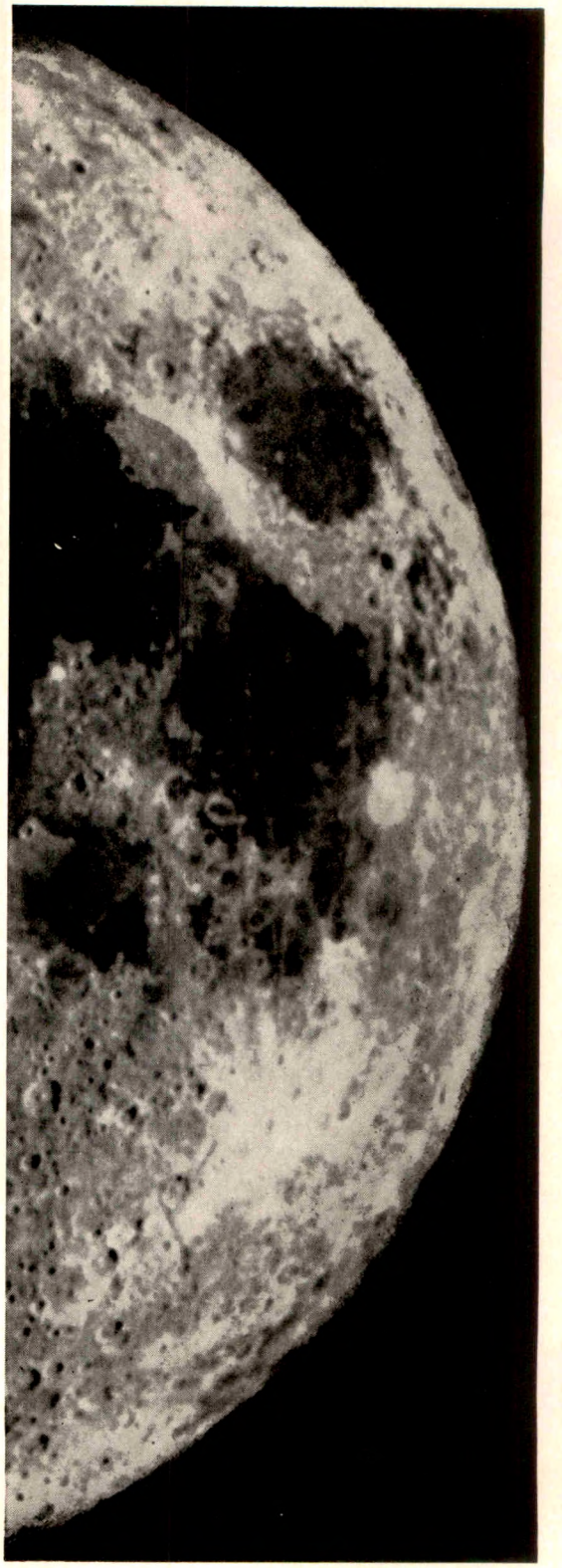
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(*Continued from page 40A*)

3 Southwest
7 Mid-Atlantic
7 Plains
3 Central

89 Number of groups that did not reply:
8 Pacific
31 Northeast
3 Southeast
10 Mid-West
8 Southwest
20 Mid-Atlantic
3 Plains
6 Central

Request not filled: One group in Southeast requested after Itinerary was complete.

"Pacific Tour: 'Carbon—An Old But New Material,' Dr. Philip L. Walker, Jr. of Pennsylvania State University, October 2–November 3, 1961 (20 lectures scheduled).

"Plains Tour: 'Reorganization of Molecular Structures Through Ligand-Interchange,' Dr. John R. Van Wazer, Jr., of Monsanto Chemical Company, October 23–November 15, 1961 (16 lectures scheduled).

"Mid-Atlantic Tour: 'The Uniqueness of Plants' and 'Plant Roots and the Root Environment,' Dr. A. Geoffrey Norman of University of Michigan, February 5–March 2, 1962 (18 lectures scheduled).

"Southwest Tour: 'Magneto-Gas Dynamics: Its Science and Technology,' Dr. Ali Bulent Cambel of Northwestern University, February 27–March 23, 1962 (19 lectures scheduled).

"Southeast Tour: 'Nutrition and Human Behavior: Experimental Investigations on the Impact of Deficient Diet,' Dr. Josef Brozek of Lehigh University, March 19–April 13, 1962 (20 lectures scheduled).

"Central Tour: 'The World's Exploding Population,' Dr. Karl Sax of North Carolina State College, March 1–March 30, 1962 (22 lectures scheduled).

"Northeast Tour: 'A *New* Energy Source: Liquefied Natural Gas,' Dr. Cedomir M. Sliepcevich of University of Oklahoma, April 2–27, 1962 (17 lectures scheduled).

"Mid-West Tour: 'Space Propulsion Engines—Their Characteristics and Research Problems,' Dr. Maurice J. Zucrow of Purdue University, April 2–26, 1962 (18 lectures scheduled).

"*Report on Lectures for 1962–63:*

"Pacific Tour: Dr. David M. Gates, National Bureau of Standards, Boulder, Colorado, 'The Energy Environment in Which We Live.'

"Plains Tour: Dr. Richard P. Goldthwait, Ohio State University, 'Glacier Fluctuations Since the Ice Age in Southeast Alaska.'

"Central Tour: Dr. Sigmund Weissmann, Rutgers University, 'Investigation of Crystalline Imperfections and Properties of Materials.'

"Mid-Atlantic Tour: Dr. William A. Nash, University of Florida, 'Static and Dynamic Buckling of Thin Shells.'

"Southeast Tour: Dr. Hunter Rouse, State University of Iowa, 'On the Role of Eddies in Fluid Motion.'

"Southwest Tour: Dr. Carl Pfaffman, Brown University, 'The Sensory and Motivating Properties of Taste.'

"Northeast Tour: Not yet complete.

"Mid-West Tour: Not Yet Complete."

Grants-in-Aid of Research

The Executive Secretary, in the absence of Dr. Harlow Shapley, reported on the activities of the Grants-in-Aid of Research Committee. Excerpts from a letter by Chairman Shapley to the Membership-at-Large setting forth the problems facing the Grants-in-Aid of Research Committee were read to the Convention. It was further explained that increased numbers of applications now required the Committee on Awards to meet three times a year (December, March, and June) rather than once a year as had been possible in the past. It was pointed out that the complete report of awards made to date had been published in the September issue of the AMERICAN SCIENTIST and that approximately 16 applications—8 granted, 4 postponed until March meeting, 4 not granted—had been processed at the December meeting and notices of these

TECH NEWS

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May 1962 certainly stood out as "Operations Research Month"—in fact, if not by official proclamation. May saw the 10th anniversary meeting in Washington of the Operations Research Society of America (ORSA) as well as OEG's 20th Anniversary Conference on Operations Research, also in Washington.

OEG, created in May 1942, is the oldest continuing military operations research organization in the country. The 20th Anniversary Conference —of international scope—reviewed applications of OR in NATO, in industry and public health, and in fiscal planning for defense, as well as considering OR education.

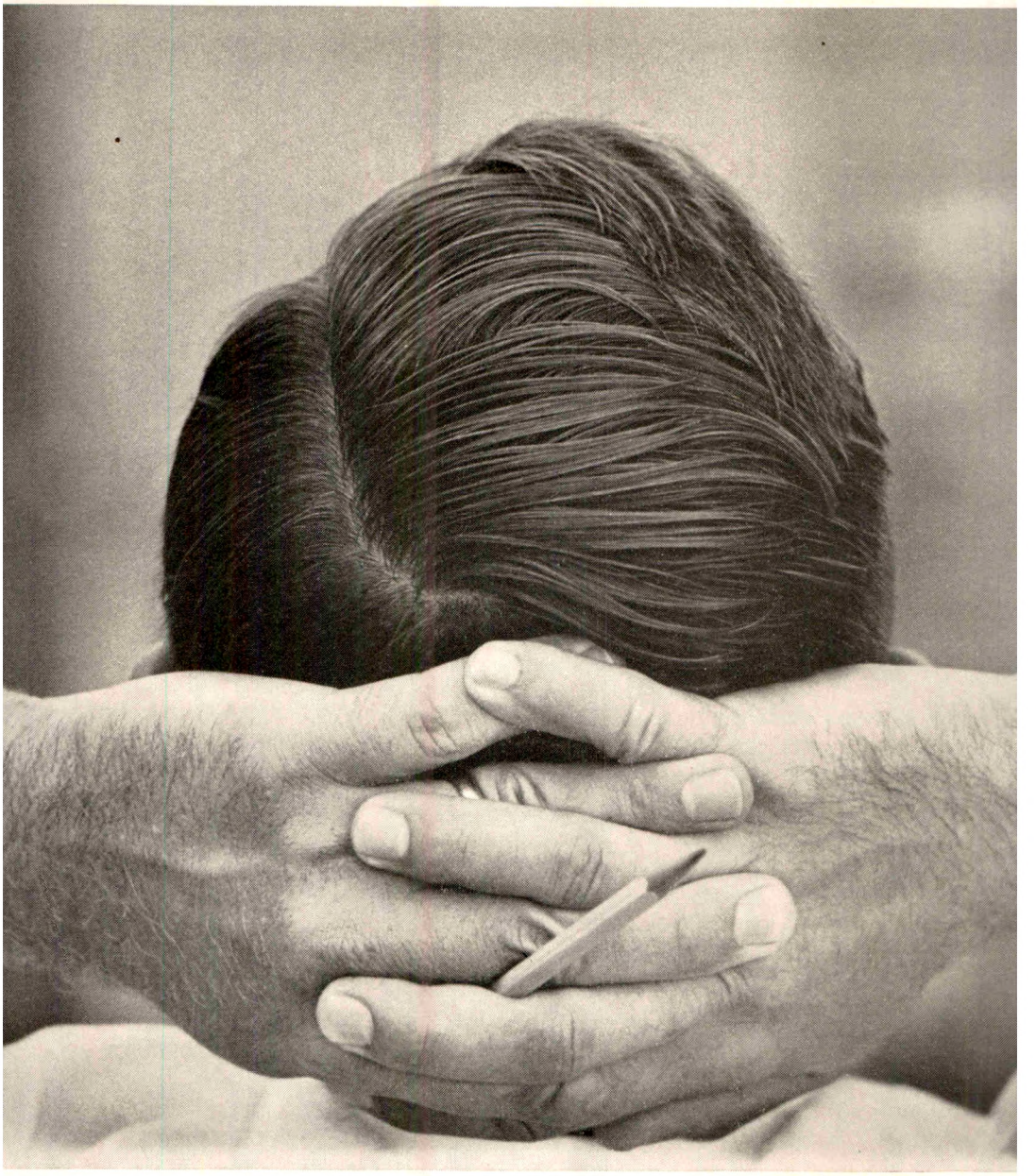
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- **Can a workable radar theory be formulated at the present state of the art?**
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(*Continued from page 152A*)

Studies in Statistical Mechanics, Vol. I, edited by J. De Boer and G. E. Uhlenbeck; 350 pages; $13.75; John Wiley & Sons (Interscience Division), 1962.

The Whispering Land by G. Durrell; 235 pages; $3.95; The Viking Press, 1962.

Asymptotic Estimates and Entire Functions by M. A. Evgrafox; 181 pages; $8.00; Gordon & Breach, 1962.

Elementary Quantum Mechanics by P. Fong; 367 pages; $9.75; Addison-Wesley Publishing Co., 1962.

A Textbook of Comparative Endocrinology by A. Gorbman and H. A. Bern; 468 pages; $12.50; John Wiley & Sons, 1962.

An Introduction to Modern Botany by V. A. Greulach and J. E. Adams; 557 pages; $7.50; John Wiley & Sons, 1962.

Algebraic Logic by P. R. Halmos; 271 pages; no price given; Chelsea Publishing Co., 1962.

Organic Peroxides, Their Formation and Reactions by E. G. E. Hawkins; 434 pages; $12.50; D. Van Nostrand Co., 1962.

The Physiology of Flowering by Wm. S. Hillman; 164 pages; $4.50; Holt, Rinehart & Winston, 1962.

Biochemical Mechanisms by L. L. Ingraham; 108 pages; $5.75; John Wiley & Sons, 1962.

Agglomeration (Based on an International Symposium, Philadelphia, April 1961), edited by Wm. A. Knepper; 1109 pages; $25.00; John Wiley & Sons (Interscience Division), 1962.

Electrical Breakdown of Insulating Liquids by J. A. Kok; 132 pages; no price given; Interscience Publishers, 1961.

Atomic Spectra by H. G. Kuhn; 436 pages; $13.00; Academic Press, 1962.

Particle Accelerators by M. S. Livingston and J. P. Blewett; 666 pages; $17.50; McGraw-Hill Book Co., 1962.

Elements of Functional Analysis by L. A. Lusternik and V. J. Sobolev; 411 pages; $12.50; Gordon & Breach, 1961.

Mathematical Tables for Research Workers in Human Genetics by S. Maynard-Smith, *et al.;* 74 pages; $7.50; Little, Brown & Co., 1962.

University Calculus with Analytic Geometry by C. B. Morrey, Jr.; 754 pages; $12.50; Addison-Wesley Publishing Co., 1962.

A Naturalist in Alaska by A. Murie; 302 pages; $6.50; The Devin-Adair Co., 1961.

Physical Techniques in Biological Research, Vol. IV: Special Methods, edited by Wm. L. Nastuk; 410 pages; $13.00; Academic Press, 1962.

Summation of Infinitesimal Quantities by I. Natanson; 66 pages; $4.50; Gordon & Breach, 1962.

Photoproduction and Scattering of Pi-Mesons by N. F. Nelipa; 99 pages; $5.00; Gordon & Breach, 1961.

The Method of Functionals in the Quantum Theory of Fields by Yu V. Novozhilov and A. V. Tulub; 79 pages; $4.50; Gordon & Breach, 1962.

The Dynamics of International Politics by N. J. Padelford and G. A. Lincoln; 634 pages; $7.50; The Macmillan Co., 1962.

Physical Chemistry by M. A. Paul; 460 pages; $8.25; D. C. Heath & Co., 1962.

Modulation and Coding in Information Systems by G. M. Russell; 260 pages; $7.50 text edition; Prentice-Hall, 1962.

Molecular Biophysics by R. B. Setlow and E. C. Pollard; 545 pages; $11.75; Addison-Wesley Publishing Co., 1962.

The Ethnobotany of Pre-Columbia Peru by M. A. Towle; 180 pages; $6.00; Aldine Publishing Co., 1962.

Fluorescence Assay in Biology and Medicine by S. Udenfriend; 505 pages; $14.00; Academic Press, 1962.

Lectures on Game Theory by E. S. Ventzel; 78 pages; $4.50; Gordon & Breach, 1961.

Introduction to Physical Oceanography by W. S. Von Arx; 422 pages; $15.00; Addison-Wesley Publishing Co., 1962.

Social Life of Early Man, edited by S. L. Washburn; 299 pages; $7.50; Aldine Publishing Co., 1962.

The World Role of Universities by E. W. Weidner; 366 pages; $6.95; McGraw-Hill Book Co., 1962.

Mathematical Statistics by S. S. Wilks; 644 pages; $15.00; John Wiley & Sons, 1962.

Plant Life of Palestine (Israel and Jordan) by M. Zohary; 262 pages; $8.00; The Ronald Press Co., 1962.

Experimental Thermochemistry, Vol. II, edited by H. A. Skinner; 457 pages; $14.50; John Wiley & Sons (Interscience Division), 1962.

Methods of Biochemical Analysis, Vol. IX, edited by D. Glick; 452 pages; $14.50; John Wiley & Sons (Interscience Division), 1962.

Comparative Neuropathology by J. R. Innes and L. Z. Saunders; 839 pages; $32.00; Academic Press, 1962.

Contemporary Physics by C. F. Von Weizsäcker and J. Juilfs; 150 pages; $4.00; George Braziller, 1962.

Soil Animals by D. K. McE. Kevan; 237 pages; $10.00; Philosophical Library, 1962.

Fossils, An Introduction to Prehistoric Life by Wm. H. Matthews, III; 337 pages; $5.75 cloth; $2.25 paper; Barnes & Noble, 1962.

Diagnosis and Treatment of Acute Radiation Injury; 425 pages; $8.00; International Documents Service, Columbia University Press for International Atomic Energy Agency and the World Health Organization, 1962.

actions were to be mailed out. All indications were that applications for 1962 would be even greater in numbers and in amounts requested than those in 1961. Although the total amount of awards had been increasing steadily (1961 25% over 1960) it would be highly questionable if any increase, let alone a corresponding increase, could be made in 1962. Certainly, the action of the Convention in approving an increase in the assessments for 1963 would be most encouraging news to the members of the Grants-in-Aid of Research Committee hard-pressed to meet all worthy requests made upon it.

Report of Nominating Committee and Election of Officers

Dr. Brode announced that, in accordance with the Constitution, he had appointed a Nominating Committee for the purpose of nominating two members to the National Executive Committee to replace Dr. Roy L. Whistler and Dr. Harvey A. Neville whose terms expired with this Convention. It was pointed out, however, that Dr. Neville, as Director of National Lectureships, would continue as an Ex-officio member of the Executive Committee.

In the absence of Dr. Farrington Daniels, Chairman of the Nominating Committee, Dr. George Moore, a member of that Committee, presented the following report:

"The 1961 Nominating Committee appointed by President Wallace R. Brode to make nominations for consideration by the 62nd Annual Convention of The Society of the Sigma Xi at Denver, Colorado, on December 29, 1961, has the honor to present the following candidates, each of whom has agreed to serve if elected:

"Executive Committee: Dr. C. Guy Suits, General Electric Company, Schenectady, New York. Four year term.

"Executive Committee: Dr. Frederick C. Lindvall, California Institute of Technology, Pasadena, California. Four year term.

"Respectfully submitted,
George Boyd
Gerald J. Lieberman
George Moore
Marsh W. White
Farrington Daniels, *Chairman*"

It was *VOTED* that:
The Report of the Committee on Nominations be accepted. The President then asked for nominations from the floor. No other nominations were presented and on motion,

It was *VOTED* that:
The nominations be closed and the Secretary be instructed to cast a unanimous ballot for the Committee Members named.

On motion from the floor, it was

VOTED that: The assembled delegates by a rising vote express the appreciation of the Convention to the Officers and Committee Chairmen for their work and efforts given in support of the aims of The Society of the Sigma Xi.

President Brode on behalf of the Officers and Committee Members thanked the Convention delegates for their expression of appreciation.

General Business

Dr. Brode announced that the evening address would be in cooperation with Phi Beta Kappa and Dr. Harrison Brown would speak on "Science and Government."

There being no further business to be presented to the Convention, the President declared the Convention adjourned at 4:00 P.M.

Respectfully submitted,
THOMAS T. HOLME
Executive Secretary

(*Continued on page 50A*)

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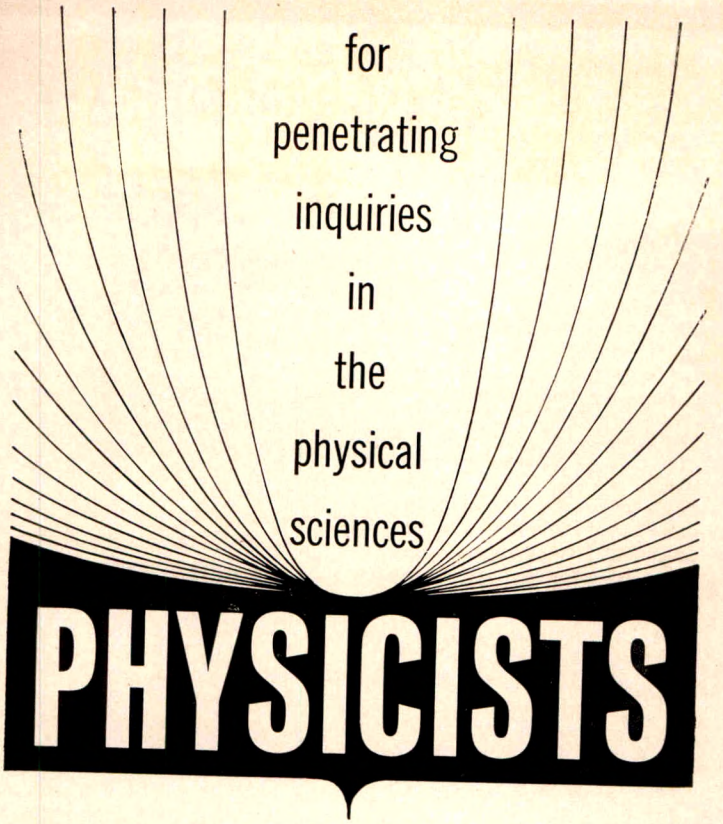
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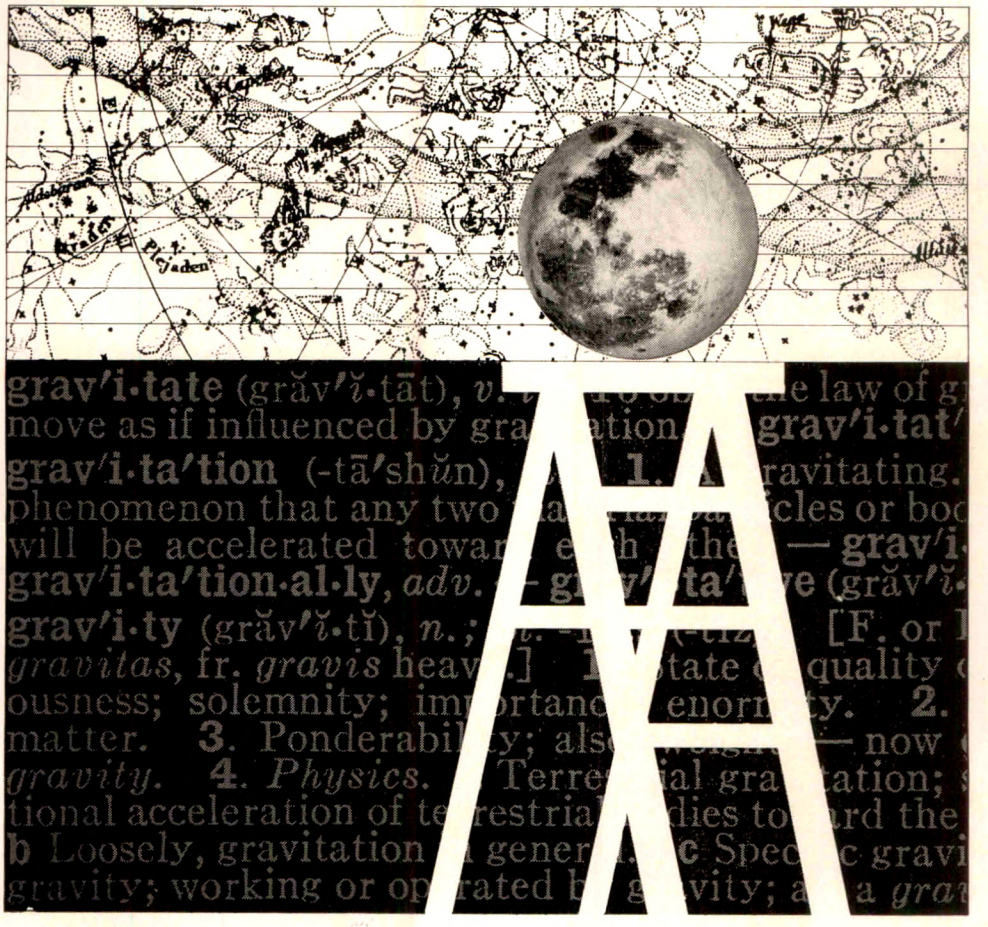
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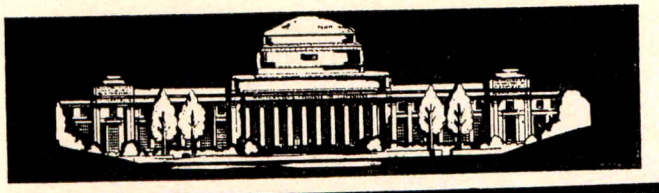
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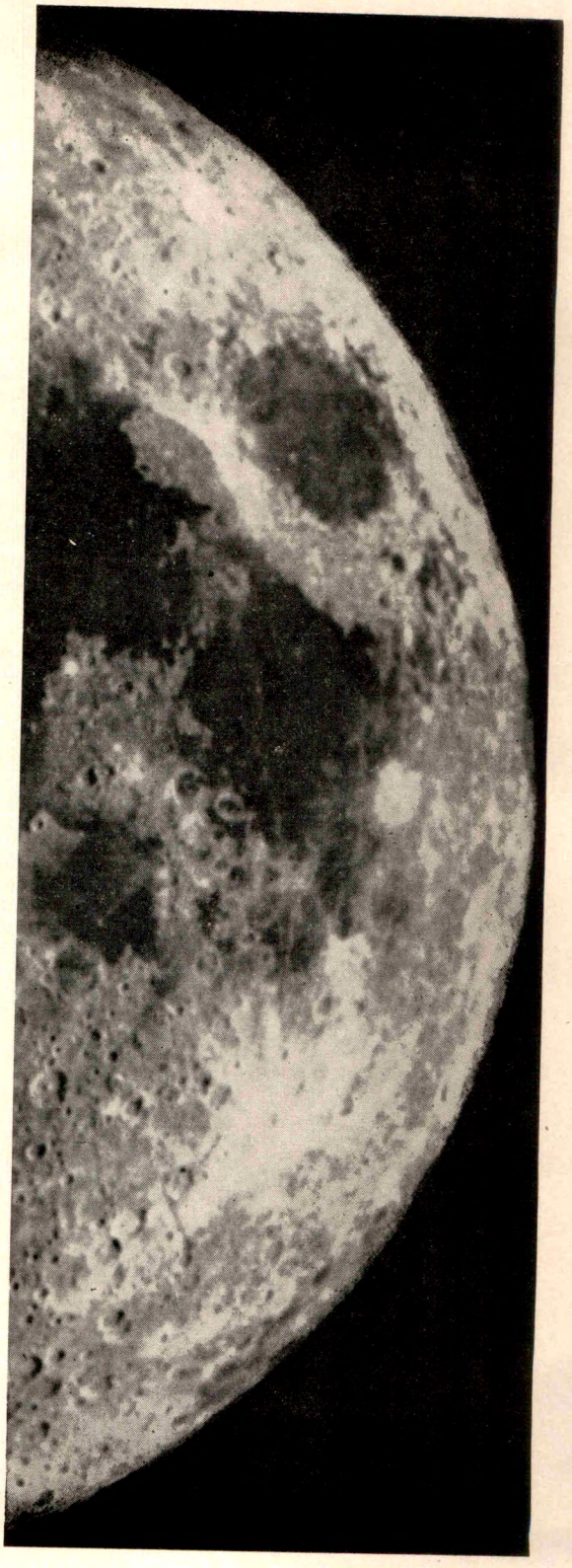
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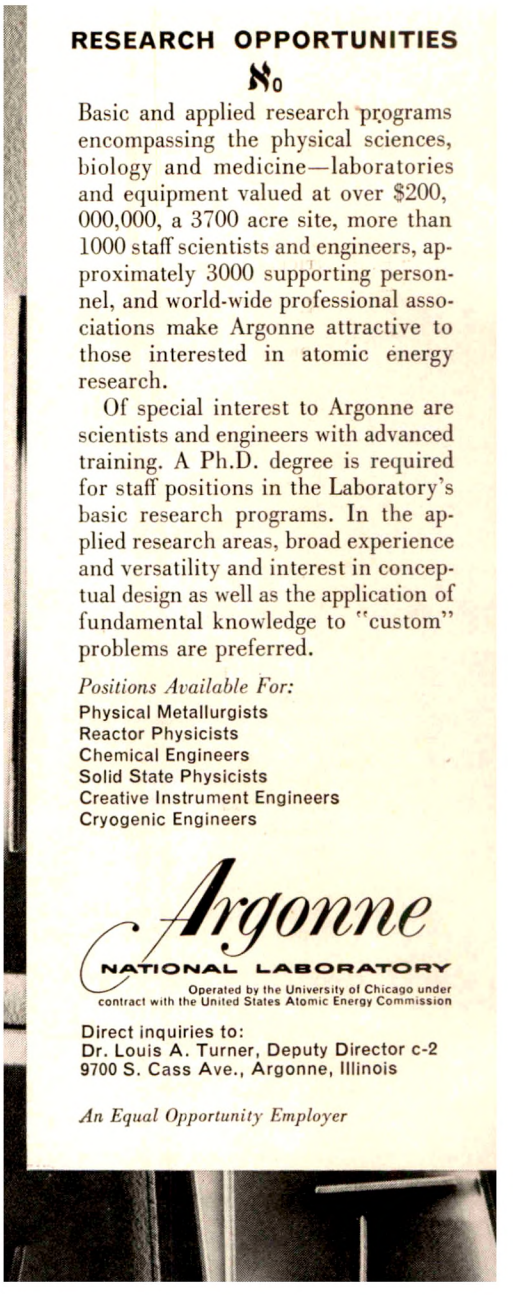
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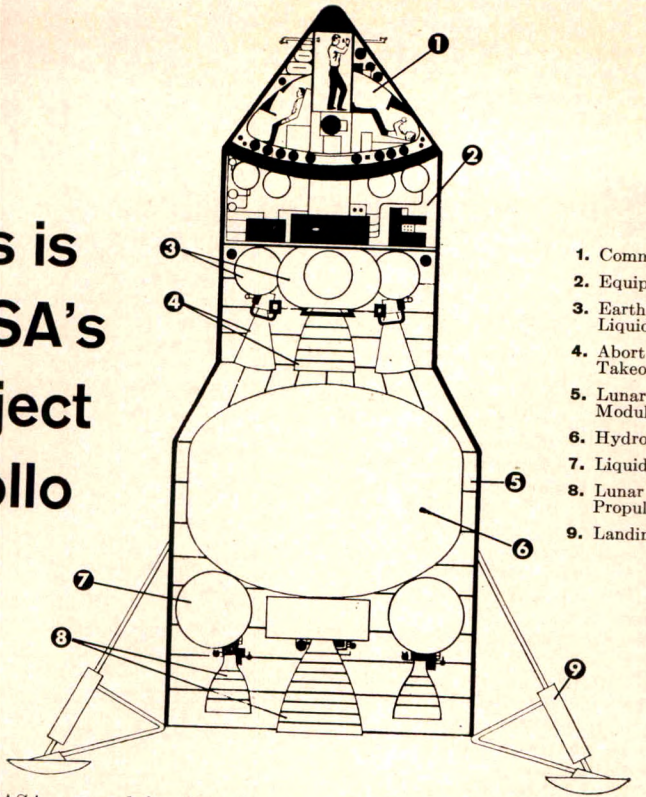
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The project will proceed in three stages. Before the culminating voyages to the moon, three-man Apollo spacecraft will first orbit the earth for as long as two weeks. Next, spacecraft will head out toward the moon, circle it and return to earth.

The end product of Project Apollo and NASA's other space programs is not just placing a man on the moon, but the release of a flood of knowledge and benefits for mankind through research and development. We will chart the last unexplored sea on the map—the great void of space; we will improve weather forecasting (where even a 10 per cent gain in accuracy would save the nation billions of dollars every year); we have already taken the first steps in establishing a global system of communications satellites; and we may expect new products and new techniques that will stimulate the entire industrial spectrum.

Still, the advances in scientific knowledge hold the exciting promise of much greater returns, far beyond what earthbound man can possibly envision.

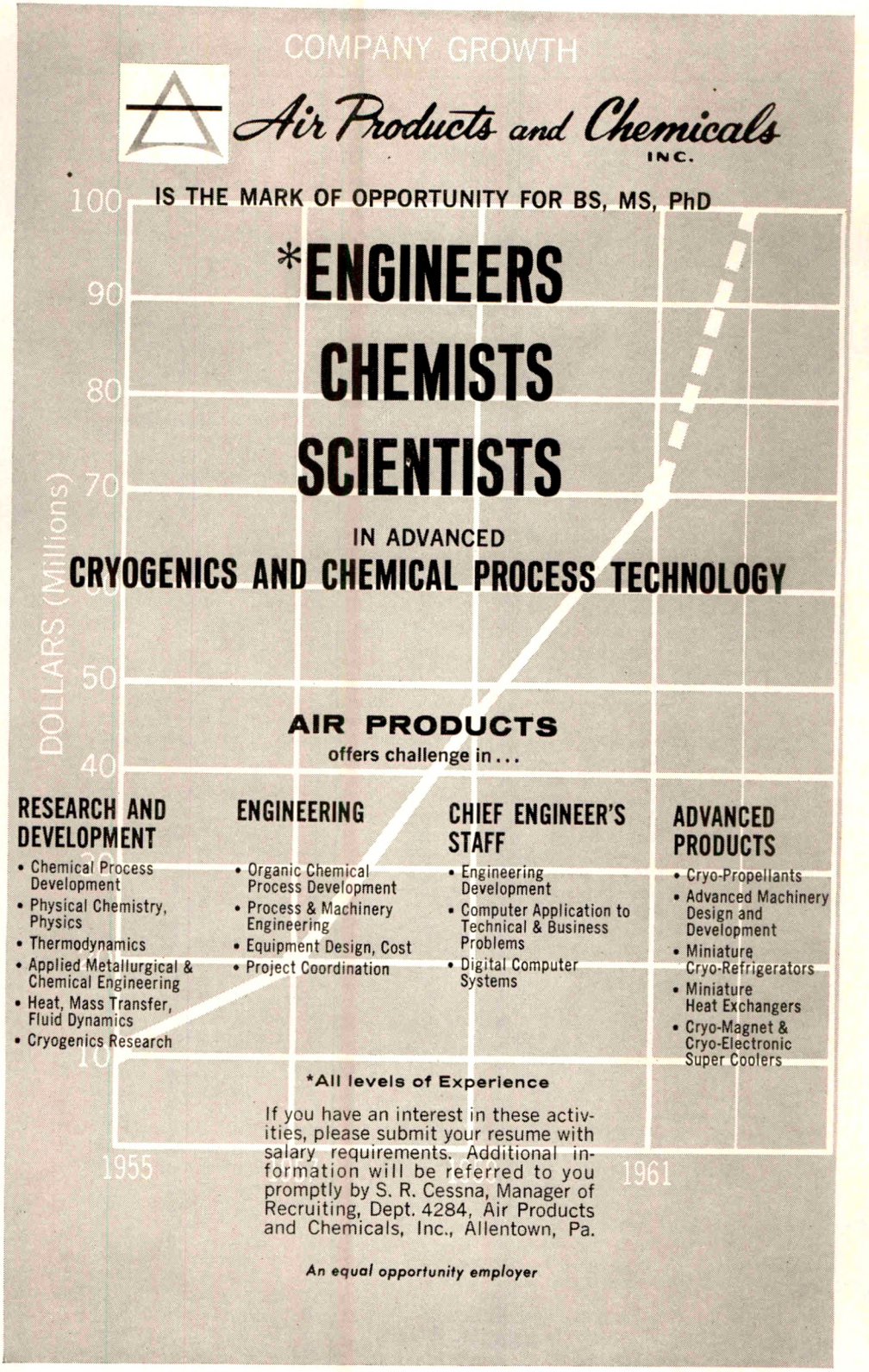
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Unified Organic Chemistry by C. A. MACKENZIE; 586 pages; $8.50; Harper & Bros., 1962.

Analysis and Design of Nonlinear Feedback Control Systems by G. J. THALER and M. P. PASTEL; 464 pages; $14.50; McGraw-Hill Book Co., 1962.

Scientists: Their Psychological World by B. T. EIDUSON; 299 pages; $6.50; Basic Books, 1962.

Basic Principles and Calculations in Chemical Engineering by D. M. HIMMELBLAU; 459 pages; $13.00 trade edition; Prentice-Hall, 1962.

The Palaces of Crete by J. W. GRAHAM; 269 pages plus 153 plates; $7.50; Princeton University Press, 1962.

Experimentation, An Introduction to Measurement Theory and Experiment Design by D. C. BAIRD; 198 pages; $6.00 trade edition; Prentice-Hall, 1962.

Transplantation of Tissues and Cells, edited by R. E. BILLINGHAM and W. K. SILVERS; 149 pages; $7.50; The Wistar Institute Press, 1961.

The Shift and Shape of Spectral Lines by ROBT. G. BREENE, JR.; 323 pages; $15.00; Pergamon Press, 1961.

Argon, Helium, and the Rare Gases, Vol. II The Elements of the Helium Group: Production, Analytical Determination and Uses, edited by G. A. COOK; 423 pages; $17.50; Interscience Publishers, 1961.

Mathematical Statistics by J. E. FREUND; 390 pages; $7.50 text edition; Prentice-Hall, 1962.

Tables of the Individual and Cumulative Terms of Poisson Distribution by DEFENSE SYSTEMS DEPARTMENT, GENERAL ELECTRIC COMPANY; 202 pages; $8.50; D. Van Nostrand Co., 1962.

Transonic Wind Tunnel Testing by B. H. GOETHERT; 397 pages; $17.50; Pergamon Press, 1961.

Treatise on Analytical Chemistry, Part II, Vol. VII: Analytical Chemistry of the Elements, edited by I. M. KOLTHOFF and P. J. ELVING; 567 pages; $20.75; $18.50 subscription price; Interscience Publishers, 1962.

Thermoelectricity: An Introduction to the Principles by D. K. C. MACDONALD; 133 pages; $6.50; John Wiley & Sons, 1962.

Contemporary Botanical Thought by A. M. MACLEOD and L. S. COBLEY; 197 pages; $6.50; Quadrangle Books, 1962.

Researches on Meteorites, edited by C. B. MOORE; 227 pages; $7.00; John Wiley & Sons, 1962.

Elements of Abstract Algebra by J. T. MOORE; 203 pages; $6.50; The Macmillan Co., 1962.

A History of Chemistry, Vol. II by J. R. PARRINGTON; 795 pages; $20.00; St Martin's Press, 1962.

Taxonomy of Setaria (Gramineae) in N. America by J. M. ROMINGER; 132 pages; $4.00 clothbound; University of Illinois Press, 1962.

An Introduction to Fourier Analysis by R. D. STUART; 126 pages; $3.00; John Wiley & Sons, 1962.

The Legacy of Hiroshima by E. TELLER with A. BROWN; 325 pages; $4.95; Doubleday & Co., 1962.

Astronomical Photography (From the Daguerreotype to the Electron Camera) by GERARD DE VAUCOULEURS (translated by R. WRIGHT); 94 pages; 21 plates; $6.00; The Macmillan Co., 1962.

Classical Electrodynamics by J. D. JACKSON; 641 pages; $10.75 college price; John Wiley & Sons, 1962.

Education and Professional Employment in the U.S.S.R. by N. DE WITT; 856 pages; $5.50 from the Supt. of Documents, Government Printing Office, Washington 25, D. C.; National Science Foundation, 1961.

Ultra-Violet and Visible Spectroscopy by C. N. R. RAO; 164 pages; $5.25; Butterworths, 1961.

The Enzymes of Lipid Metabolism, edited by P. DESNUELLE; 308 pages; $10.00; Pergamon Press, 1961.

Crystallization Theory and Practice by ANDREW VANHOOK; 325 pages; $12.50; Reinhold Publishing Company, 1961.

Progress in Nuclear Energy, Reactors, Vol. II, edited by H. R. McK. HYDER; 557 pages; $15.00; Pergamon Press, 1961.

Table and Schematic Chart of Selected Lunar Objects by S. M. KOZIK; 34 pages; $2.00; Pergamon Press, 1961.

A History of Astronomy by A. PANNEKOEK; 521 pages; $9.75; Interscience Publishers, 1961.

The Fabric of the Heavens (The Development of Astronomy and Dynamics), by STEPHEN TOULMIN and JUNE GOODFIELD; 285 pages; $5.00; Harper and Brothers, 1961.

Genetics and Evolution, Selected Papers of A. H. Sturtevant, edited by E. B. LEWIS; 334 pages; $7.50: W. H. Freeman and Company, 1961.

Static Fields in Electricity and Magnetism by D. H. TREVENA; 255 pages; $6.50; Butterworths, 1961.

Science and Technology by ANDREW G. VAN MELSEN; 373 pages; $6.95; Duquesne University Press, 1961.

Principles of Vacuum Engineering by M. PIRANI and J. YARWOOD; 578 pages; $17.50; Reinhold Publishing Company, 1961.

Autoxidation and Antioxidants, Vol. I, edited by W. O. LUNDBERG; 450 pages; $15.50; Interscience Publishers, 1961.

Quantum Mechanics, Vol. I, by ALBERT MESSIAH; 504 pages; $15.00; Interscience Publishers, 1961.

Photometric Titrations by J. B. HEADRIDGE; 131 pages; $7.50; Pergamon Press, 1961.

BOOKS RECEIVED

TO THE MEMBERSHIP: *Our readers are reminded that this section of their journal is intended to be an information service listing first editions of the most important new scientific books received for review in our Princeton Editorial Offices. The listing of paperbacks is being discontinued. In general, the Board of Editors does not encourage the review of symposia.*

The Board of Editors wishes to point out again that members volunteering as reviewers are urged to do so but that a comprehensive statement of professional training and qualifications must accompany a new application.

Advances in Computers, Vol. II, edited by F. L. ALT; 434 pages; $14.00; Academic Press, 1961.

Four Thousand Years Ago by G. BIBBY; 411 pages; $6.95; Alfred A. Knopf, 1961.

Control Mechanisms in Cellular Processes, edited by D. M. BONNER; 248 pages; $8.50; The Ronald Press Co., 1961.

Elementary Organic Chemistry by E. CAMPAIGNE; 312 pages; $10.00 trade edition; Prentice-Hall, 1962.

General Cytochemical Methods, Vol. II, 1961, edited by J. F. DANIELLI; 297 pages; $10.00; Academic Press, 1961.

Polyelectrolyte Solutions, A Theoretical Introduction by S. A. RICE and M. NAGASAWA; 568 pages; $16.50; Academic Press, 1961.

Advances in Quantum Electronics, edited by J. R. SINGER; 641 pages; $15.00; Columbia University Press, 1961.

Molecular Orbital Theory for Organic Chemists by A. STREITWIESER, JR.; 489 pages; $14.50; John Wiley & Sons, 1961.

A World Monograph of the Genus Pleospora and Its Segregates by L. E. WEHMEYER; 451 pages; $15.00; The University of Michigan Press, 1961.

Makers of North American Botany by H. B. HUMPHREY; 265 pages; $6.00; The Ronald Press Co., 1961.

Quantum Theory, Vol. II: Aggregates of Particles; 475 pages; $11.00; Vol. III: *Radiation & High Energy Physics;* 402 pages; $10.00, edited by D. R. BATES; Academic Press, 1962.

Applied Optics, edited by J. N. HOWARD; new bimonthly journal; $6.00 to members of the Optical Society; $8.00 to members of other societies of Amer. Institute of Physics; $10.00 to nonmembers and libraries; single copy $2.00; Optical Society of America publishers, 1155 16th St., N.W., Washington 6, D. C., 1962.

Captives of the Sun by J. S. PICKERING; 326 pages; $4.95; Dodd Mead & Co., 1961.

Principles & Applications of Electromagnetic Fields by R. E. PLONSEY and R. E. COLLIN; 554 pages; $12.75; McGraw-Hill Book Co., 1961.

Seventeen Century Science and the Arts, edited by H. H. RHYS; 137 pages; $3.00; Princeton University Press, 1961.

A Classification of Living Animals by LORD ROTHSCHILD; 106 pages; $4.75; John Wiley & Sons, 1961.

Synthesis of Molecular & Cellular Structure, edited by D. RUDNICK; 252 pages; $9.00; The Ronald Press Co., 1961.

Elements of Queueing Theory with Applications by T. L. SAATY; 423 pages; $11.50; McGraw-Hill Book Co., 1961.

Automatic Control Engineering by F. H. RAVEN; 402 pages; $11.50; McGraw-Hill Book Co., 1961.

The American College (A Psychological and Social Interpretation of the Higher Learning), edited by N. SANFORD; 1084 pages; $10.00; John Wiley & Sons, 1961.

Introduction to Space Dynamics by WM. T. THOMSON; 317 pages; $11.50; John Wiley & Sons, 1961.

Bird-Song (The Biology of Vocal Communication and Expression in Birds) by W. H. THORPE; 143 pages; $3.75; New York: Cambridge University Press, 1961.

Partial Differentiation by H. A. THURSTON; 160 pages; $7.50; Prentice-Hall, 1961.

Cytology by G. B. WILSON and J. H. MORRISON; 297 pages; $7.50; Reinhold Publishing Corporation, 1961.

Methods of Experimental Physics, Vol. V: Nuclear Physics, Part A, edited by L. C. L. YUAN and CHIEN-SHIUNG WU; 733 pages; $18.00; Academic Press, 1961.

Lectures on the Whole of Anatomy, An Annoted Translation of Prelections Anatomiae Universalis by William Harvey, translated from the Latin by C. D. O'MALLEY *et al.;* 239 pages; $8.00; University of California Press, 1961.

Stability by Liapunov's Direct Method with Applications by J. LASALLE and S. LEFSCHETZ; 134 pages; $5.50; Academic Press, 1961.

Moment Distribution by E. LIGHTFOOT; 363 pages; $11.00; John Wiley & Sons, 1961.

Mathematics, A Cultural Approach by M. KLINE; 701 pages; $7.75; Addison-Wesley Publishing Co., 1962.

Basic Principles of the Tracer Method by C. W. SHEPPARD; 282 pages; $8.00; John Wiley & Sons, 1962.

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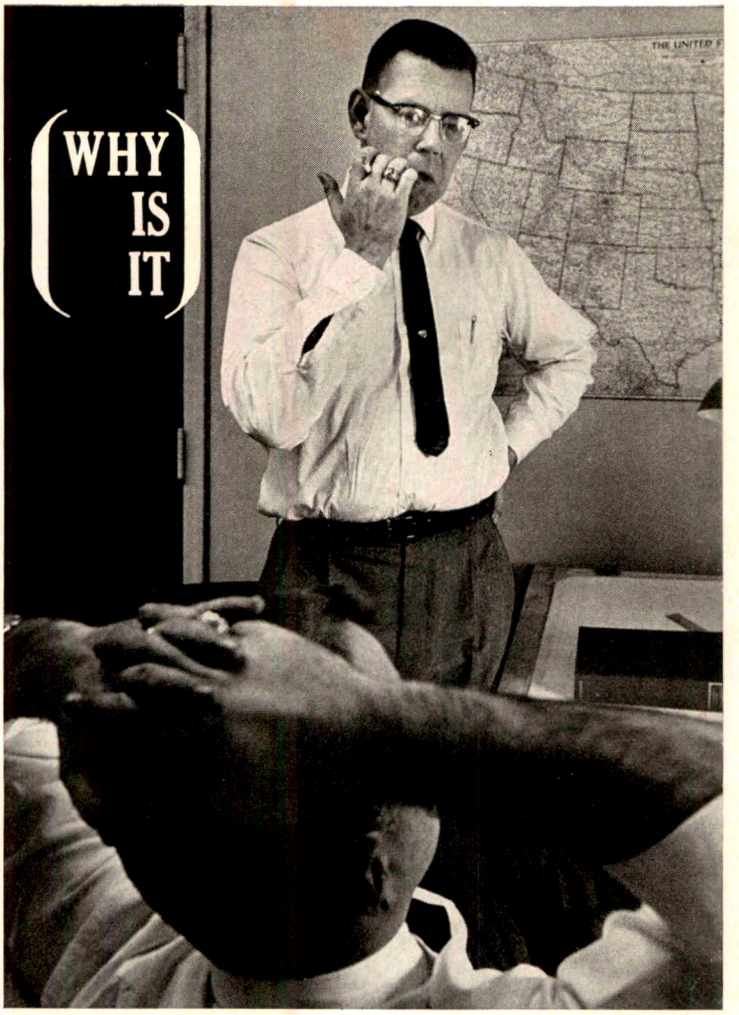
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Random Processes by MURRAY ROSENBLATT; 208 pages; $6.00; Oxford University Press, 1962.

Mathematical Method in Analytical Economics by JACOB T. SCHWARTZ; 282 pages; $14.50; Gordon and Breach Science Publishers, 1961.

The Inspiration of Science by SIR GEORGE THOMSON; 150 pages; $4.00; Oxford University Press, 1961.

Electromagnetic Waves, edited by RUDOLPH LANGER; 396 pages; $6.00; University of Wisconsin Press, 1962.

From C. E. Merrill Books, Inc.: *Introduction to Circuit Analysis* by J. D. COWAN, JR. and H. S. KIRSCHBAUM; 312 pages; $7.95; *Linear Network Theory* by C. D. FERRIS; 524 pages; $9.75; *A Modern Introduction to Organic Chemistry* by WM. B. SMITH; $6.95; *An Introduction to the Mechanics of Deformable Bodies* by M. STIPPES *et al.*; 436 pages; $9.50.

Cerebral Sphingolipidoses (A Symposium on Tay-Sachs' Disease and Allied Disorders), edited by S. M. ARONSON and B. W. VOLK; 456 pages; $18.00; Academic Press, 1962.

Scientific Method: Optimizing Applied Research Decisions by R. L. ACKOFF with S. K. GUPTA and J. S. MINAS; 464 pages; $10.25; John Wiley & Sons, 1962.

Differential Equations by H. S. BEAR, JR.; 207 pages; $7.50; Addison-Wesley Publishing Co., 1962.

Plant Taxonomy (Methods and Principles) by L. BENSON; 494 pages; $11.50; The Ronald Press Co., 1962.

The Various Aspects of Biology by C. E. B. BREMEKAMP; 199 pages; 15 guilders; North-Holland Publishing Co., Box 103, Amsterdam, Holland, 1962.

Superconductive Devices by J. W. BREMER; 184 pages; $8.00; McGraw-Hill Book Co., 1962.

Rock-Forming Minerals, Vol. I: Ortho- & Ring Silicates by W. A. DEER *et al.*; 333 pages; $15.50; John Wiley & Sons, 1962.

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Functions of a Complex Variable and Some of Their Applications, Vol. II, by B. A. FUCHS and V. I. LEVIN translated by J. BERRY; 286 pages; $7.00; Addison-Wesley Publishing Co., 1962.

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Invitation to Mathematics by WM. GLENN and D. A. JOHNSON; 373 pages; $4.95; Doubleday & Co., 1962.

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Physics and Astronomy of the Moon, edited by Z. KOPAL; 538 pages; $16.50; Academic Press, 1961.

Network Analysis and Synthesis by F. F. KUO; 413 pages; $9.25; John Wiley & Sons, 1962.

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The Moon, A Russian View, edited by A. V. MARKOV; 391 pages; $8.00; The University of Chicago Press, 1962.

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Alkyd Resin Technology (Formulating Techniques and Allied Calculations) by T. C. PATTON; 197 pages; $9.75; John Wiley & Sons (Interscience Division Manual No. 8), 1962.

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Mathematical Methods for Science Students by G. STEPHENSON; 494 pages; $7.75; John Wiley & Sons, 1962.

Moving Field Radiation Therapy by F. WACHSMANN and G. BARTH; 265 pages; $10.95; The University of Chicago Press, 1962.

On the External Characters of Minerals by A. G. WERNER, translated by A. V. CAROZZI; 118 pages; $4.50; University of Illinois Press, 1962.

Progress in Polarography by P. ZUMAN with I. M. KOLTHOFF; Vol. I, 355 pages; $12.00; Vol. II, 450 pages; $15.00; John Wiley & Sons (Interscience Division), 1962.

Radio Surface Wave by H. M. BARLOW and J. BROWN; 200 pages; $6.75; Oxford University Press, 1962.

Theory of Molecular Excitons by A. S. DAVYDOV, translated from the first Russian edition by M. KASHA and M. OPPENHEIMER, JR.; 174 pages; $7.95; McGraw-Hill Book Co., 1962.

Numerical Methods for Scientists and Engineers by R. W. HAMMING; 411 pages; $11.00; McGraw-Hill Book Co., 1962.

Alcoholism and Society by M. E. CHAFFTZ and H. W. DEMONE, JR.; 319 pages; $6.95; Oxford University Press, 1962.

American Scientists and Nuclear Weapons Policy by R. GILPIN; 352 pages; $6.95; Princeton University Press, 1962.

Physical Principles and Applications of Junction Transistors by J. H. SIMPSON and R. S. RICHARDS; 519 pages; $10.10; Oxford University Press, 1962.

The Structure and Function of Skin by WM. MONTAGNA; 454 pages; $16.50; Academic Press, 1961.

Linear Programming by G. HADLEY; 520 pages; $9.75; Addison-Wesley Publishing Co., 1962.

The Excitement of Science by J. R. PLATT; 174 pages; $3.50; Houghton Mifflin Co., 1962.

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Great Chemists, edited by E. FARBER; 1642 pages; $29.50; Interscience Publishers, 1962.

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Radiation and Waves in Plasmas, edited by M. MITCHNER; 156 pages; $4.50; Stanford University Press, 1961.

Crystallometry by P. TERPSTRA and L. W. CODD; 420 pages; $12.00; Academic Press, 1961.

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Alkali Metal Dispersions by I. FATT and M. TASHIMA; 228 pages; $7.50; D. Van Nostrand Co., 1961.

The Chemical Basis of Medical Climatology by G. PICCARDI; 146 pages; $6.25; Charles C Thomas, 1962.

An Introduction to the Theory of Linear Spaces by G. SHILOV, translated from the Russian by R. A. SILVERMAN; 310 pages; $10.00 trade edition; Prentice-Hall, 1961.

Advances in Geophysics, Vol. VIII, edited by H. E. LANDSBERG and J. VAN MIEGHEM; 392 pages; $13.00; Academic Press, 1961.

Gyrodynamics and Its Engineering Applications by R. N. ARNOLD and L. MAUNDER; 484 pages; $14.00; Academic Press, 1961.

Perceval's Narrative (A Patient's Account of His Psychosis, 1830–1832), edited by G. BATESON; 331 pages; $6.75; Stanford University Press, 1961.

Introduction to Hilbert Space by S. K. BERBERIAN; 206 pages; $6.50; Oxford University Press, 1961.

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Monogenetic Trematodes (Their Systematics & Phylogeny) by B. E. BYCHOWSKY (WM. J. HARGIS, JR., English Editor); 627 pages; $10.00; The American Institute of Biological Sciences, 2000 P St., N.W., Washington 6, D. C., 1961.

Handbook of Vector & Polyadic Analysis by T. B. DREW; 103 pages; $5.50; Reinhold Publishing Corporation, 1961.

Symposia of the Society for Experimental Biology, No. XV, Mechanisms in Biological Competition (University of Southampton, 1960) by the Society for Experimental Biology; 365 pages; $10.00; Academic Press, 1961.

America Faces the Nuclear Age, edited by J. E. FAIRCHILD and D. LANDMAN; 156 pages; $5.00; Sheridan House Inc., 1961.

The World of Teilhard de Chardin, edited by R. T. FRANCOEUR; 208 pages; $6.00; Helicon Press, 1961.

Projective Techniques and Cross-Cultural Research by G. LINDZEY; 339 pages; $6.00; Appleton-Century-Crofts, Inc., 1961.

Reflections on Human Nature by A. O. LOVEJOY; 275 pages; $4.75; The Johns Hopkins Press, 1961.

Crystallization Theory and Practice by A. VAN HOOK; 325 pages; $12.50; Reinhold Publishing Co., 1961.

Table and Schematic Chart of Selected Lunar Objects by S. M. KOZIK; 34 pages; $2.00; Pergamon Press, 1961.

A History of Astronomy by A. PANNEKOEK; 521 pages; $9.75; Interscience Publishers, 1961.

Applied Optics, new bimonthly journal, edited by J. N. HOWARD; consult The Optical Society of America, 1155 16th St., N.W., Washington 6, D. C., for subscription details.

Darwin and the Naked Lady by A. COMFORT; 174 pages; $4.00; George Braziller, 1962.

Imagination (A Psychological Critique) by JEAN-PAUL SARTRE, translated by F. WILLIAMS; 162 pages; $3.95; The University of Michigan Press, 1962.

Life and Energy by I. ASIMOV; 380 pages; $4.95; Doubleday & Co., 1962.

College Algebra by C. H. LEHMANN; 432 pages; $5.95; John Wiley & Sons, 1962.

Rhythmic Activity in Animal Physiology and Behaviour by J. L. CLOUDSLEY-THOMPSON; 236 pages; $6.80; Academic Press, 1962.

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Introduction to Chemical Instrumentation by E. J. BAIR; 349 pages; $10.75; McGraw-Hill Book Co., 1962.

An Introduction to Elementary Particles by W. S. C. WILLIAMS; 406 pages; $11.00; Academic Press, 1961.

Discrete Variable Methods in Ordinary Differential Equations by P. HENRICI; 407 pages; $11.50; John Wiley & Sons, 1962.

Irritation and Counter-Irritation by A. D. JONAS; 368 pages; $7.50; Vantage Press, 1962.

Biophysics of Physiological and Pharmacological Actions, edited by A. M. SHANES; 612 pages; $13.50; A.A.A.S.,

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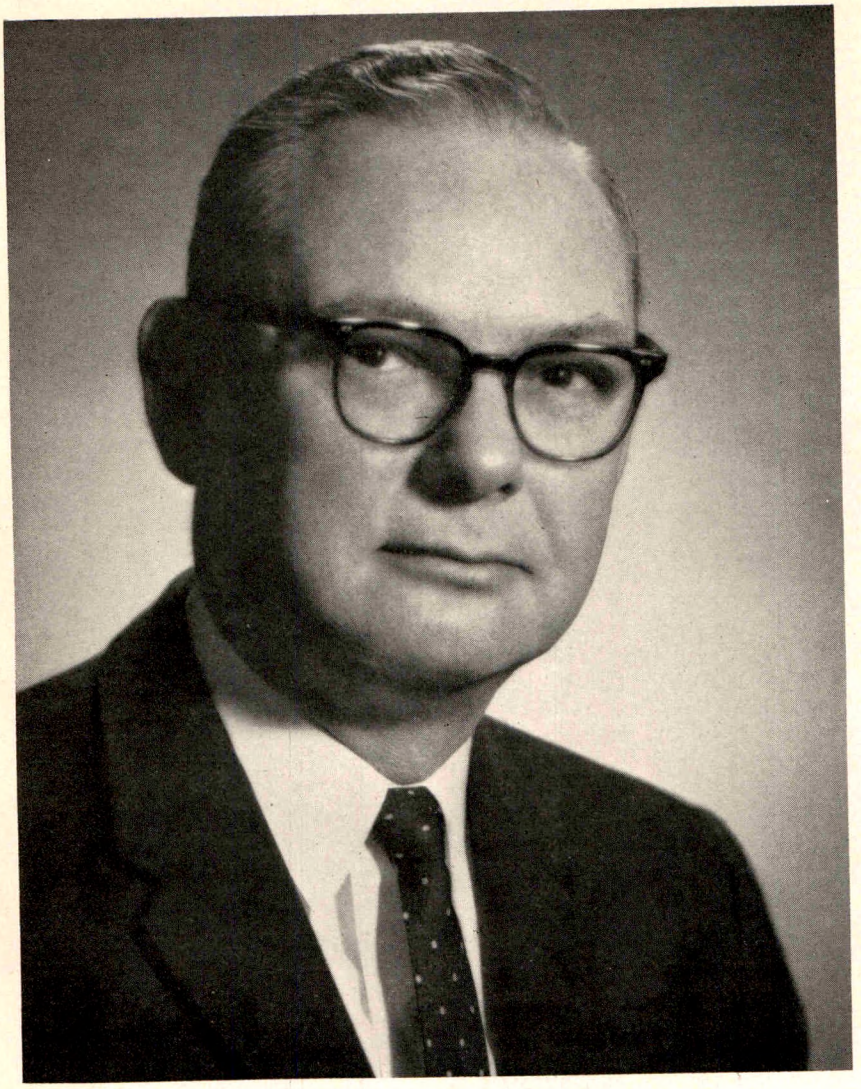
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1515 Massachusetts Ave., N.W., Washington 5, D. C., 1961.

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AMERICAN SCIENTIST

SUMMER • JUNE 1962



CARBON—AN OLD BUT NEW MATERIAL*

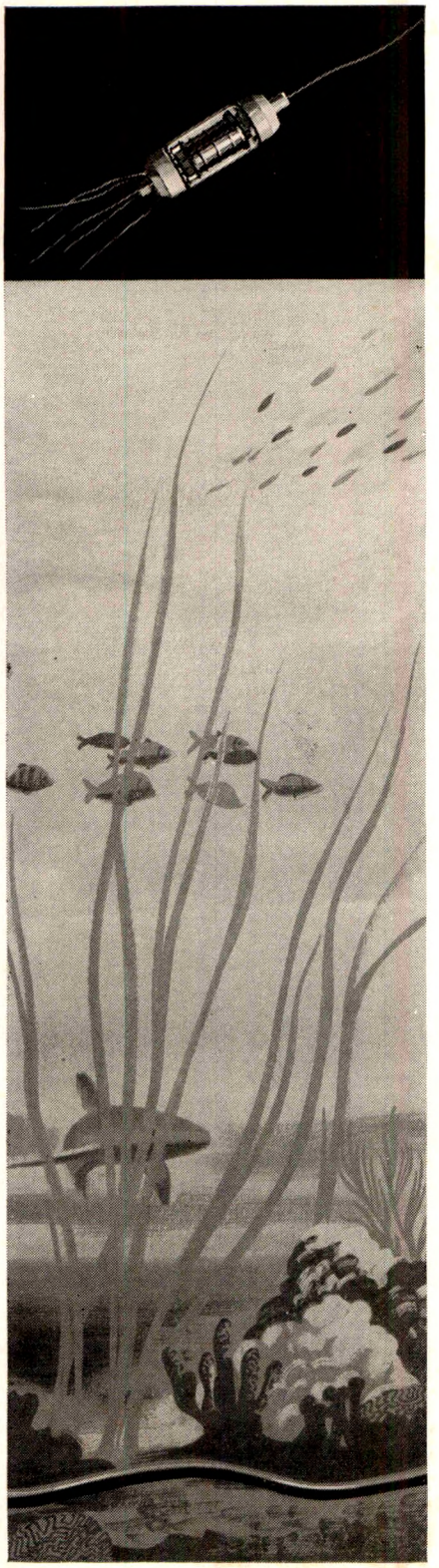
By P. L. WALKER, JR.

THIS MIGHT well be called the age of materials science. The rate of our progress in such diverse fields as space exploration, atomic energy, and the conversion of heat into electrical work depends to a large extent on the development of new materials (and old materials with new properties). Many eminent scientists feel that the most important achievement in science during the last decade is the development of the transistor. This development, more than anything else, made possible the startling advances in the field of computers, which are of such importance today in our defense and technological programs. The successful development of transistors really hinged upon the development of suitable semiconductor materials—materials of ultra high purity and large crystal size. This was a problem in materials science!

Many of the problems in materials science today focus upon the development of materials with radically improved high temperature properties. In space technology, the need of materials for missiles showing greater strength, thermal shock resistance, ductility, and oxidation resistance is critical. With the increasing interest in operating nuclear reactors at high temperatures to improve efficiency, a whole new materials problem has arisen—that of developing materials which show good volume stability under high temperature neutron irradiation. Progress which will be made in the efficient conversion of heat to electricity by means of the thermoelectric effect, thermionic emission, the fuel cell, and magnetohydrodynamics is tied inseparably to advances in the development of better materials.

With the need for development of more and better materials becoming increasingly apparent, there has been a great upswing in materials sci-

* A Sigma Xi-RESA National Lecture, 1961–1962.



50,000,000 tube hours... an unusual electron tube still keeps undersea voice signals strong

Deep on ocean floors, from North America to Europe, between Key West and Havana, Florida and Puerto Rico, under the Pacific to Hawaii and Alaska—in 20,000 miles of undersea telephone cable—a special kind of electron tube is setting a remarkable record for reliability.

This four-inch-long electron tube was designed, developed and fabricated at Bell Telephone Laboratories to operate with no attention for 20 years or more. It is part of the submarine cable repeater manufactured by Western Electric which faithfully and reliably amplifies voice signals transmitted along undersea coaxial cables.

All of the 1608 tubes built into the repeaters have operated to date without failure for a total of over 50,000,000 tube hours, or an average of three-and-a-half years. The oldest have been in service since the first deep-sea repeatered telephone cable was laid 12 years ago.

Years before it was put to use, Bell Laboratories scientists and engineers began developing this undersea tube, another example of forward-looking technology that has made the Bell Telephone Laboratories the world center of communications research and development.

Bell Telephone Laboratories

ence research in many laboratories. In addition, universities are accelerating the training of graduate students in materials science. Studies are in progress on the preparation and examination of materials consisting of individual elements or combinations of elements from most of the periodic table. One element receiving much new research attention today is carbon. This is not surprising, even though carbon materials have been known and used extensively for many years. Carbon has one of the lowest vapor pressures of any element; it sublimes at atmospheric pressure around 3350°C. [1] and melts around 3700°C. At temperatures above 2000°C., carbon has unusually good thermal and mechanical properties. Bodies and shapes made from carbon are comparatively easy to fabricate. Furthermore, carbon is in abundant supply and, hence, relatively cheap.

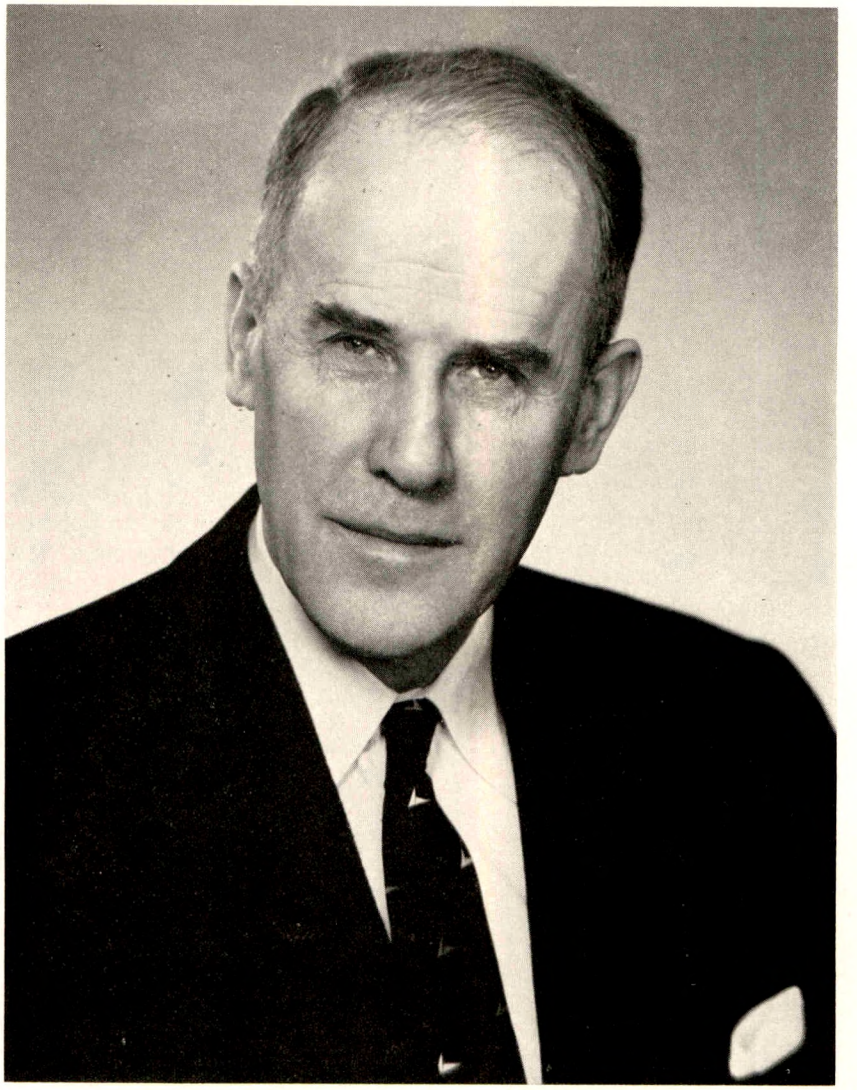
I will first consider the ideal crystal structure of carbon (graphite) and then the deviations from this structure as found in practice. Next, pertinent electrical and thermal properties of the carbon crystal will be reviewed briefly. I will then discuss different methods of preparing carbons which yield materials of widely different and interesting properties.

CRYSTAL STRUCTURE OF GRAPHITE

Carbon exists in two crystalline forms, diamond and graphite. In an early application of X-ray diffraction, it was shown that, in diamond, each carbon atom is covalently bonded to four other carbon atoms arranged tetrahedrally [2]. The carbon-carbon bond distance is 1.54 Å. The extreme hardness of diamond is the direct result of such a configuration, since rupture of this isotropic structure would require the breaking of many strong bonds. Diamond has no utility at atmospheric pressure at temperatures above 1500°C., because of its rapid transformation to graphite [3]. Diamond, of course, has many important uses at low and moderate temperatures.

The recent successful growth of synthetic diamonds from graphite at pressures of about 85,000 atmospheres and at temperatures of about 1600°C. [4] makes, in itself, an epic story in materials science. Possibly a bigger story is now in the process of being told—that is, the production of diamonds which have semiconducting properties [5]. Diamond will not be considered further in this article.

Ideal Graphite Structure: Bernal [6] in 1924 proposed the now accepted stable hexagonal lattice, which is illustrated in Figure 1, as the structure of ideal graphite. The structure is composed of a system of infinite layers of fused hexagons. Within each layer, the carbon-carbon bond distance is 1.415 Å., a spacing indicative of one-third double-bond character [7]. Each carbon atom has four valence electrons. Three of these electrons (usually called σ electrons) are engaged in forming regular covalent bonds with neighboring carbon atoms. These electrons are localized as



WALLACE R. BRODE, *President*,
The Society of the Sigma Xi, 1961–1962

are the electrons in diamond. The fourth electron (usually called a π electron) resonates between many valence-bond structures, effectively giving each carbon-carbon bond a one-third double-bond character.

The individual layers are held parallel at a separation of 3.3538 Å. (15°C.) by weak van der Waals' type forces. This spacing has been carefully measured, using X-ray diffraction [8], and closely confirmed [9] on crystalline, natural graphite from Ceylon. In precise work, it is necessary to quote the temperature at which the interlayer spacing is given because of the large coefficient of thermal expansion of graphite perpendicular to the layer planes [8,10]. The thermal expansion of graphite parallel to the layer planes is very small [8].

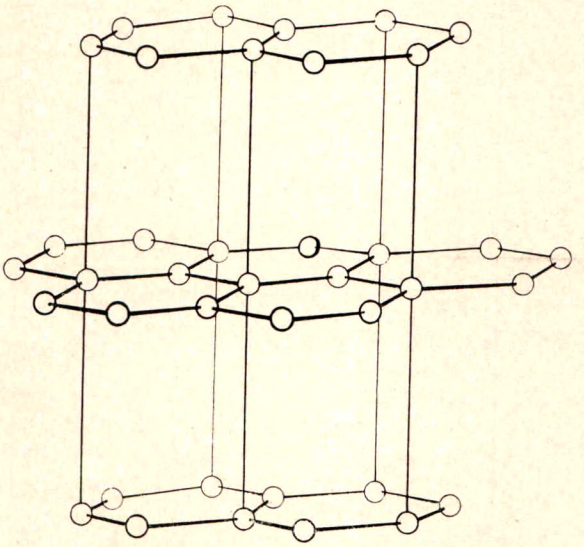


FIG. 1. The arrangement of carbon atoms in the graphite crystal.

The structure shown in Figure 1 is the so-called hexagonal form of graphite. The carbon layers form a stacking sequence (ababab), where one-half of the carbon atoms in any one layer lie above the center of the hexagons in the layer immediately below. In alternate layers, the carbon atoms are directly superimposed over each other. Although this is the predominant layer stacking in graphite, in some cases a fraction of rhombohedral structure having a stacking sequence (abcabcabc) is found [11]. This structure was proposed in 1942 to account for additional reflections found in X-ray diffraction studies on natural graphites. It is metastable with respect to the hexagonal form, from which it may be produced by fine grinding [12] and to which it reverts by heating to around 1300°C.

Deviations from the Ideal Graphite Structure: Deviation from the ideal graphite structure among most carbons of commercial interest is the

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THE GROWTH OF SCIENCE AND A NATIONAL SCIENCE PROGRAM

By WALLACE R. BRODE

THE SOCIETY of the Sigma Xi, which was organized seventy-five years ago to recognize accomplishment and promise in scientific research, celebrates this year its Diamond Jubilee. On this occasion its members and the scientific community would do well to consider the future growth of science and the effect of growth, or growth limitations, on our science programs. The world looks forward to new developments in science and technology in the hope that there may evolve a greater cultural advance, strength and efficiency.

In this discussion of the growth of science and the influence of this growth on our national science policies there will be considered three main areas. The first portion concerns the expected growth of science as measured by personnel, financial support and facilities. We will compare our growth with that of other nations and with the changes in emphasis in various cultural fields. We will develop the concept that we are approaching an equilibrium ceiling which will limit our rate of advance to essentially the expansion rate of our population.

Our second subject to be considered will be the changing sources of support for sciences from federal, state and private agencies, with a corresponding change in responsibility for administration and planning. Lastly we will consider the need for specific mission study so as to realign the responsibilities for the planning, support and direction of our science programs.

A NATIONAL SCIENCE PROGRAM

A plan for a National Science Program should consider, not only the organization of science in this country, but its coordination with broad world programs and those of other nations. It is apparent that the major

rule rather than the exception. The most important deviation is the presence of stacking disorders between the layer planes in carbon. Pioneering studies of Franklin [13] and Bacon [14] have shown that, in different carbons, the fraction of stacking disorders can vary from one to essentially zero (that is, ideal graphite). In the case of a stacking disorder, adjacent layer planes, while still parallel, are randomly rotated

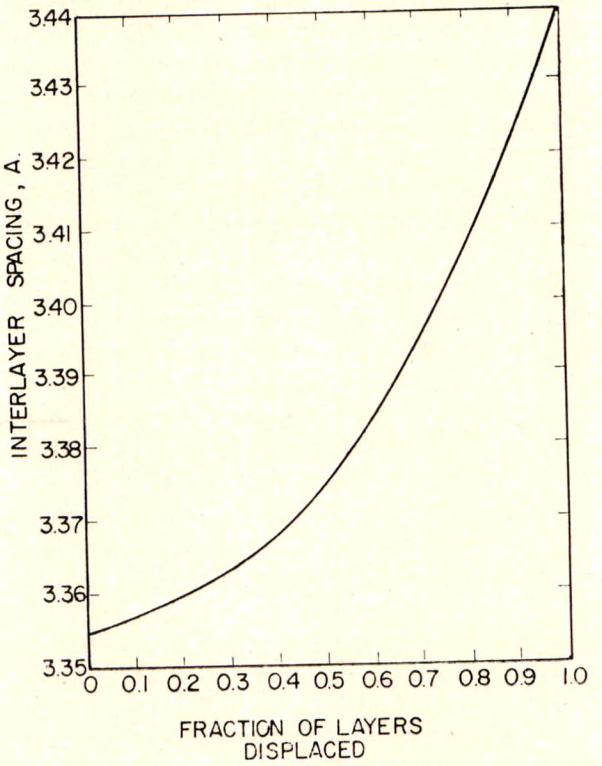


FIG. 2. Variation in the fraction of stacking disorder with mean interlayer spacing [13].

so that there is no longer a fixed relation between the location of the carbon atoms in the two planes. Variations in the fraction of stacking disorder (p) are accompanied by variations in the measured spacing between layer planes, as shown in Figure 2. The measured interlayer spacing increases from 3.354 Å. (ideal graphite) to 3.440 Å. for a carbon having complete stacking disorder. The latter material is now called turbostratic carbon [15]. According to Bacon, there are four different spacings which can exist in a stack of carbon layer planes: (1) between two oriented layers, 3.354 Å.; (2) between two disoriented layers each of which has a disoriented layer on each side of it, 3.440 Å.; (3) between two disoriented layers, one of which is followed by an oriented layer, 3.408 Å.; and (4) between two disoriented layers each of which is fol-

support for science in the future in our country must come from the Federal Government, but these funds and their administration need to be coordinated with state, private and industrial activities so as to conserve our limited resources in the future and insure an efficient production [1, 2, 3].

A National Science Program should involve among its early considerations a clarification of the missions of government agencies, universities and industrial activities. There should be studies to determine how and in what order and manner the functions or agencies should be regrouped, strengthened, reduced or even eliminated so as to provide a stronger, more efficient and more logical national and international science programs [1, 2, 3, 4].

Before we make firm commitments to major programs or organization reforms in our own national science program, it would be well to assess our requirements and capabilities so as logically to allocate our efforts. These studies must include the growth of science, the improvement of quality, the nature of support of our education and research programs, the missions of supporting agencies, and the proper coordination and direction of the research and development programs. The gradual assumption by the federal government of the financial responsibility for an increasing amount of the nation's scientific research and development programs justifies a leadership by the government in the evaluation of our national program. There have been suggestions that an evaluation commission be established as a Science Survey Commission, to operate like the Hoover Commission, and which would include in its membership and advisory panels representatives of both scientists and administrators in the major areas and from the major organizations which deal with science and technology [4].

GROWTH OF SCIENCE

Supply of Scientists

As an introduction to the development of a National Science Program we might well consider the future growth of science and the supply of scientists and engineers, so as to indicate the supply limitations, the need for conservant planning, and the logical mission of our future research and development programs.

EDUCATION

By reason of the available census figures and the known population growth rates, it is possible to predict with reasonable accuracy the future expansion of our elementary educational programs. In the United States, essentially all of the school age population (ages 6 to 14) go to the elementary schools, and hence there is no problem of increasing the proportion attending. The growth rate today of our elementary schools is

lowed by an oriented layer, 3.376 Å. The spacings in Figure 2 are those which would be measured from X-ray diffraction patterns of carbons (see below) and are a result of the presence of varying amounts of the four spacings proposed by Bacon.

The presence of stacking disorders between the layer planes of carbon is a result of the fact that the layers have a finite size. That is, carbon is composed of crystallites having a range of both heights (a varying number of layer planes per crystallite) and layer diameters. The average crystallite size in carbons varies widely—all the way from a low of 10 Å. for some carbon blacks and activated carbons to a high of 10^8 Å. for some natural and special synthetic graphites. It is suggested [16] that the ordering of layers in a crystallite first commences (that is, p becomes <1.0) when the crystallite diameter reaches about 150 Å. Valence bonds between neighboring crystallites tend to stabilize the layers in rotated positions, while the attractive forces between the layers tend to align them. The latter force increases as the square of the diameter, and evidently becomes the determining factor for crystallite diameters above 150 Å. The major decrease in the fraction of displaced layers occurs up to an average crystallite diameter of about 2000 Å. At this diameter, $p = 0.05$ [14].

Other deviations from the ideal graphite structure can be considered briefly. Carbons of small crystallite size frequently have a fraction of disorganized material present. This material consists, in part, of aliphatic groups attached to the edges of layer planes. The material can be responsible for the cross-linking between crystallites.

Well-known solid-state defects like twin lines [17], edge dislocations [18], screw dislocations [19], and vacancies [20] have been reported in graphite. Dislocations are observed by transmission electron microscopy on ultrathin sections of graphite. Vacancies are produced in appreciable concentrations when graphite is irradiated with neutrons or other high energy particles, which cause displacement of carbon atoms from their normal sites within a layer plane. The displaced atoms usually reside in interstitial sites between the layer planes. Vacancies in natural graphite have also been produced by rapidly quenching it from temperatures around 2500°C. [21]. The quenched graphite was unusually reactive to oxidizing gases, with substantial pitting observed in the layer plane. It thus appears that carbon atoms surrounding a vacancy are highly reactive.

ELECTRICAL AND THERMAL PROPERTIES OF GRAPHITE SINGLE CRYSTALS

As expected from the pronounced difference in bonding within the layers of carbon as compared to that between the layers, the thermal and electrical properties of carbon are highly anisotropic. Much has been

essentially that of our population increase [5, 6, 7]. The major task is, therefore, not to improve attendance but to improve the quality of the educational program and increase the number of qualified teachers, salaries, equipment and available space.

A study of "America's Resources of Specialized Talent" which was

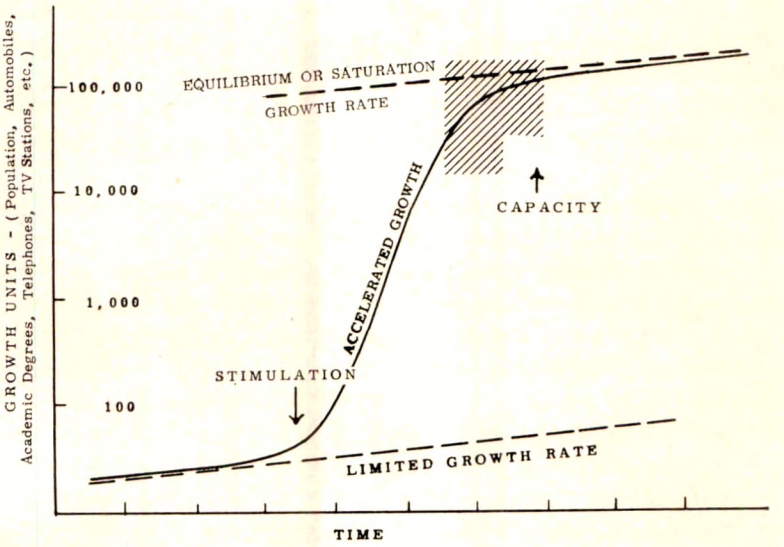


FIG. 1. Simulated case of accelerated growth superimposed on steady growth rate. *Stimulation* point indicates the introduction of a catalytic effect to accelerate acceptance rate, such as popularization, low price mass production, demonstration of essential need. *Capacity* indicates the rise of growth to saturate existing need or ability to absorb. Further growth then becomes dependent on steady growth of absorbing group. See Figure 2 for expanded area typical of situation similar to shaded area.

published under the editorship of Dael Wolfle [8] shows some interesting data with respect to the supply of specialized talent in this nation. During the past fifty years the portion of our youth group which completed high school has risen from about 10% in 1900 to nearly 50% today. In the upper half of the age group, as measured by ability tests, more than 90% now graduate from high school. The portion of the population which does not complete grade school and high school is primarily centered in the lower ability group and essentially all those of higher ability continue through grade school and high school.

While we are concerned with the small per cent of good students who may drop out, we are more concerned with improving the quality of teaching in order to attract the bright student and stimulate him to utilize his highest capabilities. Our losses in numbers of scientists and engineers are more qualitative than quantitative due to inadequate preparatory work in mathematics, languages and elementary sciences.

published on these properties, both experimental and theoretical, and it is not the purpose here to consider this material in any detail. Good general reviews are available [22, 23]. Values of the electrical resistivity and thermal conductivity of graphite single crystals will be considered briefly, since they will serve as reference limits when considering the properties of multicrystalline carbons.

The anisotropy of the electrical resistivity of graphite single crystals exceeds that of any other property. This is clear from the experimental values available, even though there is some disagreement on the extent of the anisotropy because of differences in the crystal perfection of specimens examined. In the direction parallel to the layer plane, there is reasonable agreement that the room temperature resistivity is about 4×10^{-5} ohm cm. [24]. Perpendicular to the layer planes, resistivity values vary from around 1 ohm cm. [25] to 4×10^{-3} ohm cm. [24]. Thus, the ratio of electrical resistivities is at least 100.

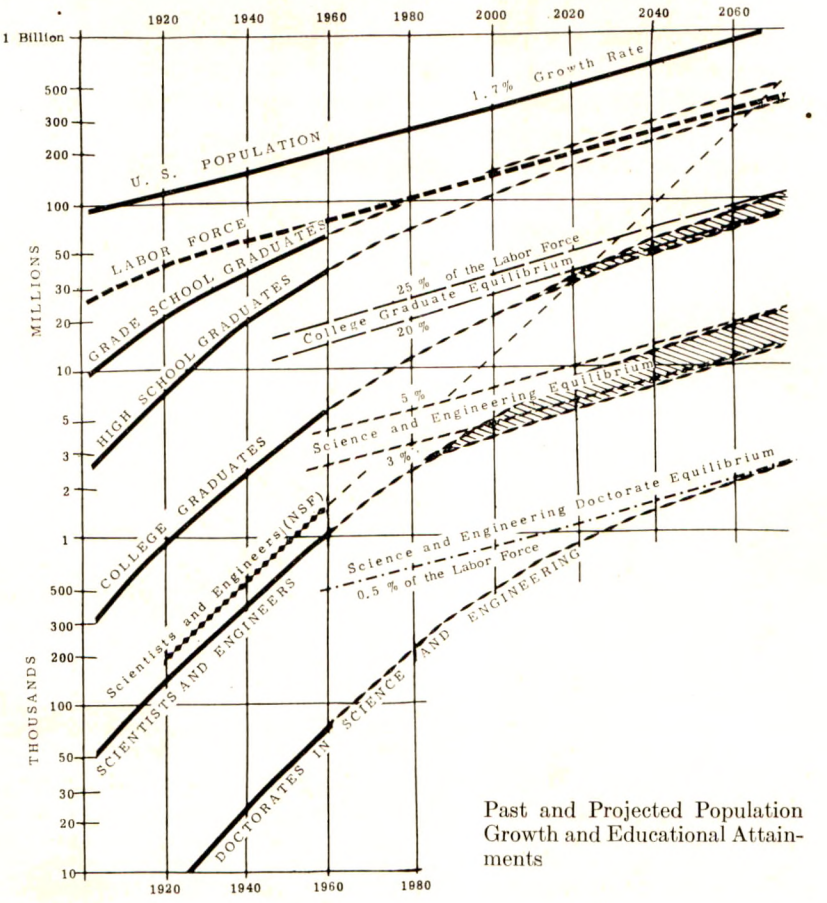
Measurements of thermal conductivities on small graphite crystals are difficult. Nevertheless, there appears to be agreement, from the limited data available, that the room temperature values are about 4.0 and 0.8 watts/cm./°C., respectively, in the directions parallel to and perpendicular to the layer planes [22]. Thus, the ratio of thermal conductivities is about 5.

THE PREPARATION OF CARBONS AND SOME OF THEIR INTERESTING PROPERTIES

Carbons of widely different properties (properties approaching those of ideal graphite more or less closely) can be prepared, by starting with different carbonaceous materials (in the form of solids, liquids, and gases) and treating these materials in various ways. The variation in properties is a result primarily of a variation in carbon crystallite size and orientation. The preparation and properties of both "old" and "new" carbon materials can now be considered.

Bulk Baked and Graphitized Carbon Bodies

History: The carbon body which is familiar to us today appears to have had its beginning in 1800 when Sir Humphry Davy first employed carbon electrodes to produce an electric arc. His electrodes were simply rods cut from carbonized wood. During most of the nineteenth century, the majority of the development work in the carbon industry was directed at producing improved electrodes for arc lighting. In the decade 1890–1900, there came a succession of inventions and developments in the electrothermal and electrolytic field, all of which required baked or graphitized carbon bodies. In 1896, Castner [26] and Acheson [27] found that some turbostratic carbons could be converted into graphitic materials by heating to sufficiently high temperatures—2000°C. and above.



Past and Projected Population Growth and Educational Attainments

FIG. 2. Past and projected future growth of population, labor force and educational attainments, based on data from the past sixty years and known trends in population growth and educational attainments. Shaded areas indicate variations which may result from influences to alter the pattern which has been developed.

Linear projection extension of scientists and engineers (NSF) at the same rate which was used to obtain their 1970 projections [14] indicates that, by about a century from now, all the labor force should be scientists and engineers. This is obviously impossible and hence there must be an approach to an equilibrium ceiling.

Bright students, held down to the level of those who do not have an interest and ability to advance in the scientific and technical areas, are often unprepared on entering college to proceed into a technical program. [9, 10, 11, 12, 13, 14].

Approximately 35% of the persons of college age enter college and about a third of these complete only junior college work or drop out for other reasons prior to graduation. The balance, or about 20% of the persons of college age, graduate from college. While the nation as a whole

This marked the beginning of a new era in which the carbon industry expanded and developed improved baked and graphitized carbons for use primarily in (1) electrolytic manufacture—e.g., of alkalies, chlorine, aluminum, and magnesium; (2) electrothermic production—e.g., of calcium carbide and silicon carbide; and (3) electric furnaces—e.g., for steel, copper, ferroalloys, and phosphorus. Then, in 1942, when Fermi and a group of scientists produced a self-sustaining nuclear-chain reaction, they used graphite as the moderator in their reactor. This opened up a whole new outlet for graphite and at the same time produced materials problems on which much research and development studies are still in progress.

Method of Production of Carbon Bodies: Bulk carbon is basically a composite mixture of carbon particles (called the filler) and a suitable binder. The most common filler is petroleum coke, which today is obtained largely by the cracking of heavy refining oil that produces gasoline, other light fractions, and coke. Usually the binder is coal tar pitch, the heavy residue derived from the distillation of coal tar from by-product coke ovens. The petroleum coke, as received from the oil industry, is first heated in the absence of air (calcined) to about 1300°C. to reduce its volatile matter content and to pre-shrink the coke. Excessive shrinkage of the coke during the baking of the carbon body produces undesirable cracking. The calcined coke (about 100 parts by weight) is ground to the appropriate particle size and mixed thoroughly with melted pitch (about 30 parts by weight) at about 150°C. The mix is then formed into the desired shape by extrusion or molding. The formed body is baked, in the absence of air, to about 1000°C. The function of the baking operation is to convert the pitch from a thermoplastic material to an infusible solid. As the binder is pyrolyzed, large quantities of hydrogen and hydrocarbons are evolved, resulting in the loss of about 40% of the binder weight. Thus, the baked body is composed of about 85% (by weight) of filler carbon and 15% binder carbon.

As we shall see later, the open porosity of baked carbons is typically between 25 and 30%. For some end uses, a material of much lower porosity is necessary. The most common method used to decrease the porosity of a carbon body is to impregnate it with a fluid (frequently a coal tar) which, upon subsequent pyrolysis, deposits secondary carbon in the open voids. A body may be impregnated a number of times, with the impregnant usually being coked after each impregnation.

The gas-baked carbon body is composed of small carbon crystallites. Within these crystallites, the layer planes are all randomly rotated. When the baked body is heated in the absence of air to temperatures between 2500 and 3000°C., the carbon crystallites grow sufficiently to impart substantial stacking order between the carbon layers. The results of "graphitization" include a substantial change in the electrical, ther-

has been expanding at a fairly steady rate of about 1.7% per year in population (see top line in Figure 2) this expansion has been due, in part, to the extension of our life span, to immigration and to an uneven birth rate. The influence of the depression and wars on our birth rate is quite evident from the solid line in Figure 3. This latter curve indicates

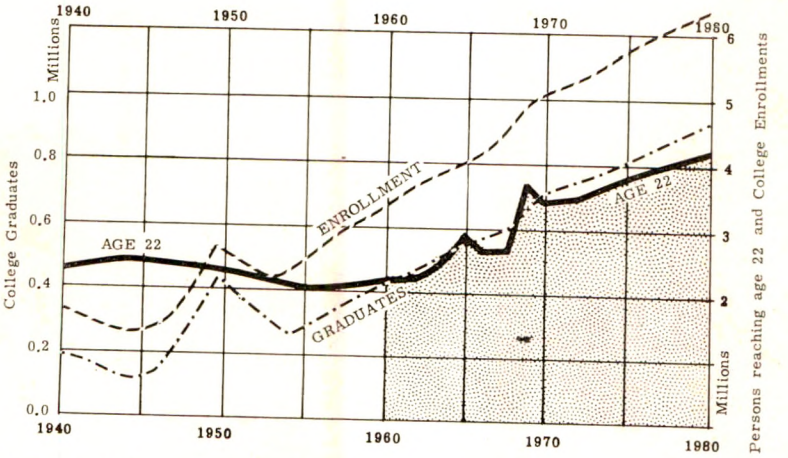


FIG. 3. Annual number of persons reaching age of 22 (College graduation age) number of annual college graduates; and number of enrolled college students completing their sophomore year. Annual graduation predictions derived from average of (a) one fifth of population at age 22 and (b) one seventh of total college enrollment [11, 15].

the annual population of 22-year olds, which is the normal age for college graduation [15]. From these data we can easily predict the probable number of students to be enrolled in our colleges and the number of graduates to be expected each year. It would appear that there is a reasonably close coupling between the number of 22-year olds and the number of college graduates in any one year. It is obvious that a marked break will occur about 1969, but its effect will be spread over a period from 1968 to 1970 and will not be as sharp as the population break. From 1970 to 1980 the population rate of increase of 22-year olds drops off to about 3% as compared with the 5 to 6% rate of expansion in the previous decade 1960–70. However, the college graduate population will probably continue to grow at about 5% per year during most of this decade.

Many who originally feel that they wish to be scientists and engineers transfer into other fields; on the other hand, only a few transfer from other fields into science and engineering. At Northwestern University 90% of the freshmen engineers graduate, but only 50% complete the engineering curricula whereas of the balance, 40%, transfer to other curricula and only 10% withdraw and fail to graduate. This withdrawal rate

mal, and mechanical properties of the carbon body. In addition, a body of much higher purity is produced, because of the liberation of additional hydrogen, sulfur, and metallic impurities. In some cases, the liberation of metallic impurities is enhanced by graphitizing in a halogen atmosphere.

Mechanism of Carbonization and Graphitization: The carbonization and graphitization of the filler and binder phases will first be considered separately and then the effect of interaction of the two phases will be discussed. Consider petroleum coke (the filler) first. When heavy petroleum residues are heated between about 350 and 550°C., they crack,

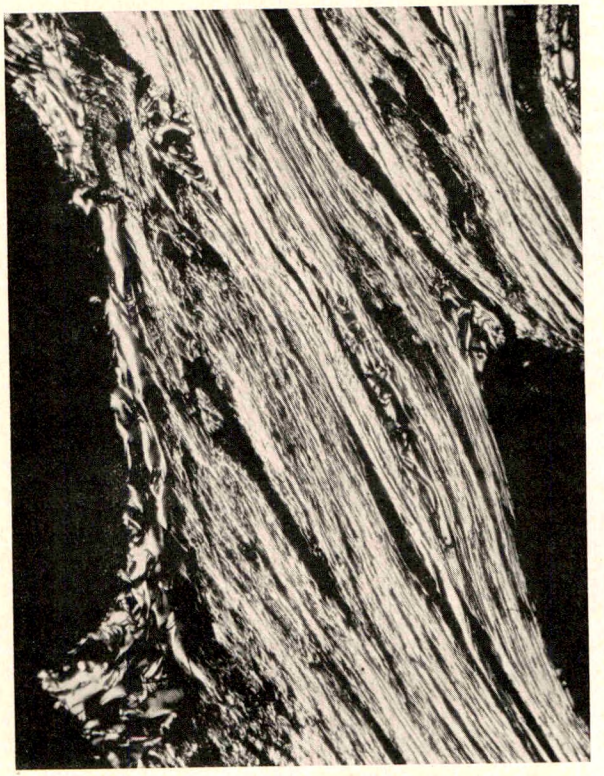


FIG. 3. Texture of a typical calcined petroleum coke, magnification 75× [28].

liberating low molecular weight gases and forming a solid phase—"green" petroleum coke. Petroleum cokes show remarkable flow patterns within the particles, apparently caused by the evolution of gases and aided by the thermal motion of the viscous mass during coking. It is found [28] that large, platelike aromatic molecules are aligned parallel to the flow as a result of shear stresses. During calcination, the aligned structure is preserved (see Fig. 3), although fissures develop and enlarge as additional volatile hydrocarbons are evolved. Calcined petroleum coke is a turbostratic carbon, containing crystallites of about 30 Å. in diameter.

is less than that of the university as a whole and would support the concept that a higher quality of student elects to major in science and engineering.

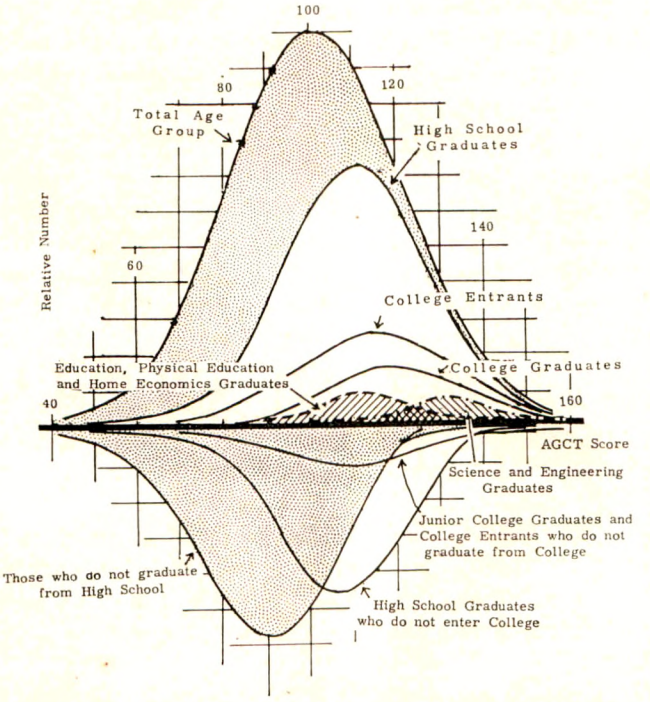


FIG. 4. Distribution of Ability (by Army General Classification Test (AGCT) scores) of all members of a current age group and those reaching different educational levels. Curves below horizontal line indicate character of groups which do not continue. Based on data from Wolfle [7, 8].

For the past fifty years the proportion of our college student group in science and engineering has remained nearly static at about 20% of the graduating class or enrolled students. There has been fluctuation between areas, as influenced by the times; physics may expand at the expense of engineering; biology at the expense of chemistry; but these are internal, and the over-all component of science and engineering remains at a reasonably constant proportion of one-fifth of the student group.

A study of the available data would indicate that we might be able to increase the proportion of the age group graduating as scientists and engineers. An increase to as much as 25% in the portion could only be effected by development of financial grants-in-aid, draft deferment, and improved educational methods so as to attract the good students who fail to go to college, and to avoid the loss of those whose inadequate or improper elementary preparation has inhibited their selection of a scien-

Upon heating above the calcination temperature, crystallites grow slowly up to about 2000°C. and then rapidly. The extent of crystallite growth under given heat treatment conditions is markedly sensitive to the extent of platelet alignment in the "green" coke. The better and more extensive this alignment, the greater is crystallite growth. This fact is taken to indicate that vapor-phase condensation is not the likely mechanism of graphitization since such a process should be nearly independent of coke structure. The mechanism favored [29] for crystallite growth involves the displacement and rearrangement of crystallites relative to each other. The more nearly two crystallites are aligned with their layers parallel to each other, the easier it is for them to rearrange by thermal motion sufficiently to coalesce. This leads to crystallite growth without mass transfer or the work of breaking strong chemical bonds. As will be seen later in a striking example, low-temperature carbons which have their crystallites poorly aligned or strongly cross-linked undergo negligible additional growth, even when heated to 3000°C.

It has been found that the extent of platelet alignment in green petroleum coke is dependent upon at least two factors: (1) the manner of coking and (2) the cleanliness of the charging stock [30]. To attain good alignment, the liquid hydrocarbons should be in a state of substantial quiescence and thermal homogeneity during solidification. The presence of granular precipitates or asphaltenes in the oil during coking is found to increase markedly the rate of coking and prevent good alignment of adjacent platelike molecules.

Consider now the carbonization and graphitization of coal tar pitch (the binder). A typical pitch contains about 90% by weight of aromatic molecules with platelet diameters from 4 to 8 Å. [31]. Upon heating the pitch to between about 450 and 550°C., these molecules lose hydrogen and low molecular weight hydrocarbons and condense into platelets of larger sizes, resulting in the formation of a solid, "green" pitch coke. The extent of crystallite growth upon subsequent heat treatment to graphitization temperatures depends, as is the case with petroleum coke, upon the perfection of crystallite alignment in the green coke. This alignment, in turn, is at least dependent upon (1) the nature of the aromatic compounds in the pitch and (2) the character of the insoluble phase present. Considering the first point, pitch is estimated to contain about 5000 different compounds [31]. These compounds would be expected to yield cokes showing wide differences in their graphitizability. For example, the crystallite growth of cokes produced from anthracene and phenanthrene, two compounds identified in pitch [32], has been studied [33]. These compounds, having the same formula ($C_{14}H_{10}$), are isomers. Upon heat treatment of the cokes to 3000°C., the anthracene- and phenanthrene-derived carbons had average crystallite diameters of about 2000 Å. and 200 Å.. respectively. Considering the second point, pitches

tific or engineering career. It is of course true that there are brilliant students with aptitudes in both science and music, or engineering and law, who must make a career decision. However, the inducements towards science and engineering in the present age make it reasonable to accept a factor of one-fifth of our college graduates (or 4% of those of college age) as that portion of our population which has the ability and desire to major in or to engage professionally in science and engineering.

PROFESSIONAL CAREERS

Figure 4 shows that there are many who are not college graduates who have the same equivalent abilities as those who do complete college and major in such areas as education. There are very few who have the potential ability to major in science and engineering, who do not go to college and graduate. This higher ability group must be shared by all professions, and includes only a very few potential science and engineering majors. Because not all who major in science and engineering enter the profession, our current output consists of only 85 to 90% of the engineering majors and 30% of the science majors, thus providing our professional raw material in science and engineering. This, at the present time, amounts to about 55,000 new persons annually [16].

This number will increase in size as the population of our nation increases, and therefore, each year adds more manpower to our pool of available scientists and engineers. The present pool of scientists and engineers is about 1,100,000 or 1.4% of our working population of 80,000,000.

In our group of about 1 million scientists and engineers, the retirement-death- and transfer rate is about 2% per year. Hence, of 55,000, some 20,000 are required for replacement and the balance provides 35,000 or 3% for growth. We can make reasonably good estimates of the population and age distribution up to the year 2000 and probably for some period beyond (Figs. 1 and 2). By the year 2000 we will have a world population of six billion, of whom some 380 million will be in the United States. On the basis of known relative numbers of students, science majors, growth, and increased efforts to obtain the maximum number of qualified scientists and engineers, we would expect to increase the number of scientists and engineers in this country from the present 1,100,000 or 1.4% of the working group today to about 6,000,000 or about 3% of the working group in the year 2000. We may ultimately increase the proportion of scientists and engineers to 4% of the working group, but as we approach this higher value the rate of increase in total numbers will most certainly approach and parallel the population rate increase of about 2%. The present United States expansion rate of the scientist and engineering profession is 3 to 6% which is considerably less than the world expansion rate of 10 to 12%.

are known to contain varying amounts of insoluble particles. Removal of the particles (in the pitch because of entrainment of solids from the coke oven) substantially enhanced crystallite alignment in the coke subsequently produced [34]. It is concluded that these particles act as nuclei for the coking of aromatic compounds in the pitch and decrease crystallite alignment because of their large surface heterogeneity (the relation between crystallite alignment and surface heterogeneity will be discussed shortly).

There is much concern about the relative extents of crystallite growth of the filler and binder within a carbon body upon graphitization. Some pitches, when heat-treated alone, graphitize as well as petroleum cokes [35]. However, Noda and Inagaki [36] conclude that pitch is less graphitizable when it is heat-treated with an admixture of petroleum coke than when heat-treated alone. They found that graphitized pitch carbon in a

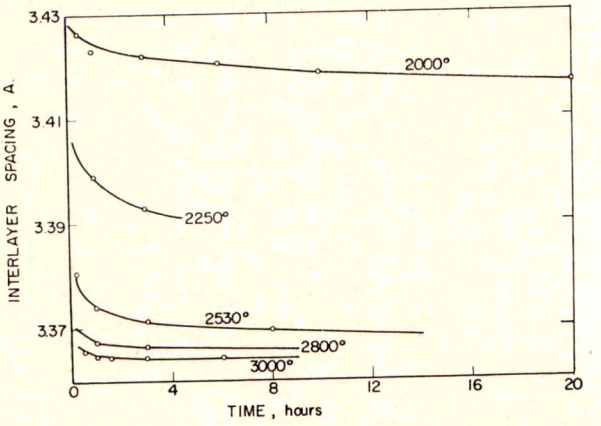


FIG. 4. Studies on kinetics of graphitization of a typical carbon body [37].

body had an interlayer spacing of 3.38 Å. and a crystallite height of 260 Å. By comparison, the pitch carbon graphitized alone had an interlayer spacing of 3.36 Å. and a crystallite height of 340 Å. They concluded that, in a carbon body, pitch exists in a thin film around the filler particles. As such, it has difficulty flowing even when it softens; and, consequently, the platelet groups have difficulty orienting.

Recently, some significant work on the kinetics of graphitization of a baked carbon body between 2000 and 3000°C. has been conducted [37]. Samples were rapidly plunged into the high temperature zone of a furnace (containing argon) and held for varying lengths of time. Interlayer spacings were then accurately measured on the samples. The results are shown in Figure 4. One can see that, at each graphitization temperature, the interlayer spacing decreases rapidly at first, and then more and more slowly, as it approaches (practically speaking) some limiting value. These kinetics are consistent with there being a steady in-

Medicine and dentistry are not included in our definition of science and yet many pre-medical and pre-dental students major in science. Many students, especially in our smaller colleges, are urged to major in science and with additional required education courses if they intend to teach in our high schools. This procedure is preferable to following the college of education pattern of majoring in education and taking a limited, and often very limited, amount of substantive course work in the area to be taught [16, 17].

There are also many who go into business but seek a science back-

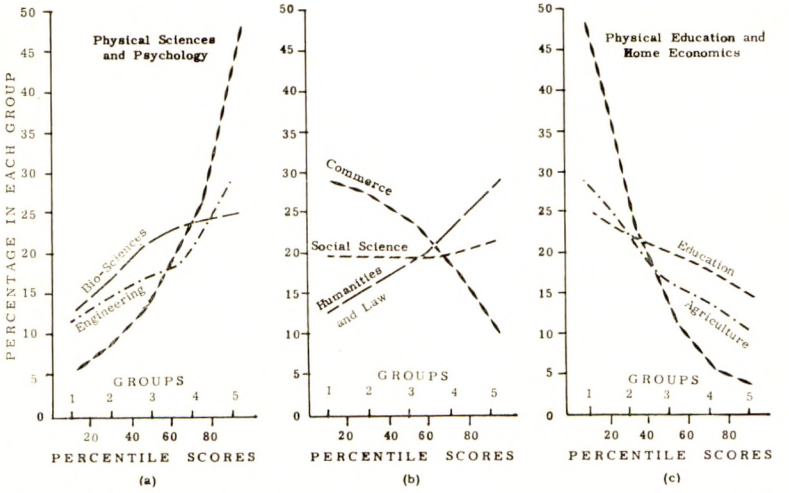


FIG. 5. Relation between fields of specialization and intelligence levels of professionally employed college graduates. (Averaged data from Wolfle—"America's Resources of Specialized Talent" [8].

ground as an undergraduate major; some among the women majors marry, and thus drop out of the technical labor force. The number of science majors who move to non-science careers is nearly 70%. The number of engineering graduates who do not continue in the profession is relatively low being only about 15% and is partially offset by those who by self-study, technical institutes or other means enter the engineering profession without a regular bachelor's degree. The summation of these deviations from direct scientific and engineering careers amounts to about 45% of the total science and engineering graduates. To the extent that these majors go into other creative professions, such as medicine, teaching, law, business or politics, we should be pleased that persons in other areas have a scientific background and appreciation. In some universities there are special courses for those who do not plan to advance professionally in science beyond the bachelor's degree, and the degree obtained is essentially terminal so far as their science program

crease in activation energy for additional crystallite growth with increase in extent of graphitization. Much additional kinetic work needs to be done, not only on the effect of soak time, but also on the effects of heating and cooling rates upon graphitization and upon the resulting electrical, thermal, and mechanical properties of carbon bodies.

It is of interest to consider the extent to which complete three-dimensional ordering has been achieved upon the graphitization of carbon bodies. The interlayer spacing of four different carbon bodies has been accurately measured [9], following heat treatment to 3600°C. under 10 atmospheres of argon pressure. The lowest spacing found was 3.3569 Å. (15°C.), corresponding to a fraction of displaced layers of 0.10. Bacon [14] very carefully studied the extent of crystalline perfection of three artificial graphites. The sample of greatest perfection, which had a fraction of displaced layers of only 0.019, is thought to have reached a temperature of 3200°C. and to have been produced from a coke of unusually good crystallite alignment. Its X-ray diffraction pattern, shown in Figure 5, reveals an unusually large number of diffraction peaks.

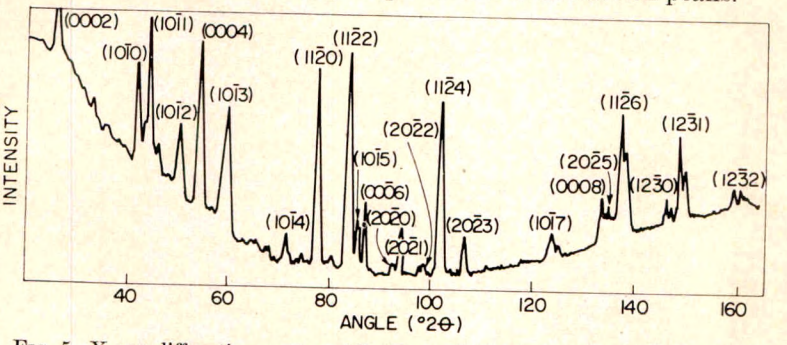


FIG. 5. X-ray diffraction pattern (Cu-Kα radiation) of a highly graphitized artificial graphite [14].

Physical Structure of Carbon Bodies: The density of ideal graphite is 2.268 g./cc., but, in practice, the density of carbon bodies is considerably less than this figure because of porosity between filler particles and between crystallites within both the filler and binder carbon. Figure 6 is a micrograph of a longitudinal section of a 10-inch diameter graphitized electrode [38]. The apparent density of this body is 1.60 g./cc., and its total porosity is about 29%. The voids between filler particles and fissures within filler particles are clearly seen. Figure 7 is a transmission electron micrograph of a thin section cut from nuclear graphite [39]. This micrograph shows a strong resemblance to the familiar grain structure of etched metal specimens. Individual crystallites can be recognized; they have an average layer diameter of about 3000 Å. Porosity between crystallites is clearly in evidence. Area (A) is a pore at the junction of three crystallites. Line MN is a sharp boundary of about 50 Å. width between two crystallites.

is concerned.

ABILITIES AND FIELDS OF SPECIALIZATION

Wolfle [8], in comparing the intelligence distribution of majors in different fields of specialization, reported that only 15% of the physical science majors came from the bottom two-fifths of the college population, while 70% came from the top two-fifths. In the biological and engineering areas, there was not as great a differential although the proportion was in the same direction with nearly twice as many in the upper part of the intelligence distribution as were in the lower part (Fig. 5).

An interesting case was the area of psychology which attracted nearly as many in the lower intelligence distribution as in the upper, in college undergraduates as majors, but among those who elected to do graduate work or remain employed in the field, only 5% were in the lower fifth and 45% were in the top fifth. In other words, psychology was an attractive area for low quality students, but most of these selected the major as a means to a degree and a few in this lower level were able to go on to graduate work or professional status. In contrast to the areas of the sciences, humanities, law, and engineering were the areas of business, home economics, education, physical education and agriculture. In the latter group 30 to 50% of the majors were in the lower fifth and only 5 to 15% in the top two-fifths of the intelligence distribution. These data serve to demonstrate that the science and engineering majors are essentially in the upper portion of the college group and that the college group involves the upper portion of the high school group (Fig. 5a).

The 3% limit of scientists and engineers in our working force (Fig. 2) may be difficult to prove. Essentially, we have to prove a negative theory, that is, with additional inducements more would have studied science and engineering and that these additional students would have abilities equivalent to those who do major. Our feeling is that these additional recruits do not exist as potentially qualified persons, and that we essentially now recruit nearly all of those who have the ability and initiative to become scientists or engineers.

In the Soviet Union, where science and engineering studies are pressed upon potential students with ability, one finds about the same per cent of student enrollment in science and engineering as in our own college age group (about 4% of the college age group). However, the transfer to other curricula or occupations in Russia is much less than the 45% shift of our science and engineering majors to medicine, law, business, or politics. That science is useful as a background for other activities is, however, recognized by the Soviets; about one-third of the members of the Soviet Presidium have had their training in science or engineering.

If we recruit essentially all those who should be scientists and engineers, we would gain more by expanding our efforts to make better scien-

As shown [40], some of the porosity between carbon crystallites is unavailable to helium at room temperature. This is taken to mean that it is blocked off behind openings less than about 2.8 Å. in diameter. The amount of pore volume closed to helium in a variety of calcined petroleum cokes has been measured, along with the relative extent of crystallite alignment from the intensity of the (002) X-ray diffraction peak. It was found that the amount of closed pore volume increases with



Fig. 6. Photomicrograph of a longitudinal section of a graphitized electrode. magnification 10× [38].

a decrease in the extent of crystallite alignment, as shown in Figure 8. The effect of crystallite size on the amount of pore volume closed to helium in selected carbon bodies has also been investigated [41]. It was found that, as the crystallite size decreases, the fraction of pore volume inaccessible to helium increases. It was concluded that the closed pore volume is coextensive with the crystallite surfaces and is the result of small separations between adjoining crystallites. Walker and Geller [42] measured the pore volume closed to helium in a graphitized petroleum

tists and engineers out of our students, rather than to add a few reluctant candidates who, for lack of instruction or funds may have chosen an easier or different course. We should not use our funds and efforts to train those who neither possess the ability nor desire to be scientists on the theory that we need poor scientists as well as good scientists.

From these (Wolfle's) data it would appear that there are some of the age group in the upper ability level who do not go to college. While college graduates are essentially in the upper ability level (Fig. 4), there are some graduates in the lower ability level. There are two explanations for this: (1). Some colleges do not enforce an ability selection so that essentially there are no rejection standards; any high school graduate is admitted and anyone who stays for four years gets a degree [17]. (2) There are some schools who have essentially established "soft spots" in the college or university where those with a low potential and ability can graduate. It is generally recognized that neither engineering nor the sciences serve as such soft spots although there may of course be some exceptions.

It would appear that our scientific and technical advance will be more dependent on developing our bright and capable students, rather than providing a broad and average training to all alike. Separation and priority advancement of our gifted students is an essential step in accomplishing the maximum capability of this nation in science and engineering [13]. For many years it has been recognized that the scientific and engineering professions require as a minimum, a bachelor's degree, and in many areas of science and technology even further advanced study has become essential. In this scientific and technical age, our scientific professions have been promoted and even glamorized to such an extent that we can, with some assurance, feel that the vast majority of the intellectually able students not only complete their high school training but, for the most part, their college training. We should, of course, be concerned that essentially all of our intellectually qualified students go to college. Our hope for the future of sciences and engineering is not materially to increase the per cent of scientists and engineers in our student group, but rather to improve the quality and level of those available.

A problem to be resolved by educators is the high school graduate who should receive additional education but who cannot keep up with the rapidly accelerating pace of the professionally oriented students. It would be expected that a portion of these intermediate-level students who do not now go to or complete college may have an interest and limited ability in science and technology and will provide an essential supply of technicians, mechanics and expert operators.

Limitation of Growth

Science and technology, and the number of scientists and engineers,

coke and in a graphitized carbon body containing the same petroleum coke of identical particle size. They found, by difference, that the amount of closed pore volume per unit weight of binder carbon considerably exceeded that in the filler carbon. On the basis of previous results [41],

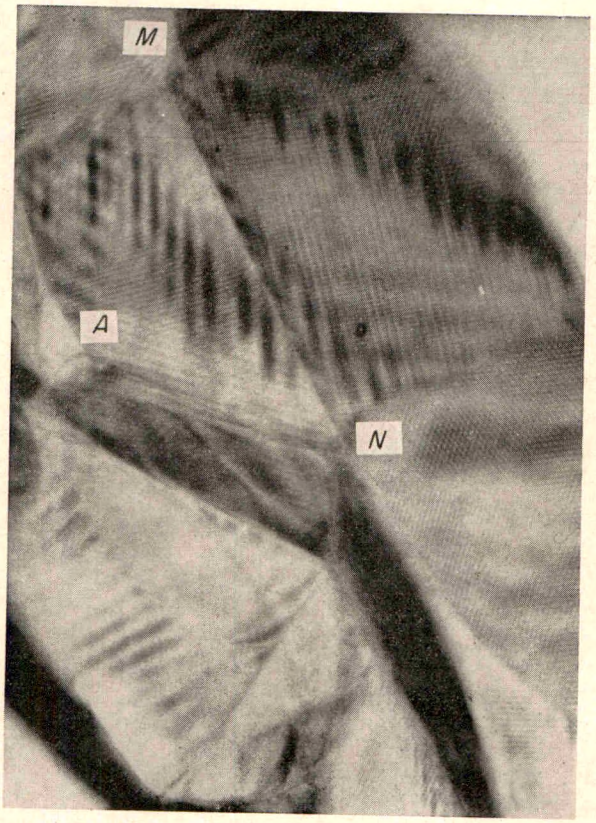


FIG. 7. Transmission electron micrograph of a thin section of nuclear graphite, magnification 170,000× [39].

this also suggests that the crystallite size in the binder carbon is less than that in the filler carbon.

The open porosity in carbon bodies has been studied by gas adsorption and mercury porosimetry [43,44]. The pore volume distribution in voids greater than 400 Å. in radius (usually called the macropore volume) for samples of unimpregnated and impregnated nuclear graphite is shown in Figure 9. For the unimpregnated body, the majority of this porosity is usually located within a fairly narrow range of pore sizes. This volume is created by the packing of filler particles. As is seen, impregnation primarily reduces this volume. Following seven impregnations, the open pore volume of this nuclear graphite was reduced from 22 to 7%. By as-

are expanding more rapidly than our economic and population expansion. Therefore it must be obvious that, unless something intervenes, ultimately the entire world population could be scientists and engineers, all publications could be scientific or technical, and all our gross national product could be devoted to research and development. There is a limiting factor or equilibrium ceiling, however, which we have already mentioned, and this insures us of a reasonable limitation to the proportion of scientists and engineers in the world.

A serious question is whether our scientific growth and production can stand doubling every ten years. Such a rate of increase is our present growth rate in many areas, but we are postulating a gradual reduction of our over-all scientific and technical growth rate to about 2% per year by the year 2000 rather than a continuance at our present 8% per year and doubling every ten years. A continuance of our present growth rate of 8% per year in all areas would require an increase in the per cent of scientists and engineers in our working group from the present 1.3% to approximately 10% in the year 2000 and to 100% by the year 2060 (Fig. 2). This is obviously an impossible situation.

It is just not practical to contemplate an 8% expansion in growth rate as universally applicable to all scientific activities over any considerable period of time in the future. It will be necessary to assign priorities and growth limits to some of our scientific and technical activities. As a result of the bulge in the birth rate increase in the 1940–50's, there will be nearly double the number of new scientists and engineers being annually produced in 1970, as compared with the number of new scientists and engineers produced in 1955. Today, we are annually adding about 55,000 and by 1970 we should be annually adding about 100,000. The number of scientists and engineers produced will not continue to increase sharply at this accelerating rate beyond 1970, and the expected total of scientists and engineers in 1980 will be 2.5 million or 2.5 times today's total. Therefore, more effective utilization of the talents of our scientists and engineers and the creation of a supporting body of technicians is essential to effect our maximum production.

The predicted doubling of the number of students in our universities [5, 18], in 1970 is not based on the 1960 enrollment of 3,235,000 but rather on our 1955 enrollment of 2,667,000, for the expected enrollment in 1970 is estimated at 5,447,000. In some surveys it is contemplated that not only will the number of persons of college age increase rapidly up to 1970 [10, 14], as indicated in the foregoing data but that, in addition, the per cent of this group which will go to college will rise from its present proportion of about 35% to about 45%. Such an increase should be disturbing to those who feel that already more go to college than should and that the present day elimination of one-third of our college students prior to graduation is a reflection on our standards of admission.

suming cylindrical pores, the expression $A = 2V/\bar{r}$ can be used to calculate the total surface area in the macropore range. That is, for each volume increment (for example, between data points in Figure 9) a corresponding surface area increment, A, can be calculated by taking the arithmetic average pore radius, $\bar{r}$, over the increment. If this is done, it is frequently found that the calculated macropore surface area is greater than the total surface area as measured by gas adsorption. A

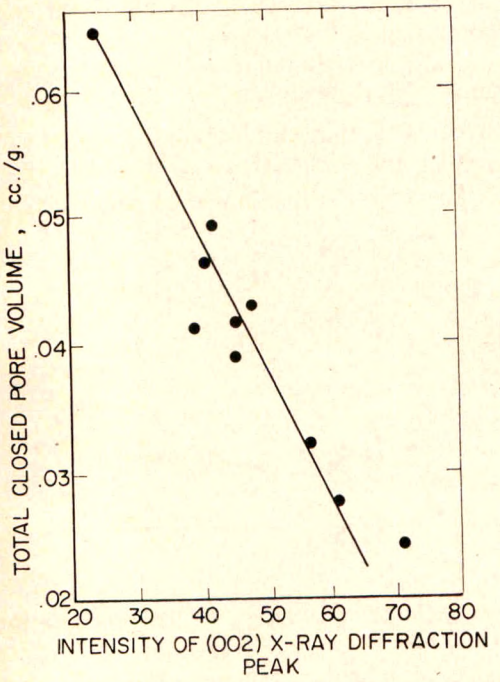


FIG. 8. Variation of closed pore volume in calcined petroleum cokes with crystallite orientation [40].

reasonable explanation is that the macropores contain pore constrictions, which result in too high a volume of mercury being assigned to too small a pore size.

The specific surface area of baked and graphitized carbon bodies, as calculated from low temperature (78°K.) nitrogen gas adsorption, is usually low—in the range from 0.5 to 2.0 m.²/g. This area can be compared with the area calculated from crystallite size data. For example, a baked carbon body with an average crystallite size of 30 Å. would have a crystallite surface area of about 900 m.²/g. The great difference between the measured and calculated surface areas means that essentially all of the crystallite surfaces are inaccessible to nitrogen at 78°K. This is a result of a high degree of crystallite alignment and a close approach of crystallites in regular carbon bodies. It will be seen later that this is not always the case with carbons.

From experiences with coal, there was concern that the surface areas

Those who predict a rapid increase in the number of students in our universities in the next decade do not contemplate a proportional increase in the teaching staffs in spite of the fact that we generally recognize that our teaching profession is understaffed. The U.S. Office of Education prediction [19] estimates that, with nearly double the number of students in 1970, the college and university operating staffs would in-

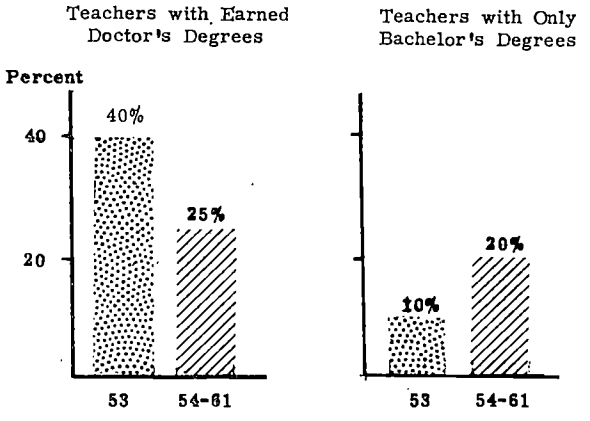


FIG. 6. Reduction in training of new college teachers (1954–61) as compared with total staff in 1953. (Data from Maul, NEA Report R12 [18]). These data may overaccent the situation, for many college teachers now complete degrees after joining the faculty, but the current proportion of college teachers with advanced degrees is less than it was a decade ago.

crease by only 50%. In their estimate they include the 10% of the staff who are currently working full time on contract non-teaching programs, and, in the 1970 estimates, they expect the number engaged on contract programs to double. Thus, in fact, the number to be engaged in teaching would only increase by 33% and would handle nearly twice the number of students.

An attempt has been made to develop a concept for the future growth of science and engineering as measured by the increase in the number of scientists and engineers. In spite of an expected near-doubling in the next decade of the annual number of college graduates by 1970, due to an anomalous bulge in our school age population, the long range expansion after 1970 will continue at this accelerated rate and our rate of production of college graduates will eventually approach our population expansion rate. The reduction in the growth rate of our supply of scientists and engineers to the growth rate of our population will in the end be a controlling factor in the growth rate or expansion of our national scientific programs.

There are a number of obvious indications of the approaching pinch or equilibrium ceiling in science. There is a critical shortage of teachers

of nuclear graphite as measured by low temperature gas adsorption might be low because of activated diffusion [45]. That is, the adsorbate might be able to penetrate only a small fraction of the pore area in the time allotted for equilibration. Therefore, areas obtained from low temperature adsorption were compared with those obtained from adsorption of argon at room temperature. Previous work had shown that gas diffusion and equilibration of argon is rapid even in molecular sieve materials, at room temperature. The agreement in the areas was excellent, thereby confirming the conclusion that nuclear graphite does have a low, open surface area.

Electrical and Thermal Properties of Carbon Bodies: We will consider briefly the electrical resistivity and thermal conductivity of carbon bodies. Petroleum cokes are irregular in shape and, upon forming of carbon bodies containing them as filler, the particles tend to align with their longer axes either parallel to the extrusion direction or perpendicular to

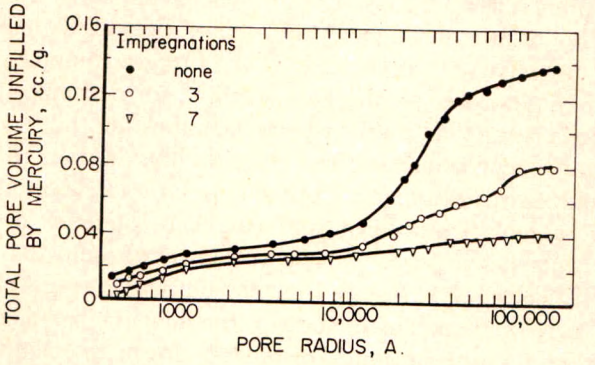


FIG. 9. Typical distribution of open pore volume in nuclear graphites of different porosities [44].

the molding direction. The degree of alignment increases as the particles become more asymmetric in shape. Also, carbon crystallites which compose a coke particle are aligned with their layer planes more or less parallel to the longer axes of the particle [28]. Thus, because of the marked anisotropy of such properties as electrical resistivity and thermal conductivity within a carbon crystallite, particle alignment imparts to formed carbon bodies some degree of anisotropy in these properties. As will be seen, however, the extent of anisotropy is much smaller than that found in graphite single crystals. The primary reason for this is that much of the resistance to electron and heat flow in a carbon body is contributed, not by the filler particles, but by the binder-carbon bridges between filler particles [46]. This is a result both of the higher resistivity of the binder coke to electron and heat flow and of a decrease in area for flow at the binder constrictions.

Typical room-temperature values of electrical resistivity and thermal conductivity parallel to the extrusion direction in baked carbon bodies

of science in our secondary schools, where the competition for technical assistants in contract research and development programs has drawn heavily on the elementary teacher group due to ability by government contractors to provide better working conditions and more attractive salaries. In the next higher level of teaching, our colleges and universities are beginning to show the lack of adequate staffs, both in numbers of teachers and their formal preparation. Many small schools have been unable to recruit personnel, and in nearly all colleges and universities there has been a marked drop in the number of persons with doctor's degrees entering the teaching profession and there has been a corresponding rise in the number of new teachers who have only a bachelor's degree [17] (Fig. 6).

Many government agencies and universities are planning on a level of operation for the next decade and on construction to serve for the balance of this century. Such government agencies as Defense and Atomic Energy contemplate a nearly double increase in organization size (8% increase per year) in the next ten years. Others such as the Space Agency contemplate as much as a five-fold expansion by 1970 (20% increase per year), and there will undoubtedly be new and uncontemplated technical agencies which will be created in the future.

If, as seems likely, the U. S. expansion rate of scientists and engineers drops to about 2%, or about one-fourth the present expansion rate, by the year 2000, we must be prepared to curtail our current technical expansion rate. It must be obvious that all areas of science and technology cannot continue to expand at the current rate. If some are to continue to expand at the current rate of 8%, or even at a greater accelerated rate (as is true of some of our space, atomic and defense areas), then other areas will have to be held static or even reduced.

Production of Publications

Over the past century there has been a steady increase in science and technology as measured by not only the number of trained experts, but also by the number of publications. The increase in the number of experts or the number of publications demonstrates quantitatively, that the world expansion rate in science and technology has been fairly constant at about 10% per year. This is a compounding effect resulting in a doubling of the annual world production every eight years.

One of the best measures of the quality and quantity of our scientific literature is the abstract journal. Outstanding as an abstract journal is *Chemical Abstracts*, published by the American Chemical Society, which endeavors to collect and abstract every available contribution in all languages from all parts of the world [20]. The area of chemistry represents only about 20% of the world's total scientific and technological literature, but chemistry is typical of science as a whole. Chemistry

(total porosity of about 25%) are 5×10^{-3} ohm cm. and 4×10^{-2} watt/-cm./°C., respectively. Upon graphitization at about 2800°C., a carbon body of similar porosity has typical electrical resistivities of 7×10^{-4} and 9×10^{-4} ohm cm. parallel and perpendicular to the extrusion direction, respectively. The anisotropy of electrical resistivity is only 1.3 compared to at least 100 in a single crystal. Comparable graphitized carbon bodies have typical thermal conductivities of 2.3 and 1.4 watts/-cm./°C. parallel and perpendicular to the extrusion direction, respectively. The anisotropy of thermal conductivity is 1.6 compared to about 5 for a single crystal.

Carbon Black

History and Methods of Preparation: Carbon blacks are formed by the thermal decomposition of gaseous and liquid hydrocarbons, with the operation occurring at essentially atmospheric pressure. Two processes are used to produce the majority of the carbon black manufactured today: (1) the channel process and (2) the furnace combustion process. In both of these processes, the heat required for decomposition of the hydrocarbons is supplied by their partial combustion.

The channel process has evolved since its beginning in 1872. Carbon black is collected by impingement of small, natural gas diffusion flames on relatively cool channel iron surfaces. The character of the black, particularly its particle size, can be varied by altering the size of the burner tip used and the distance from the burner tip to the channel surface. The narrower the slot in the tip, the finer the particle size of the black produced. At an optimum distance between the channel and the burner tip, both the particle size and black concentration are at a maximum. If the distance is increased, the particle size and yield of the black are reduced because of its reaction with oxygen. This reaction is deliberately encouraged in some cases in order to yield smaller blacks of higher porosity and oxygen content.

Until 1940, the channel black process was used to produce the majority of carbon black consumed, but, since then, the furnace combustion process has grown much more rapidly and today its use is responsible for the greater amount of black. This is a result, apparently, of easier processing of furnace blacks in synthetic rubber. Furnace combustion blacks are also produced in diffusion flames. However, instead of the large number of tiny flames utilized in the channel process, a large volume of natural gas and/or liquid hydrocarbons and air undergoes combustion and cracking in firebrick-lined furnaces. The carbon black, suspended in the spent reaction gases, is collected by a combination of electrostatic precipitation and centrifugal force. This process is capable of producing blacks of considerably larger particle size than the channel process.

includes rapidly expanding areas, such as nuclear reactions, solid state, and antibiotics, as well as slower expanding areas such as leather, soap, and storage batteries. Thus the area of chemistry as a whole provides a good guide upon which to base our broad predictions. Perhaps as important is the fact that *Chemical Abstracts* is outstanding in its world

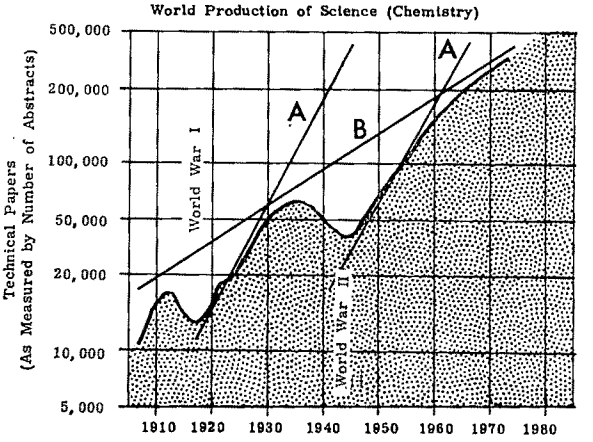


FIG. 7. World production of chemical research as measured by abstracts in *Chemical Abstracts*. *A* = Peace-time growth rate, 8 to 10% per year and doubling in numbers every 10 to 8 years. *B* = Average growth including war years, 4% per year and doubling in numbers every 20 years (data from Baker [20]). The average growth rate "B" is of course depressed by the war effects, whereas, the accelerated growth rate "A" undoubtedly includes material and ideas which germinated during war years and had to be postponed until peacetime to be developed and published. A more realistic "normal" growth rate for science would be between "A" and "B" or about 8% per year.

coverage and hence provides the best international source for such predictions. For the past fifty years, excluding the war years when normal publication was not maintained, there has been a steady expansion in the scientific literature as measured by *Chemical Abstracts* (Fig. 7). This growth amounts to about a 10% increase each year or a doubling every eight years. The so-called population explosion produced by the 1.7% population growth per year and doubling every fifty years is insignificant as compared with the impending "scientific explosion."

With this continuing growth of about 10% more each year and a doubling in the annual growth about every eight years, it is obvious that more has been published in the past ten years in science than was published in science in all preceding time. Another way of indicating this compounding growth effect in science is the oft-quoted phrase that 90% of all the scientists who have ever existed are alive today. The point which we have been developing is that growth and expansion can continue at an accelerating rate only so long as the supply or source is in excess of

A small but important class of carbon blacks is produced in the furnace thermal process. In this process, the black can be formed in the absence of oxygen or oxygen-containing product gases. When natural gas is used, carbon black is produced in an intermittent operation. Air and natural gas for complete combustion are first fired into an insulated furnace, heating it to as high as 1400°C. The natural gas is then forced through the furnace and cracked, producing a very coarse carbon black. The particle size of the black can be reduced somewhat by diluting the natural gas with an appropriate amount of flue gas from the heating cycle. Furnace thermal black is also produced by the continuous decomposition of acetylene. Since this reaction is exothermic, the heat liberated from the decomposition can be used to maintain the reaction.

About 90% of the carbon black produced is used as a filler in rubber. The carbon increases the strength and resistance to abrasion of rubber. Carbon black is also used as an additive to inks, paints, and plastics, and as thermal insulation.

Mechanism of Carbon Black Formation: There are, broadly speaking, two main views on carbon formation in flames: (1) Hydrocarbons decompose into acetylene and other low molecular weight species, followed by a process of simultaneous condensation and dehydrogenation to produce carbon and (2) hydrocarbons, by a process of dehydrogenation and condensation, produce polymers of high molecular weight (carbon). Porter [47] contends that the first mechanism, through acetylene as the intermediate, accounts for most of the production of carbon in diffusion flames. He reasons, on the basis of kinetic and thermodynamic arguments, that, at temperatures above 1000°C., decomposition predominates over polymerization. Therefore, if carbon is to be produced by mechanism (2), the hydrocarbons would have to remain in the preheating zone of a flame long enough (at temperatures below 1000°C.) to produce a polymer which would be stable above 1000°C. This polymer could then dehydrogenate and produce carbon. However, Porter concludes that the hydrocarbons in a diffusion flame are usually heated to 1000°C. too rapidly to allow polymers of sufficient size to grow. Although these arguments are plausible, the mechanism of carbon formation cannot yet be considered to be well understood.

Structure of Carbon Blacks: Electron microscope studies have revealed that the particles of most carbon blacks are essentially spherical in shape. There is a great range of average particle diameters in carbon blacks—from a low of about 100 Å. for the smallest channel black to a high of about 5000 Å. for the coarsest furnace thermal black. Carbon blacks are composed of varying fractions of turbostratic crystallites and disorganized carbon. The sizes of the crystallites are essentially independent of the black particle size; the crystallite size in almost all cases is in the range from 10 to 20 Å. [48]. The amount of disorganized carbon

the requirement. Once we reach the proverbial "bottom of the well" our supply will be limited by the inflow or seepage of our source material, namely, the population growth.

Thus, there is the general concept that when advancing at maximum capability, there is a reasonable limit to the rate of growth or expansion equilibrium of science and technology. By virtue of our apparent head start, we are in all probability due to reach rate limitation or equilibrium sooner than others, and, in some areas of science or technology, we may be at this rate equilibrium in the near future, when our growth rate will parallel our population growth rate. It must be stressed that such a situation will not be easy to attain or maintain. It is easy for the slow walker to double his rate of progress by accelerating this pace, but the fast runner operating at nearly his maximum capability can, on a percentage basis, do little, and with great effort, to accelerate his rate of progress.

Expansion of Professional Organizations

Many organizations are making plans for the future which cover much of the remaining period of this century. The American Chemical Society, the American Institute of Physics and other professional groups are seriously considering the future publication problems of their technical and abstract publications, their probable growth, and what will be necessary to handle their needs in the next forty years. The scientist expects a continuous expansion of the world's scientific literature, as measured by the abstract journals, of about 10% increase per year and, hence, doubling in amount every eight years. There is no indication that there will be any let-up or reduction in this world expansion rate in the next forty years. However, it is postulated that the national expansion rate in this country will drop in this period of time.

A study by the National Science Foundation [21] of the growth of some sixty of our leading scientific and engineering societies in this country, shows an average growth of 4.25-fold in the past twenty-two years or an average growth of 8% per year. The major growth of many of these societies was in the late 40's and many have decelerated since then. The greatest growth, as would be predicted, has centered in physics and electronics, with a nearly 6-fold increase and the other societies have more nearly averaged a 3.5-fold increase, or 5% per year in the twenty two-year period of study. From these data and the student population figures it would be reasonable to predict that the expansion rate of our societies by 1970 and beyond for the balance of the century would drop from 8% to 3% or 4% depending on the degree of saturation and changes in the nation's scientific emphasis. This coincides with the long range prediction of the American Chemical Society which expects a 3 to 4% annual increase in this period. On the other hand, the physicists predict an increase from their 16,000 members at present, to 38,000 by 1970, which

TABLE I

PHYSICAL DATA FOR TWO SELECTED CARBON BLACKS [49]

Property	*Channel Black*	*Furnace Combustion Black*
Helium density, g./cc.	2.02	2.06
Open pore volume, cc./g.	0.170	0.002
Apparent density, g./cc.	1.50	2.05
Total surface area, m.²/g.	1000	69
Free surface area, m.²/g.	587	65
Internal surface area, m.²/g.	413	4
Electron microscope surface area, m.²/g.	264	74
Average pore diameter, d, Å	17	20

is as high as 26% of the total carbon present in the smallest particle-sized channel blacks [48]. Carbon blacks are more or less porous depending upon the perfection of their crystallite alignment, the amount of disorganized carbon present, and the extent of their oxidation during preparation. Recently, a method based on low temperature adsorption data and helium densities, which gives information on the nature of the porosity within blacks, has been presented [49]. These data are summarized in Table I for a very fine-sized channel black and a widely used oil furnace

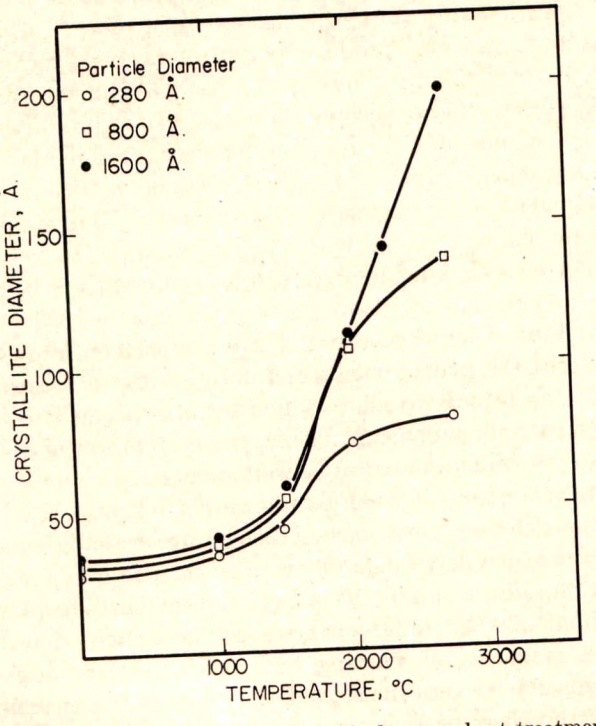


FIG. 10. Crystallite growth of carbon blacks upon heat treatment to elevated temperatures [50].

would require an annual increase of from 10 to 12% per year. This may be based on the swing to physics in recent years but is not realistic with respect to the long range supply of personnel.

Economic Expansion

One could develop a similar thesis on the ultimate limited rate of growth of science and technology by a study of program budgets compared to the rate of expansion of gross national products. A study by the National Science Foundation [22], of the total funds expended over most of the past decade, in the United States on research and development, indicates that, in this period, there has been a steady growth of about 15% per year in the funds expended. This means a doubling every 5 years, and this rate of increase cannot continue for too long a time in the future. In 1960–61, our research and development expenditure was 14 billion dollars, of which the federal government provided 9 billion. Since our gross national product increases at only 3.5% per year, our research and development budget would exceed our gross national product before the end of this century if both continue at their present rates and the research and development budget would exceed our governmental budget before 1975 (Fig. 8).

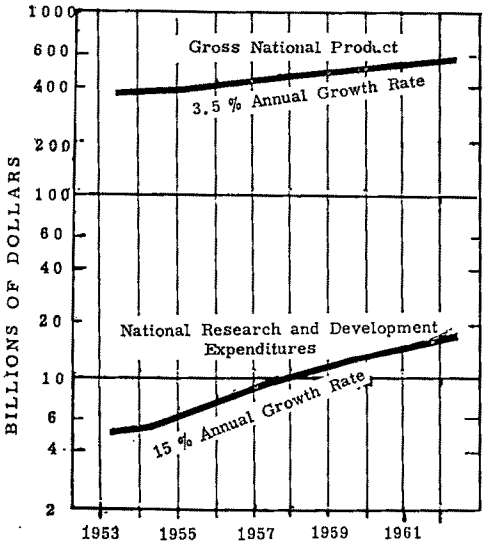


FIG. 8. Comparative growth of U. S. Research and Development Funds and the U. S. Gross National Product [22].

The concern of our economic planners about this growing influence on our budgetary program is demonstrated by the reproduction of the National Science Foundation estimates under the title of the "Rising Outlays for Research and Development," not on the science page, but rather on the business activity page of a recent number of the *U.S. News and World Report* [23]. Also, the graphical presentation of these funds

combustion black. It is seen that the channel black has considerable internal porosity, whereas the internal porosity in the oil furnace combustion black is negligible. The amount and nature of the porosity in blacks is important in affecting the behavior of the black in its applications.

Graphitization of Carbon Blacks: The heat treatment of carbon blacks to temperatures between 2000 and 3000°C. produces interesting results. Biscoe and Warren [15], using X-ray diffraction, performed pioneering studies on the graphitization of Spheron-6, a channel black having an arithmetic mean particle diameter of about 250 Å. They found that, upon heat treatment to 2800°C., the crystallite diameter and height were

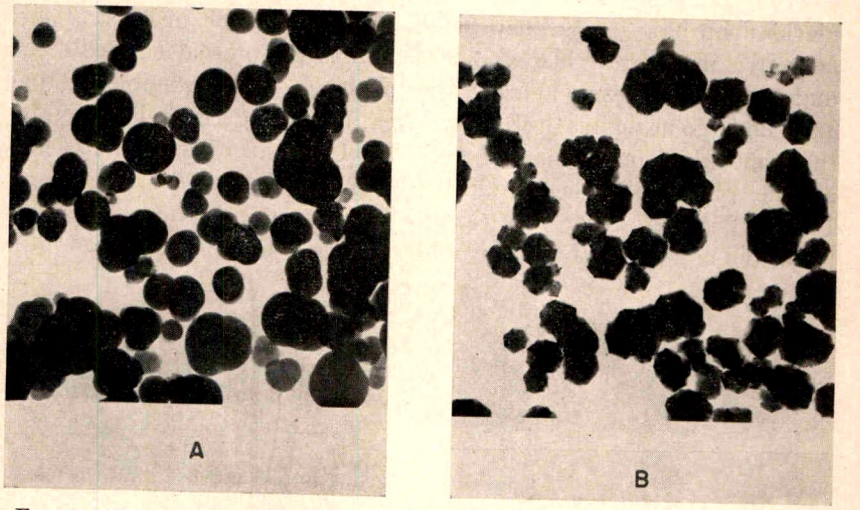


FIG. 11. Electron micrographs of (A) raw furnace thermal black and (B) black heated to 3000°C., magnification 35,000X [29].

increased only to about 65 and 40 Å., respectively. The graphitized black had an interlayer spacing of 3.44 Å., indicating no stacking order. It is shown [50] that the extent to which the crystallites in carbon black grow upon heat treatment is limited by the particle size of the black. Figure 10 shows results for the growth in crystallite diameter with temperature of three blacks of widely different particle diameters. It is suggested that crystallites in carbon blacks never grow to a size greater than one-third of the particle diameter [16].

The heat treatment of carbon blacks has been studied using electron microscopy [28, 51]. Figure 11 shows a large furnace thermal black both in the raw state and following heating to 3000°C. No appreciable change in the appearance of the black is observed for heat treatment up to 1500°C. As the heat-treatment temperature is further raised, dark strips which run from the periphery to the inside of the particle are noted.

as a per cent of our gross national product appeared in the review of this report by *Chemical and Engineering News* [24]. Facilities, like funds, may also be used as a growth measure and are probably more specifically localized to single programs or a limited group of scientists and engineers. A study of the growing size of special facilities for space, atomic, geophysical, educational, agricultural, and health programs would also lead to the obvious conclusion that there must also be an equilibrium growth limit to the continuance at the present expanding rate [25, 26].

Just as this nation is closer to the saturation equilibrium in many technological areas so are also our levels of support for research and development higher than those of other countries. No other nation approaches our high ratio between research and development expenses and gross national product. Other nations, however, are moving in our direction and may eventually reach or surpass our level.

The possible conflicts in the future between nations may be technical wars for prestige or economic leadership, and, in fact, many now consider the space race in this category. New major "technological wars" may upset our calculations and plans for normal scientific and technological expansion, but, if so, they will only do so by accelerating the effort in a given area, at the expense of other areas and distorting the distribution of our technological efforts rather than creating more scientists and engineers. We are, of course, assuming that we are then producing scientists and engineers at our maximum capability rate. There must be some careful studies on the relative distribution of our effort in space, nucleonics, meteorology, oceanography, defense, education, agriculture, health, industry, and other important areas in order to maintain a balanced and effective program in the government, universities, industry and foundations.

Development of Other World Areas

We should recognize, however, that the present day contributions to science and technology are in no way proportional to national populations in the various areas of the world. Many of the highly populated areas of the world are producing very little in science and engineering, and others are producing more than their share according to their relative population size. In 1959, the United States, with only 5% of the world's population, was producing about 30% of the world's scientific literature and the U.S.S.R., with 8% of the world's population, was producing 16% of the world's scientific contributions. On the other hand, South America, Central America and Mexico with a total population about equal to that of the United States contributed only about 2% of the world's scientific literature (Fig. 9).

Ethnologists and educators tell us that there is essentially little difference in the intellectual ability ranges among different races and nation-

Above 2500°C., the dark strips grow into trigonal or trapezoidal dark shapes, whose bases coincide with the edges of the polygonal outlines of the particle. It is reasoned that these dark areas are produced by diffraction effects from the layer planes of the crystallites. This leads to the conclusion that crystallites are in preferred orientation with their layer planes forming the particle surface. Figure 12 presents a schematic diagram of alignment of the crystallites in a raw and heat-treated carbon black [28]. As discussed previously, the extent of crystallite alignment in the raw carbon blacks varies widely, resulting in a variation in the extent of open, internal porosity.

Effect of Graphitization of Carbon Blacks on Their Physical Structure and Surface Properties: The degree of change in the surface area of carbon blacks upon heat treatment depends upon the amount of internal surface area in the raw black. Blacks with large internal surface areas undergo large decreases in area; whereas, blacks with negligible internal area, undergo negligible decreases in area. The area decrease is caused by the removal of open, internal porosity. Upon heat treatment of the channel black in Table I to 2800°C., its surface area decreased from 1000 to 251 m^2./g.—or very close to its geometric area. On the other hand, the non-porous furnace combustion black in Table I underwent a

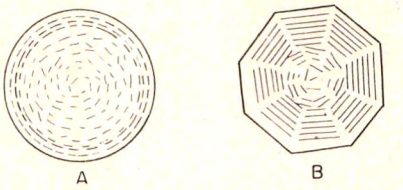


FIG. 12. Schematic diagram of alignment of crystallites in (A) raw and (B) heat treated carbon black [29].

negligible change in surface area upon heat treatment to 2800°C.—from 69 to 68 m^2./g. Figure 13 shows the change in surface area of a medium channel black upon heat treatment [52]. The initial increase in area is produced by activating the black, upon removal of its volatile matter as oxides of carbon. Above 1000°C., the area of the black decreases sharply.

Despite the removal of open porosity from carbon blacks upon heat treatment, the helium densities of the blacks frequently undergo large decreases. For example, the helium density of the channel black in Table I decreases from 2.02 to 1.52 g./cc. upon heat treatment to 2800°C. [53]. It is shown [53] that the extent of the change in helium density upon heat treatment can be affected by preoxidation of the black. The channel black Spheron-6, with negligible internal porosity, undergoes a change in helium density from 1.94 to 1.92 g./cc. upon heat treat-

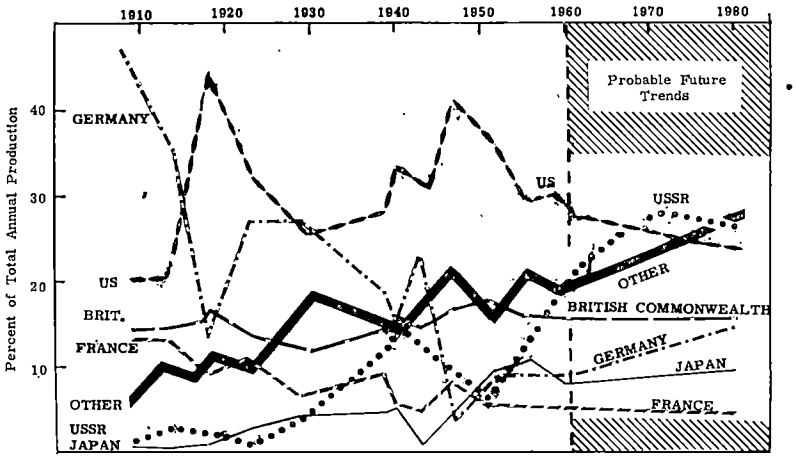


FIG. 9. National contributions to the world's chemical literature (Data from Baker [20]). (Note: Chemical production is roughly equivalent to 20% of world's total scientific and technical literature). Particular notice should be taken of the relative steady growth of "other" nations. About one half of "other" nations includes such highly advanced small nations as Italy, Belgium, Sweden, Switzerland, The Netherlands, etc., but the balance includes the newly developing nations which are being brought up to productive levels. The U. S. actually has increased its production of chemical literature. However, this production appears as a reduction when plotted as a proportionate contribution to the world's total because of the accelerated rate of production in other areas of the world.

In Figure 9, the 1947 peak production in the U. S. of 42% of world science (chemical) represented some 16,000 technical papers. The 1960 drop of the U. S. share of the world's production to only 28% does not, however, represent a reduction in U. S. effort, but rather a greater increase of activity in other areas of the world. The total U. S. production in 1960 was 38,000 papers or some 2.5 times increase over the 1947 production.

The apparent steady state of the British Commonwealth and the reducing percentage contributions of France both, when converted to total production by superposition of the data in Figure 7, actually show a considerable expansion. The case of the U.S.S.R. is one of accelerated expansion which will probably equal U. S. production by 1965–70 but will undoubtedly approach and parallel the U. S. rate as saturation is reached and other under-developed nations become productive. On a per capita basis, the U. S. might not be ahead of some of the smaller and highly advanced nations but among the larger nations we are holding a marked leadership even though our rate of increase shows evidence of approaching a saturation growth rate.

alities. There will be brilliant individuals in all nations and races, Hindus or Eskimos, Danes or Japanese. It is a part of our national philosophy to try to provide an opportunity for developing nations and their gifted populace to advance and raise the productive and technical level of their countries. It will be some time before the less productive areas of the world will reach to the level of the highly productive areas, but it must be recognized that their rate of advance, especially if their initial level was low and they are aided by more advanced nations, will be at a much faster rate than the advance of the highly productive

ment at 2500°C. Preoxidation of the black to 22% weight loss results in a decrease in helium density from 1.95 to 1.64 g./cc. upon heat treatment to 2500°C. By contrast, oxidation of the heat treated (raw, original) black to 22% weight loss increases the helium density to 1.98 g./cc.

Heat-treated carbon blacks have been studied very extensively by surface chemists, because of their large and homogeneous surface. That is, the surface consists ideally of only the layer planes of carbon. The extent of surface homogeneity has been determined in three main ways:

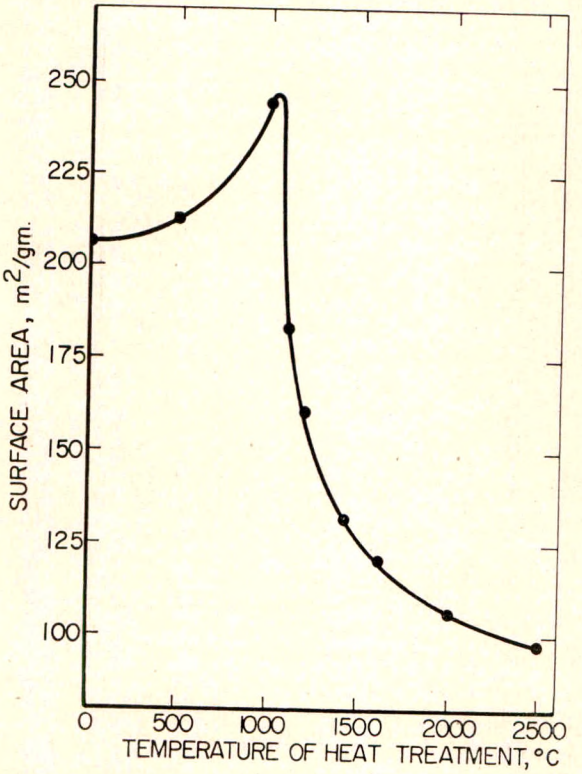


FIG. 13. Effect of heat treatment on surface area of a medium channel black [52].

(1) from the appearance of an entire adsorption isotherm, (2) from heats of physical adsorption as a function of surface coverage with adsorbate, and (3) from a deviation of an adsorption isotherm from linearity at very low coverages.

Considering the first method, it has been predicted theoretically that multilayer adsorption on a homogeneous surface should give rise to an isotherm composed of a number of steps [54]. A step should appear at the completion of each adsorption layer. The adsorption of argon at 78°K. on a thermal black, similar to that shown in Figure 11, has been measured, following its heat treatment at a series of temperatures [55]. The

nations. It is more difficult to increase the expansion rate or proportional increase as maximum capability is approached. It was easy to increase the per cent of school age children going to elementary schools when only 10% of the school age children were in attendance; but when 98% of the school age children are in attendance our growth rate essentially settles down to our population growth rate.

We are involved in two somewhat divergent philosophies; one is to attain world leadership and the other is to bring, by aid and education, areas of the world up to our own level of science and technology. We must eventually accept the long-range philosophy that, as we aid others to rise to our cultural level, our own leadership position will be relatively reduced. Also it will take greater effort on our part to retain leadership and, ultimately, others may rise to or surpass our level.

We have mentioned that in 1959 our portion of the world's total output of scientific literature was about 30% and that of the U.S.S.R. was about 16%. The world's expansion or growth increase rate in this cultural area is about 10% per year, whereas the United States growth rate is only about 6 to 8% per year; the U.S.S.R. is expanding at about 16% per year. Recent calculations would indicate that although our total output in 1960 increased over 1959, other areas of the world developed at a faster rate so that our proportionate production dropped to about 28% of the world's science and that of the U.S.S.R. rose to about 18%. These data are based on treating each of these two countries as a unit and not considering the 30% population advantage which the U.S.S.R. has over the U.S. We would expect the U.S. and the U.S.S.R. to be producing the same amount (about 24%) sometime between 1965 and 1970 but after that period the U.S.S.R., by reason of its 30% greater population, should produce a larger portion than the U.S. However, the output of both countries should reduce relative to the rest of the world as the less developed areas become more productive. There are many of the developing countries whose rate of expansion is far greater than either that of the U.S. or U.S.S.R., and it must be recognized that there are limiting factors in the increase of rate of growth as one approaches capacity. At the same time, one must also recognize that to maintain a high level of accomplishment requires continued and extended effort, and that it is not a matter of coasting along once one has reached an equilibrium rate.

Many accomplishments are measured on a national basis, when a more logical comparison would be on a per capita basis. In the Olympic games, a quite different order is obtained when one determines points won per capita. Often, however, we may lead even on such a basis. For example, we not only have the largest total number of telephones and automobiles in this country, but also we have the highest ratio per capita.

development of steps of increasing steepness in the isotherms with increasing heat treatment temperature is seen in Figure 14, indicating a progressive increase in surface homogeneity. The steepness of the steps in the isotherm for the black heated to 2700°C. appears to be comparable with that found in the isotherm obtained from the adsorption of argon on Ceylon graphite heated to 2700°C. [56].

Considering the second method, the change in the heat of physical adsorption with coverage is dependent upon the degree of homogeneity of the surface. If the surface is heterogeneous, physical adsorption occurs on the strong sites first and a large decrease in heat of adsorption with

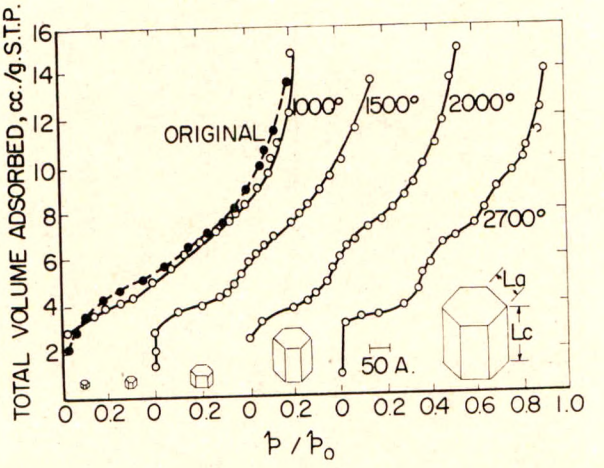


FIG. 14. Change in surface homogeneity of carbon black upon heat treatment as followed by adsorption isotherms [55].

increase in coverage results. For a very homogeneous surface, there is little change of heat of adsorption with coverage until significant adsorbate interaction occurs. The heats of adsorption of argon at 78°K. on Spheron-6 blacks heated up to 2700°C. have been measured calorimetrically [57]. As shown in Figure 15, the heat of adsorption at low coverages on the raw black is seen to rise sharply to about 3.8 kcal./mole. On the black heated to 2700°C. (called Graphon by surface chemists), the heat of adsorption at very low coverages is only about 2.7 kcal./mole. The increase in heat of adsorption due to adsorbate interaction becomes progressively more prominent as the black is heated to higher temperatures, because the effect is less obscured by a decreasing heat of adsorption due to surface heterogeneity.

Considering the third method, it is reasoned that, on a completely homogeneous surface, the adsorption isotherm at very low coverages should be linear and go through the origin [58]. On the other hand, for a heterogeneous surface the adsorption volume should increase more

EFFECTS OF THE GROWTH OF SCIENCE ON SUPPORTING AND PERFORMING INSTITUTIONS

In the past, science and technology have been compartmentalized in our governmental, university, industrial and institute operations and programs. With the growth of science there has developed an increasing amount of cross-fertilization and interaction, not only between sciences but between organizations and institutions. Today there is an increasing liaison and flow of personnel, support and joint operations so that the philosophy or principles of operation in one area have an effect in other areas. A part of the interaction between the government and other sectors exists because of the accelerated growth of science and technology, with the attendant increasing federal support coupled with a limited supply of scientists and engineers.

Development of Internal Competence in Governmental and Non-Governmental Operations

A basic difference between philosophies of the operation of our government scientific and technological programs would appear to be external versus internal operation. The supporters of either form are not determined to eliminate completely the other form but rather each tends to favor one procedure more than the other.

In line with the philosophy of strengthening of the internal competence of the government, there should be considered the method of operation of many of our major scientific agencies such as the Atomic Energy Commission, National Aeronautics and Space Administration, the National Science Foundation, the Defense Department and certain commissions and boards. These groups have in the past supported various policies of contract-operated institutes and programs, some with preference to the assignment of programs to government-operated agencies or laboratories, and others emphasizing external contract operation.

There should obviously be reasonable opportunities for universities, industry and the government to develop their capabilities in research and development programs. There does seem to be a need, however, to determine the extent, direction, objective, and mission in many of our programs and the degree to which an internal capability in the government is desirable. Among the major problems to be considered is the extent to which, and with whom, contract operation should be used in basic and applied research. What kind of programs are best done on a cooperative basis between the government, industry, universities or private foundations? What should be the extent of development of career scientific staff within the government to guide the government programs [11]?

rapidly with increasing adsorption pressure at very low coverages, until the strong sites are covered; then the isotherm should become linear. The volume of strong sites is given by the intercept that is obtained upon extrapolating the linear portion of the isotherm back to zero pressure. This technique has been used on original and oxidized Graphon by following the adsorption of nitrogen at 78°K. [59]. Figure 16 shows the low pressure end of the isotherm for the original Graphon and the sample

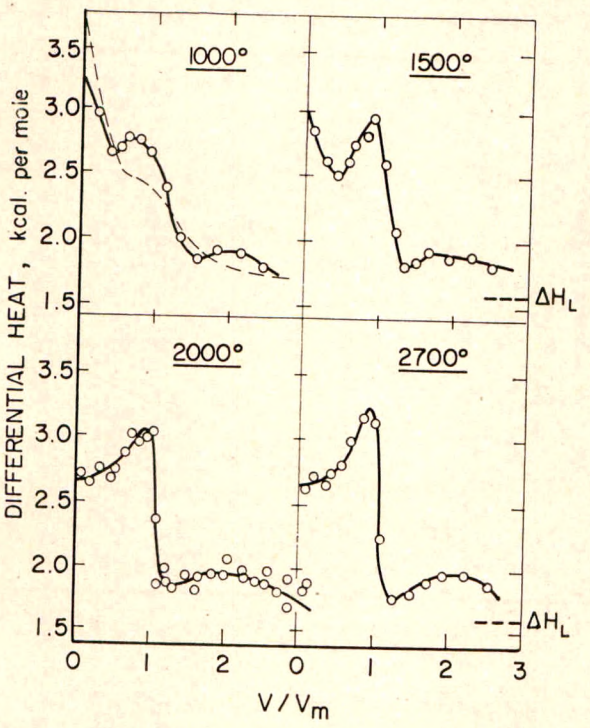


Fig. 15. Change in surface homogeneity of Graphon upon oxidation as followed by heats of adsorption [57].

oxidized to 8.5% weight loss. The original Graphon has a negligible amount of strong sites as measured by this method. On the other hand, following 8.5% burn-off, the strong site area has increased to 1.6% of the total surface area. Oxidation is probably increasing the strong site area through exposure of the edges of layer planes, by reacting away carbon atoms at the intersections of the polygonal surfaces and/or within the layer planes (pitting).

The effect of heat treatment of a large thermal black, similar to that shown in Figure 11, on its increase in surface area upon oxidation has been investigated [60]. Figure 17 summarizes the results. The original black undergoes a large increase in area, whereas the black heated to 2700°C. increases in area to about the extent that the geometric area

Need for Career Leadership

In the cultural, educational, industrial and governmental areas of our nation we should endeavor to attract and create a corps of career experts of distinction to lead and guide our programs [27].

An important key to a strong and effective national program in science and technology is a staff of career scientists and administrators with scientific comprehension who can be entrusted with operational responsibility. Advisory groups from industry and universities should be advisory to these experts and should not be operative or enforcing on the career directors. There could be evolved, in our government, the same pattern which now exists in our universities and industry; career experts are chosen to operate, and consultants, advisers and visiting workers do not have administrative authority.

We do have now in the government some very competent leaders in science, but the number could be increased. The apparent sources of replacement, however, are evaporating due to the recruitment pressures from outside competition. An improved climate or atmosphere of employment and utilization would hold those career leaders we now have and attract others.

Recognition of Mission

We are concerned that in many areas of the government where scientific competence of a high level once existed, there is now starvation for funds, combined with removal of programs, and removal of good staff by recruitment and creation of competing groups on a non-governmental basis (although often with government funds). These factors have all combined to reduce the prestige of the agency and the government in science and technology. There are a number of old line government agencies which held in the past, and still do to an extent, the respect of the scientific community. However, even though there is an expanding scientific era, these agencies are still on nearly the same level, with respect to their man-hour employment appropriations, as they were twenty-five years ago. Many of these agencies, in order to keep up with the expanding scientific world, have been forced to line up along with the universities and industry to receive the handouts of the donor groups, when in fact they should have been donor groups themselves. It would seem reasonable that geological research and development grants from the government to universities and research institutions should emanate from the Geological Survey rather than that the Geological Survey should depend on Defense, Atomic Energy and Space Agencies for supplementary funds to maintain their own programs at a reasonable level.

It is recognized that, in the future, the Federal Government will play an increasingly more important role in the support and guidance of

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increases by virtue of a decrease in particle size. This study was selected because it illustrates one of the main principles in the production of an activated carbon of high surface area. The raw material to be converted to activated carbon should have small, poorly oriented crystallites.

Electrical and Thermal Properties of Carbon Blacks: Because carbon black particles are essentially spherical in shape, the electrical resistivity and thermal conductivity are highly isotropic for bodies made from loose black or from carbon blacks plus a binder. Blacks range widely

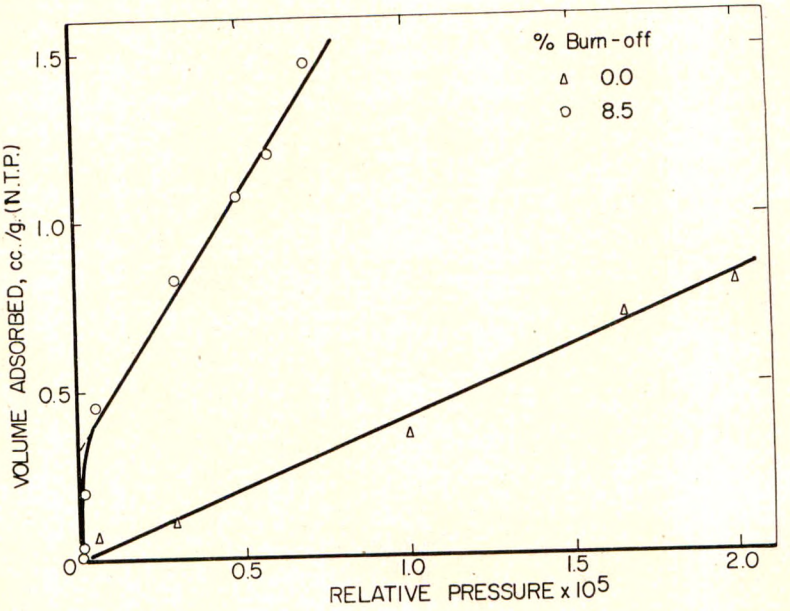


FIG. 16. Change in surface homogeneity of carbon black upon heat treatment as followed by adsorption at very low coverages [59].

in their electrical and thermal properties with variations in particle size, amount of volatile matter, and extent of porosity. It is not the intent here to review all such data. Rather, several points of particular interest will be considered.

Chamberlin [52], among others, has measured the electrical resistivity of compacted carbon blacks following their heat treatment to different temperatures. Figure 18 presents results on the same channel black discussed in Figure 13, where the black is held under a pressure of 2500 psi. Even though there is a progressive increase in crystallite size with increase in heat treatment temperature, the electrical resistivity goes through a pronounced minimum at about 1300°C. This general shape of curve is characteristic of that found for many carbon blacks. It is reasoned that the minimum is caused by the production of a large concentration of positive holes [61] in the black upon the removal of

the nation's scientific and technological advance. On the other hand, it is recognized that the present major support by the government for some three-fourths of the nation's program in scientific and technological research performed by both universities and industry, does not come to any great degree from those agencies which have research as their prime mission. Rather, this research support is a fringe benefit or a minor part of an appropriation made primarily for other purposes. This small per cent of the major appropriation which is spent for support of research and development often appears as a reasonable responsibility for a technologically-based agency such as Defense, Space or Atomic Energy and this portion of its budget is usually approved without special pleading. On the other hand, lesser appropriations sought directly for the same equivalent purpose by smaller groups, such as the National Science Foundation, National Bureau of Standards or Geological Survey may require lengthy pleading and may fare very poorly. The process of tying research grants to a major appropriation of much greater magnitude may be the easy way to obtain the appropriation but it does not seem to be the proper solution. If we are to expand our scientific and technological program support by the government to many times the present amounts, perhaps we should reconsider the methods of appropriation.

The recognition of the mission is of importance in governmental as well as non-governmental organizations which were established as educational or honorary in character; academies, institutes, societies, universities and other tax-exempt groups, have assumed an increasing operating rather than advisory role, with multi-million dollar contracts with the government and other sources of support, and with little, if any, of the work performed by the *bona-fide* members of the body. Like the contract operations performed by many universities, these groups recruit technical workers to perform the tasks, but just as the faculty of the university may have little, if any, control or influence over the contract operation, so it often happens that the members of the academy, institute or society may have a similar lack of influence or control over that which is done in the name of the organization.

The extent to which the government should maintain an internal capability is dictated by much the same factors which guide universities and industry to do the same. Each organization has a mission and the fulfillment of such a mission requires competent leadership. Each may accept activities which are not in a direct line with the mission. Industry may run a school, the university may run a factory and the government may do both. In certain cases, and to a proper degree, such activities which deviate from the mission may be supported, but usually as an interim measure. Long-range planning should lead to transfers of extraneous activities to proper sponsoring agencies. Such transfers are seldom made on a voluntary basis for they violate the simple laws of

volatile matter. Upon heat treatment to higher temperatures, the positive hole concentration is decreased, as crystallites further grow.

Carbon blacks are widely used as insulation. It is of interest that a high porosity channel black, packed to an apparent density of 0.2 g./cc., has a room temperature thermal conductivity of only 2×10^{-4} watt/-cm./°C. [62].

Pyrolytic Carbon

History and Method of Preparation: Recent accomplishments in the production of massive pyrolytic carbon (PC) shapes constitute an important chapter in materials science and technology. PC can be defined as that material produced from the decomposition of a carbonaceous gas and deposited on a substrate. Most commonly, the gas is methane or natural gas, but acetylene, carbon suboxide, and other gases

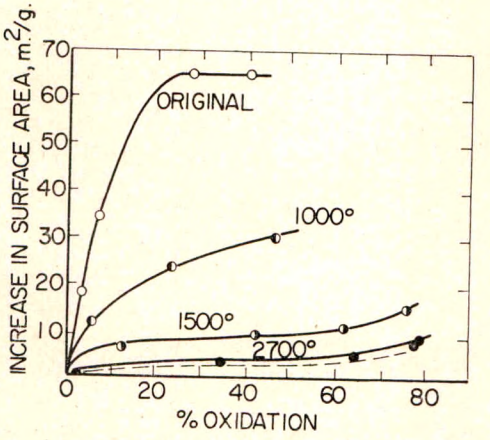


Fig. 17. Effect of heat treatment on the efficiency of activation of carbon black [60].

have been used. PC is an old material. Sawyer [63] described its formation in a patent in 1880. At about the same time, Edison was depositing PC on lamp filaments. In the early 1940's, Christensen [64] described the formation of carbon films which could be used as electrical resistors. Such resistors are in wide use today.

In 1956, the status of PC was this—a material made in relatively thin films on relatively small substrates. Comparatively little research was in progress on its preparation or properties. But the need for a high temperature material of unusual properties in space vehicle structures, such as thermal nose shields, rocket nozzles, and leading edges, changed the situation abruptly. Large-scale research and development studies over the last five years have led to the satisfactory production of massive PC bodies of at least ¾ inch thickness and many square feet in area.

empire building. This impediment to voluntary realignment is often removed through the medium of study commissions which may provide logical reasoning and recommendations for the realignment.

Method of Support

A problem which must be met in the broad consideration of the expansion of the support level to scientific and technological research and development is not only how much and how it should be distributed, both within and outside the government, but the effect on the possible distortion of government, university, and industrial programs and establishments. In many of our major research and development programs in the future we may find it desirable to divide and assign various phases, through cooperative agreement and conference between the government, university and industry, and to pool and divide our resources. Broad grants may be more advisable in basic research programs, and, in fact, these grants may extend to cover the major operating costs of universities and research foundations [28].

It does not follow that an institution which engages in research is a university. The relation of research to a university function is primarily in the creation of research workers through training, education and experience. Industry, the government, and research foundations may all engage in research without taking on the characteristics of a university and, in fact, the university may, in such operations as contract research and development, be exceeding its basic function when there is not a reasonable portion of such activity directed towards development of new research workers. Several of our major universities have indicated in their fiscal reports that more than half of their annual expenditures are in non-academic contract work for government agencies. One university indicated that it had over 1700 contracts with the government and was signing new ones at the rate of five a day. Another of our leading universities indicated that 82% of its budget was in non-academic programs. One may well ask if there is a limit to the extent to which a tax-exempt educational institution may engage in non-academic activity without developing a "conflict of interest" situation. The American Chemical Society, as a gesture in support of education, sells its rather expensive *Chemical Abstracts* at about one-fourth the cost of production to *bona-fide* educational institutions. Its definition of a *bona-fide* educational institution is an institution that spends at least 50% of its budget on academic affairs. It is now faced with a question as to whether its definition of a *bona-fide* educational institution should be changed, for a number of our leading universities fail to qualify by reason of their large business activities. Dr. Herbert Longenecker [29], President of Tulane University, has recently pointed with emphasis to the "Bureaucratic Chaos" produced by contract programs emanating from a wide

These bodies have very desirable properties at elevated temperatures, such as high strength, high emissivity, low permeability to gases, and large anisotropy in their electrical and thermal properties. Much research is now in progress on upgrading the properties still further by producing alloys of pyrolytic carbon and co-deposited metals.

Mechanism of Pyrolytic Carbon Formation: For PC of satisfactory properties to be formed, it must be deposited from a carbonaceous gas which is at a much reduced pressure. Operation at pressures as low as 10^{-3} atm. is common. At low pressures, the gas phase reactions which

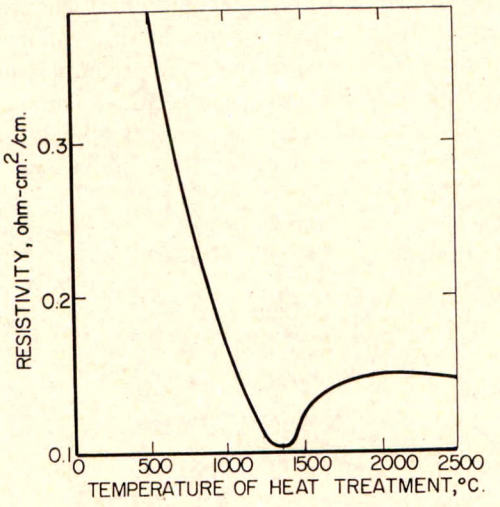


FIG. 18. Effect of heat treatment on the electrical resistivity of carbon black [52].

produce carbon black are minimized, resulting in an increase in the density and ordering of the crystallites in the carbon deposit.

The intermediate steps involved in the conversion of a carbonaceous gas to PC are not well understood. Usually, the substrate (frequently bulk graphite) is heated to a temperature considerably in excess of that of the entering gas. Depending upon the difference in temperature between the substrate and entering gas, the gas pressure, gas flow rate, geometry of the system, and the diffusion coefficients and accommodation coefficients of the gas and its decomposition products, the entering gas will be converted to carbon at some rate and with certain properties. Obviously many dehydrogenation and condensation reactions of the parent hydrocarbon and its decomposition products occur on and close to the surface once the gases have been brought to sufficiently high temperatures following multiple collision with the heated substrate.

The properties of PC films are also determined by the mobility of the carbon fragments on the growing surface at the time of their deposition. The higher their mobility, the larger the crystallite size and the better

variety of government agencies. He noted with regret the tendency to separate research from the academic programs; and he urged an advisory commission to try to develop long-range policies to enable federal aid to education to be given according to well-planned programs which will not distort our academic structure.

The recent article in *The Reporter* magazine [30], on the "Affluent Professors" may to some be an exaggeration, but it describes a real effect. Just as universities may distort or impede their proper function by engaging in too much extra-curricular or conflict-of-interest activity that is not in their mission, so the faculty may also become similarly involved. There are many who feel that a university is essentially its faculty. If the faculty is free, like the university, to engage privately in contract research consulting, operate private research organizations and otherwise contribute their time and energy to activities which are distinct from their academic duties in the university, we may again have another conflict-of-interest area. Just as some universities may be devoting the major portion of their budget and effort to non-academic contract operation, using as an operating base their university title and tax-exempt status, so also the faculty in many cases is engaged in a similar activity with the professorship as a similar prestige position. With good reasoning, a professor can point to the similar activity of his employer as a proper justification. The special privileges of "no conflict of interest" for universities and their professors, and the rigid application of conflict of interest to government agencies and scientists need to be studied along with the long-range planning for a national science program.

The performance of governmental functions by non-governmental agencies has been the subject of a number of Congressional hearings and has been reviewed in a recent article in *The Reporter* on "Ideas, a New Defense Industry" [31].

A problem which will have to be resolved in time is the extent to which the recipient of a grant-in-aid or contract has any responsibility to the donor. There are those who feel that there is no responsibility accompanying the funds for research and development other than the responsibility of the government to provide the funds. On the other hand, it is apparent that science and technology have become a major and integral part of a national program and policy and that scientists do and should have a hand in the formulation of these policies and programs. If scientists participate in the formulation of our policies and programs, then they should aid in the support and promotion of these policies and programs.

Within the government there should be a realignment of those areas in which the government itself engages in basic research so as to provide a greater coordination between these areas and the support which the government may provide to other workers in the same general area of

the crystallite orientation which is attained, because of the ability of the fragments to move to preferred positions of low potential energy rather than remain at the sites at which they are deposited.

Hirt and Palmer [65] have reviewed results for the rate of formation of PC from methane for temperatures up to 1100°C. They find that the temperature coefficient is large; in terms of an apparent activation energy it yields a value of 104–107 kcal. This is close to the dissociation energy of the first C-H bond in methane and suggests that homogeneous

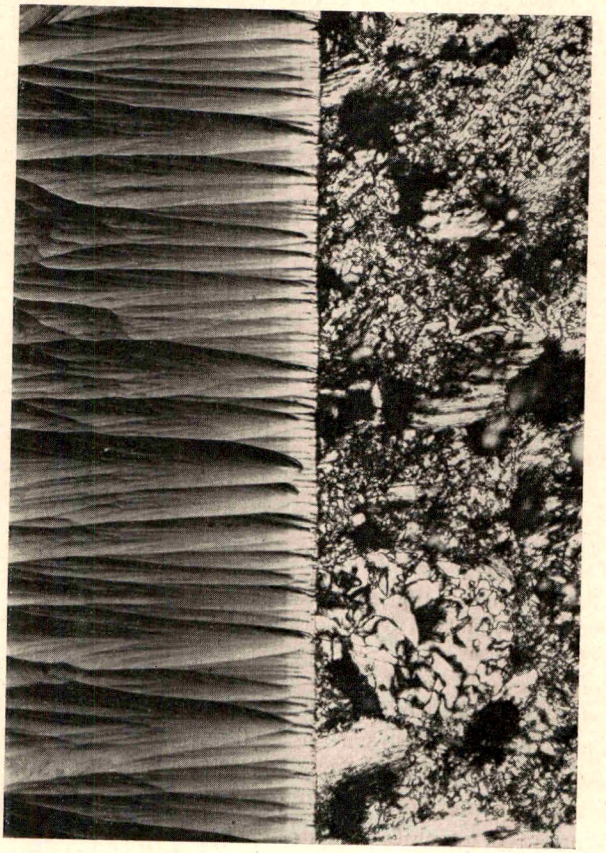


FIG. 19. Photomicrograph of pyrolytic carbon deposited on bulk graphite, magnification 250× [68].

unimolecular decomposition of methane in the gas phase determines the deposition rate. In their studies on PC from carbon suboxide (C_3O_2), these workers find an activation energy of 54 kcal., which they feel is a reasonable figure also for the dissociation energy of C_3O_2 into $C_2O + CO$.

Structure of Pyrolytic Carbon: The crystallites in good quality PC are well aligned relative to each other and, in turn, aligned with their

science and technology. The government's development programs for supply and hardware should be primarily directed towards industrial contract operation, with a reasonable amount of a parallel internal government program of a pioneering character so as to insure the government's internal competence for the guidance and assessment of a contract operation.

The need for coordination of the many scientific and technical programs supported by various governmental and non-governmental agencies is particularly apparent in our foreign operations, where the mission or motive of the supporting agency may be somewhat obscure and the need or reason for the program may not be obvious. The some two hundred million dollars which this government spends abroad each year on the support of foreign research and development programs often is motivated by political or social rather than scientific objectives.

Coordination of these programs between agencies, and with non-governmental and foreign groups with concern in these areas, would materially improve their effectiveness and in some cases might lead to elimination of unnecessary or undesirable activities.

Support to universities for basic research should be coordinated with other educational assistance programs and, where practical, should avoid specific contracts for item work [26]. Priority consideration should be given to the establishment of a broad university grants system of support. The government should not use non-government operation, especially tax-exempt organizations, as a mechanism for avoiding government regulations in the operation of laboratories or services which are unique to the government and which are not a logical function of the contracting agency. If government regulation is inimical to an efficient scientific laboratory or service, then steps should be taken to change our regulations, if practical, so as to make such operation possible.

The government should strengthen its own laboratories which provide unique services to the public, such as weather, standards, census, patent, mapping, food and drugs, documentation and others as well as those agencies which provide internal advice and service with respect to the government's own procurement and operation.

As science advances and changes there will be some areas which expand, some which contract, and others which may remain essentially static. Only when one approaches a ceiling of availability of personnel, funds or facilities does one have to consider seriously relative priorities. Until recently, we have been in the enviable position of almost limitless boundaries in the operation of science. We were in much the same status as the early pioneers in this country with respect to the conquest and settlement of virgin territory. Today, there is little free land which is available for homesteading. We are also reaching a position in science with respect to personnel and perhaps funds where there appears to be

layer planes closely parallel to the substrate surface. As a result of good crystallite alignment, PC can be formed with apparent densities as high as 2.22 g./cc., which approaches closely the density of ideal graphite. The density of PC varies markedly with deposition temperature at higher gas pressures. For example, Brown and Watt [66] find a steady increase in density from 1.18 to 2.22 g./cc. for formation of PC from 0.2 atm. of methane between 1700 and 2100°C. Diefendorf [67] finds a similar result for deposition from 0.05 atm. of methane, but does find that, at a pressure of 2×10^{-5} atm., the density remains at about 2.2 g./cc., independent of substrate temperature. Deposits of low density

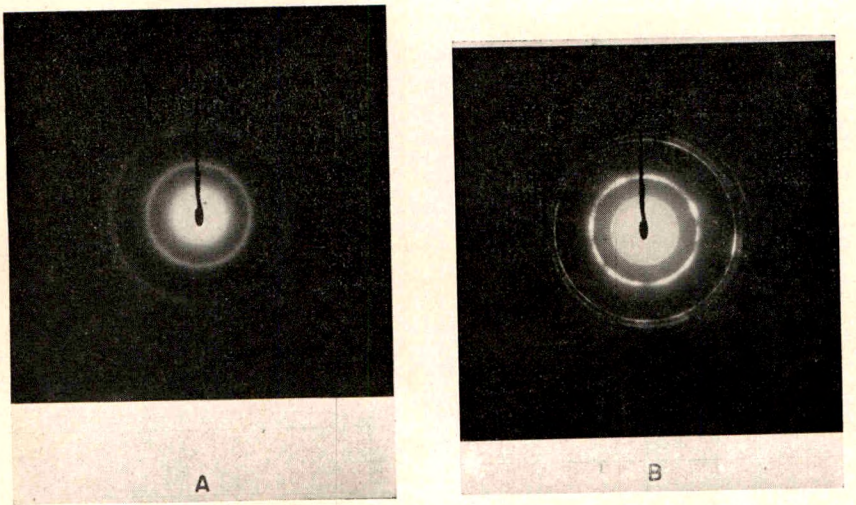


FIG. 20. Transmission electron diffraction patterns showing effect of substrates on development of crystallinity in pyrolytic carbon. (A) porcelain, copper, or platinum and (B) nickel [71].

are attributed to the formation of large carbon black particles in the gas phase and their inclusion within the film.

Figure 19 is a micrograph of PC deposited on bulk graphite at 1850°C. from 0.5 atm. of methane [68]. The difference between the porous, randomly oriented graphite body and the dense, homogeneous PC is evident. A finer grained PC is often desirable. It can be made readily at higher deposition temperatures and lower gas pressures.

The crystallite size of PC depends strongly upon the deposition temperature [69]. Using a bulk graphite substrate, the average crystallite sizes of PC produced from methane are below 100 Å. for deposition at 1700°C. and increase to about 150–300 Å. for deposition between 1900 and 2500°C. Heat treatment of PC between 2500 and 3000°C. further improves crystallite alignment and increases crystallite size. That PC is highly graphitizable is to be expected because of its good

a finite limit imposed by population growth and the expansion of our gross national product. Before we reach these definitive limits we should have firmly established some procedures for logical assignment of priorities and support.

Just as we indicated that other examples could be used such as funds or facilities, as well as personnel, to indicate the approaching equilibrium limit to the expanding rate of science and technology, so also there are other areas of utilization in addition to government organization which should be studied. Such areas as our educational programs, research organizations and industrial activities would also be subject to review as to procedures and organization much as we have suggested in the area of government science.

Who Speaks for Science and Technology?

We have made no attempt to indicate specific governmental shifts or reorganizations, although many such suggestions have been made in various proposals by others for a reorganization of the government's programs. Such proposals and ideas deserve serious consideration by any panel or commission which may be established for the review of the national science and technology problem.

A recent article in *Science* [32] has questioned the narrow interpretation of the field of science and has indicated a desire to include areas in social, economic and health studies, under a broad coverage of science. On the other hand, there has been a tendency within science itself for small groups or representatives of limited areas to assume the mantle of leadership and act as the spokesman for all of science. The choice of commission membership needs to be made with considerable care to insure that there is a reasonable representation of science and technology in both depth and breadth. The areas of utilization including government, university and industry must be considered as well as the supporting administration in these areas and the national organization of professional status in science and technology.

The present form, composition and assigned functions of the National Science Foundation, the President's Science Advisory Committee, Advisory Committees of the National Academy or Interdepartmental Committees do not provide the essential representation from government agencies, universities, industry, foundations, and government administrators which is desirable and necessary for a proper study commission.

It is not out of the question to expect scientists and others to exercise a broad and understanding appraisal towards areas outside of their immediate area of specialization and the extent to which such understanding is developed among members of a survey commission will be an important factor in the success of such a commission.

crystallite alignment. For example, heat treatment of PC (formed at 1000°C. from anthracene) to 2650°C. produced a carbon having an interlayer spacing of 3.3567 Å., or a fraction of displaced layers of only 0.1 [70]. It is found that, in PC heated to 3250°C., crystallites are aligned to better than 1° [69]. Such material can be classified as mosaic single crystals. The availability of such material in convenient shapes should be of considerable importance in studying the solid state properties of graphite.

The substrate on which PC is deposited can also have an effect on its crystallite size. PC films were formed from carbon suboxide at 700°C. over porcelain, copper, platinum, and nickel substrates [71]. Figure 20a is a transmission electron diffraction pattern of the film deposited over the first three substrates. Diffuse halos, corresponding to a small crystallite size, turbostratic carbon are found. On the other hand, the diffraction pattern of the carbon deposited on the nickel substrate (Figure 20b) is characteristic of a material of substantial crystallite size having extensive layer plane ordering. Similar diffraction patterns for the carbon films deposited on the former substrates are obtained only upon their heat treatment to temperatures in excess of 2000°C.

Electrical and Thermal Properties of Pyrolytic Carbons: Because of the high degree of crystallite alignment in PC, its electrical and thermal properties are highly anisotropic. Consider good PC deposited around 2500°C. Typical room temperature electrical resistivities are 3×10^{-4} and 0.5 ohm cm., respectively, in directions parallel to and perpendicular to the substrate surface. Thus the ratio of electrical resistivities is at least 1000. Typical room temperature thermal conductivities are 3 and 0.03 watts/cm./°C., respectively, parallel to and perpendicular to the substrate surface. The ratio of thermal conductivities is about 100, which is considerably higher than that reported for single crystals. It is suggested [22] that, in single crystals, the thermal conductivity perpendicular to the layers is relatively high because of spiral dislocations which provide paths between the layers.

With the very high anisotropy in thermal conductivity of PC and some of its other properties, it makes an almost ideal nose cone material for space vehicles. On account of air friction, a nose cone is subjected to large amounts of heat concentrated in a small area. Carbon has a high emissivity and thus efficiently loses by radiation some of the heat produced. The remainder of the heat is primarily lost by conduction away from the cone (parallel to the surface) to a large area of the space vehicle where it, too, is radiated away. Little heat travels through the nose cone to damage both sensitive instruments and lower melting-point materials.

Because of its negligible porosity, PC has a very low permeability to gases. This has prompted the coating of uranium carbide fuel particles (used in nuclear reactors) with PC. The carbon serves joint functions as a

The discussions as to whether there should be a Department of Science, Education or Culture are in part real and vital and in part pedantic. There probably is little that a Department of Science could do in the nation which could not be done by a properly coordinated and staffed Science Foundation, National Academy, National Research Council, Association for the Advancement of Science, President's Advisory Committee, Inter-departmental Council or private foundation. The problem rests to a considerable extent on what group or groups should exercise how much authority and direction. It is precisely to resolve this administration problem that we recommend a broad commission to study the question rather than the arbitrary assignment of the problem to an existing agency, especially if the coordinating agency represents only a limited area of science or administration [1, 30].

SUMMARY

It is obvious that we are entering a new era in both science and technology and that it is desirable to take stock and evaluate the growth of science, our methods of operation, subsidy and direction of emphasis. Support has been given to a review of our national and governmental science organization and science policies by a study group. Such a study group should include career scientists and administrators from the legislative and executive branches of the government together with representatives from universities, industry, scientific societies and foundations to study these problems. Among the subjects which should be considered are the nature of the government's role in the support of research; the type of personnel essential to the government's programs; the mission, composition, and function of various organizations or agencies; and the advisable changes considered essential to create an over-all stability and improvement to our national scientific and technological program.

A National Science Program, to be effective and successful, must have adequate support in the form of confidence and representation from the scientific community, including the scientists and administrators in the government, universities and industry.

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moderator of fast neutrons and as a coating which captures dangerous fission by-products, thereby preventing them from getting into the main coolant stream.

Other Carbons

There are many other ways to prepare carbons with interesting and different properties in addition to those already described. Some of these approaches will be discussed briefly.

Evaporated Carbon Films: Bradley [72] developed a technique for producing thin carbon films by the rapid evaporation of bulk carbon. These films are very satisfactory as replicas of surfaces in electron microscopy studies. Carbon is evaporated by passing a large current through two pointed carbon rods, with the points held lightly together. Intense local heating at the points results in evaporation of the carbon, some of which travels to and deposits on a substrate some distance away. These films consist of small crystallites.

Surprisingly little has been reported to date on studies concerned with modifying the properties of evaporated carbon films, even though many appealing possibilities exist for doing so. The crystallite size, crystallite orientation, and film purity can be varied by changing the rate of evaporation, distance between the source and substrate, substrate temperature and material, and type and pressure of atmosphere in the evaporator. Films can be doped with metallic impurities by their concurrent evaporation. The structure of films can also be changed by heating them to elevated temperatures following evaporation [73].

Dietz and McFarlane [74] showed the interesting possibilities of modifying the structure of evaporated carbon films by varying the pressure in the evaporator. They evaporated carbon onto a substrate held at 78°K. In runs 1 and 2, the films were prepared under vacuum and under an initial pressure of 100 microns of krypton, respectively. The film formed in run 1 had a characteristic metallic luster; that formed in run 2 had a dull luster and an opaque black appearance. Adsorption isotherms were run on the films at 78°K. The films prepared under vacuum had a surface area of about 1 $m.^2/g.$, compared to an area of about 1000 $m.^2/g.$ for the film prepared under a krypton pressure.

Artificial Graphite Single Crystals: Acheson [27] found that at temperatures above 2000°C. silicon carbide decomposes leaving highly graphitic carbon. He concluded that carbide formation is a necessary intermediate step in the graphitization of carbon. Even though this is now known not to be the case, it is true that carbon of higher layer ordering can be formed through the decomposition of certain carbides than can be formed, at equivalent temperatures, by graphitization of pure carbon materials. For example, aluminum carbide can be decomposed readily to form large single crystals of graphite at 2500°C. [17]. These

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crystals have an interlayer spacing of 3.3542 ± 0.0005 Å. at 15°C., or complete three-dimensional ordering.

Large graphite crystals can also be grown from an iron melt saturated with carbon. Cooling the surface of such a melt causes precipitation of single crystals that float to the surface. Purification of these crystals with HCl and HF at room temperature or heat treatment to 2800°C. yields a carbon with an interlayer spacing of 3.3540 Å. at 15°C. [9]. In recent studies, single-crystal graphite films were produced in graphite crucibles

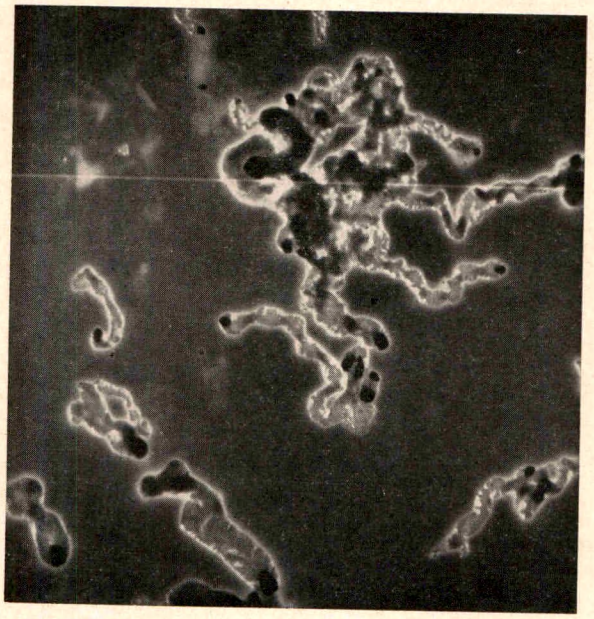


FIG. 21. Dark-field electron micrograph of carbon produced by decomposition of carbon monoxide over iron, magnification 25,000× [77].

on the surfaces of melts of tungsten, zirconium carbide, nickel, chromium, rhenium, and molybdenum [75].

Colloidal Carbons: Carbons that resemble carbon black can be formed in flames in which the hydrocarbon is reacted with chlorine instead of oxygen [76]. In the case of methane, carbon is produced by the over-all reaction, $CH_4 + 2Cl_2 \rightarrow C + 4HCl$. The carbons produced contain varying amounts of chlorine depending upon the incoming chlorine-hydrocarbon ratio. For example, for ratios of 1.3 and 2.0, carbons containing 9.4 and 45.1% chlorine, respectively, can be formed. The "chlorocarbons" are black in color, have no appreciable odor, and resist oxidation better than the usual carbon blacks.

Colloidal carbon can also be prepared using a plasma flame. That is, gas can be heated to very high temperatures when passed through an electric arc. A hydrocarbon can then be added to the hot primary gas

COOPERATION—A RESPONSIBILITY OF THE SCIENTISTS

By EDWARD R. WEIDLEIN

IF ONE were asked to name the most dynamic force in the known universe, he might grope for some time before he made a hesitant choice, wavering between such imponderables as the power of the sun and the energy hidden within the atom. His reluctant choice in any event would probably be wrong—for the most powerful force known to man still is an idea. Again and again it has been the occasional forceful idea that has been followed to remake and reshape the world we know. Some of these ideas have been good. Some have been wrong or misguided. Some have burst upon the world with disturbing force. Others have had to grow, to be nurtured, before they were recognized as great.

Mellon Institute is the story of an idea, evolved in the brain of one man, an idea that gripped the minds of other men, who gave it their support and helped it to grow, so that today it stands accepted by all industry and science as a living force. Fifty-five years ago, in 1906, Dr. Robert Kennedy Duncan, while attending the Sixth International Congress of Applied Chemistry in Rome, developed his idea which seems simple as we regard it today. It was, briefly put, to be an understanding partnership between American industry and science. He arranged for the first Industrial Fellowship at the University of Kansas on January 1, 1907. It brought results. His views attracted attention and industrial interest. In 1910 he was called to the University of Pittsburgh to establish a similar research department. Mellon Institute was established in 1913 but, unfortunately, Dr. Duncan died in 1914.

Before the Institute was established, Andrew W. Mellon and Richard B. Mellon saw the merit of scientific research for the benefit of mankind through the development of industry. In general, however, American industries of that day were not applying scientific research for the solution of their problems. Nor were these industries fully aware of what such research might mean to them. The Institute was established for the purpose of giving this aid.

This venture of scientific research very soon proved its value. New industries, new materials, and new processes came into being. Industries and the whole estate of man benefited. Yet the Founders saw beyond these results. It seemed to them that there were needed researches in science that would have a more direct relation to human welfare, which might also advance our basic knowledge of technology. Such fundamental work was started, and the entire program through these many years has been sustained by the surviving members of the Founders' families. Their policy has been to place greater emphasis on basic research, and

stream, to be heated up and decomposed to carbon. This is an interesting process, since such carbons can be formed at temperatures above 2000°C. in the absence of oxygen.

Carbon of highly interesting properties (called CMC) can be formed by the decomposition of carbon monoxide-hydrogen mixtures at atmospheric pressure over finely divided iron [77, 78]. The temperature at which the maximum deposition rate occurs depends upon the CO-H_2 ratio, but a temperature between 500 and 600°C. is commonly used. Figure 21 is a dark-field electron micrograph of the raw carbon (78%C-22% Fe) formed at 528°C. from a 98.6% CO-1.4% H_2 mixture. The carbon frequently grows as two filaments, sometimes tightly twisted into a rope and other times, untwisted. At the end of the carbon growths, there invariably is located a particle of iron which presumably is the

TABLE II

COMPARISON OF CRYSTALLITE SURFACE AREA WITH GAS ADSORPTION SURFACE AREA FOR DIFFERENT CARBONS

Carbon	Temperature, °C.	Crystallite Size A. Height	Crystallite Size A. Diameter	Area, $m.^2/g.$ Crystallite	Area, $m.^2/g.$ Gas Ads.	Crystallite Area / Gas Ads. Area
CMC	528	87	174	200	144	1.4
Fine channel black	~1000	7	12	2800	1000	2.8
Furnace thermal black	~1200	17	28	1300	15	87
Petroleum coke	~600	25	35	900	0.5	1800
Film carbon	1000	21	30	1000	<0.1	<10/000

catalyst for the formation of the particular filament. The carbon has an unusually large crystallite size, considering its formation temperature; but the packing of the crystallites is very random and loose. Table II presents some data on the crystallite size and surface area (from gas adsorption of nitrogen at 78°K.) for a number of carbons which we have discussed. From the crystallite size and true density of each carbon, a crystallite surface area can be calculated. The extreme range in the ratio of crystallite area to gas adsorption area is seen. Most of the crystallite area is available to the adsorbate in the case of CMC. On the other hand, a negligible amount of crystallite area is available in the case of good pyrolytic carbon.

Because of its low crystallite alignment, it would be expected that CMC would graphitize poorly. This is the case. Heat treatment of the sample in Table II to 3000°C. only increased its crystallite height to 270 Å. and decreased its interlayer spacing from 3.373 to 3.371 Å. That is, the sample still had about 40% stacking disorder.

Molecular Sieve Carbons: Polymers can be pyrolyzed and converted

a very substantial trust fund has been established for this purpose.

The Institute has become a guild of scientists. Here are gathered many men trained in chemistry, physics, engineering, and biology. They help one another directly; and, even more importantly, they create an atmosphere of achievement for themselves in which there grow vision and determination for thorough, careful work. Their success is not accidental. It is the outcome of strong minds and orderly thought, of good will and of good fellowship, of seeking toward a great end. As one who has been associated with this system of applying science to industry since its inception at the University of Kansas, it is gratifying to know that a good idea received sympathetic recognition and support.

The Institute has, in a period of fifty-five years since the idea was conceived, achieved many tangible results which have won the esteem of science and industry. Yet, when one pauses to ponder the accumulated evidence of accomplishment, it does not seem enough to put one's finger at any point and say, "This is what it has done." For there are important imponderables to be taken into account. Often, no matter how striking or dramatic or distinguished the definite results may be, it is the imponderable achievements that make an organization great.

It may, for instance, prove ultimately less important that some industries have been aided to evolve better products than that the Institute's point of view toward scientific research has exerted a profound influence on all industry and on human life as well; or that the men it has trained have followed their researches out into the plants of industry and so reaped a great deal more than was ever expected; or, that, as the Institute's objectives and methods became understood and invoked trust, the general concept of the value of scientific research was lifted a little higher. Or, to carry the thought further, that through research in pure science the Institute has laid the basis for fundamentally new service to man.

It would be easy to cite the case of this fellowship or that, which has succeeded in all that it set out to do, and to let the record stand, and this might be the easy and obvious way of so doing. However, when trained minds and proper facilities are applied to specific problems, practical solutions are expected. If they were not forthcoming, that would be news.

The ideals and aims of Mellon Institute have always been to find basic knowledge about this chemical world in which we live and to give such fundamental knowledge to scientists and to the world for the benefit of all. Its unflagging interest in the why and how of nature has been spurred not merely by the fundamental needs of industry but by the need of more basic knowledge in such human fields as medicine and public health.

Industrial research in the United States received its first big impetus

into carbons of varying yields and structures. An interesting case in point is polyvinylidene chloride $(CH_2\text{-}CCl_2)_n$. Upon slow heat treatment to 600°C., the polymer loses hydrogen chloride and is converted into a carbon having a surface area of 1300 m.²/g. [79]. The majority of its pores have diameters in the range of 7–8 Å. Adsorption of hydrocarbons on the carbon is activated, with the speed of adsorption depending upon the size of the hydrocarbon [80]. It has been concluded [81] that this material has only about 25% of its carbon in the form of parallel layer planes. The remainder consists of single layer planes and disorganized carbon.

Conclusions

This discusssion has been concerned with some of the important forms of carbon—their preparation and properties. Even though carbon as a material has been used for many years, much research and development work will continue to be done in search of new and improved forms of carbon. There is much interest in producing bulk carbon bodies of densities more closely approaching the true density of graphite. Thus, attention will center on the mechanical compacting of carbon at temperatures above 2500°C., where creep occurs at a significant rate. For nuclear reactors operating at high temperatures, carbons with better volume stability are highly desirable, if not essential. Improved oxidation resistance of carbon for high temperature applications is necessary. The formation of large and pure single crystals of graphite from liquid carbon will be studied in great detail. The production of carbon-metal films having interesting electrical and magnetic properties shows much promise.

Progress in the development of new carbon materials and in the improvement of old carbon materials will continue to be recorded—as a chapter in the over-all story of materials science.

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as a result of our experience in World War I. During that war it became evident that our basic background in the application of science to industry was weak. It was the beginning of an understanding partnership between American industry and science. The importance of Duncan's idea was taking on new dimensions. In one of his papers published in 1911, 50 years ago, he relates that he secured with great difficulty the names and addresses of seventy-five corporations in the United States possessing bureaus of research or individual research scientists. To these organizations he sent questionnaires inquiring into conditions under which industrial scientists labored. In the light of present practice, the results of this questionnaire are astounding. The replies indicated that there was no general practice; that employment conditions of scientists were chaotic, and often verged, as far as pay and service went, on those of a common laborer.

Duncan's insistence on the importance of research in industry, the use of his ability to write and speak and publicize his views, has played an important part in helping to establish research laboratories in industry, several having had their beginning in Mellon Institute. New research institutes came into being, as well as research foundations in the universities. This advancement resulted in a rapid expansion of our scientific knowledge with steadily increased productivity; and the outcome has been new and better products and a higher standard of living for all of us. The objective of scientific research today, moreover, is broader than the solution of technological problems. It takes into view the responsibility for enlivening the imagination of the masses who will be the chief beneficiaries of these new ways of living. It is amazing how much more clearly most people see things at home and in Russia in the light of the satellites, the sputniks—a few people with sufficiently acute vision have been seeing what was going on in Russia and at home long before the sputniks, but most people, because they were unable to see it themselves, refused to believe what the few were reporting. Interest in pure research in our country has been greatly stimulated by the sputniks. We have also had reactions in the public, in the administration, and in Congress which are motivated by panic rather than by analysis of our situation and calm deliberation. This could lead to the expenditure of many millions of dollars on ill-conceived projects. Science in its highest form, however, never advances without its results benefiting humanity sooner or later. Science may hold the balance of power between peace and war; certainly, with proper support, it will try most seriously to improve the lot of mankind.

Creative thinking and new ideas in science are still among mankind's most vital resources. We depend on science for our material welfare; we depend on it for our industrial advancement; and, indeed, we depend on it for our very existence in these days of international tension.

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An adequate number of truly creative scientists and engineers educated to the highest levels of competence may be more important to America than number. Actually, the great new ideas, the breakthroughs in both pure and applied fields have come from a relatively few individuals. How many ordinary physicists would it take to make a contribution equivalent to that made by Fermi? The discovery of the principles of electromagnetic induction by Faraday ranks with the great discoveries of all time. It led to the technological foundations upon which much of the electrical industry rests. Maxwell and Hertz, through their basic research, established the foundations for radio and television. Even the invention of new scientific instruments plays an important part. The most dramatic example of this kind is the immense stimulation given to nuclear physics by the invention, some thirty years ago, of machines that accelerate atomic particles.

These are only a few highlights. Anywhere you look you find material things of today's civilization based on some individual's idea. The basic ideas of a few have created a very great demand for engineers and scientists in our working force. This has created a temporary shortage of scientists and engineers. However, we should still focus more attention on quality and less on quantity. The fact that Soviet Russia is training engineers at three times our present rate does not automatically mean that we should equal or exceed her rate. In our system we require an economic balance. In order to use more technical people, we have to learn how to employ them effectively.

Perhaps the longest distance in the world lies between a discovery in a research laboratory and its translation into a vast, sprawling plant. Before the discovery can exert any influence, it must be placed in people's hands. This is the all important task of the industry's applied scientists and engineers. Yet, in other ways, including process research and development, technical sales and service, and management activities, scientists are also helping to shape a better world.

These new areas of science expand into fields that give the fundamental scientists and the industrial scientists an equal opportunity for creative work.

The fundamental scientist's objective is to develop new scientific facts without regard to specific commercial goals. Often this has little or no immediate value. It is thus distinguished from applied research, which uses such knowledge in the solution of immediate practical problems. It is fundamental because it develops the very roots of scientific knowledge, and because it lays the foundation for invention which stimulates the whole economy by creating new industries and new jobs.

The silicone industry is a good example. The first silicon compounds were prepared in a university laboratory in England around 1900. These products were studied occasionally but not until 1944, after extensive

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research by the applied scientists, were products developed that possessed commercial value. A new industry was created. During the war, production was confined to a few materials with essential military value. Since then, the line of new products has been expanded and the productive capacity has been doubled and redoubled. Other chemical industries have also entered the field.

A very interesting sidelight to this is the establishment of a medical research center by the original silicone company to serve the medical profession on a nonprofit basis, by providing technical aid in the use of silicones in medicine and surgery. The foundation's reports cover such interesting results as artificial heart valves, correction of dural defects, use in connection with perforations of eardrums, correction of detached retina, and relief of intestinal bloat.

The scientific knowledge which has made possible modern industry was acquired slowly for the most part in the universities of the world, by men who were stimulated by curiosity. The popular conception, however, that most successful scientists follow a career of stumbling upon important discoveries is not true. The discovery at the Bell Telephone Laboratories of the semiconductor phenomena, which made possible the transistor, was certainly not an accident, but the result of the kind of orientation which a creative environment can impart to a basic research inquiry. The long and distinguished tradition of this laboratory in the fields of communication and electronics thus provided an altogether fitting and probable birthplace for this important discovery.

Basic research can be defined where the primary aim of the investigator is a fuller knowledge or understanding of the subject under study, such as the missile program, cancer or heart disease. One of the clearest lessons to emerge from the history of science is that various scientific disciplines, seemingly unrelated, have a way of stimulating each other in an unexpected manner.

The time arrived a few years ago when our universities were no longer prolific enough to fuel the developing scientific revolution with the necessary advances in basic research, so industrial laboratories had to push into these untraveled regions where there was no sure guarantee of financial reward. There are many good reasons for establishing pure research departments by industry, the most important of which is the coupling of basic research with applied work. Of equal importance is the coupling between science and national defense so as not to impede the flow of new scientific knowledge toward defense technology. This does not mean that industry is no longer supporting fundamental research in the universities. On the contrary, industry is giving greater support today than ever before. This is not only important to develop new scientific data, but better to train the scientific personnel for industry.

Industry is a very broad term covering a vast array of human activity,

THE ROLE OF MATHEMATICS IN THE RISE OF MECHANICS*

By SALOMON BOCHNER

IN THE present article we will make some observations on the mathematical nature of mechanics during some phases of its development, and we will be concerned with the inward mathematical structure rather than with the outward mathematical setting. But we will not be able to go essentially beyond the beginning of the nineteenth century. After that time, mechanics and paradigms from mechanics began to penetrate into many other areas of theoretical physics, so that observations on the mathematical nature of mechanics after the early nineteenth century would have to take large parts of theoretical physics into account. But we are not prepared to offer a commentary of such a scope.—Also, in the seventeenth and eighteenth centuries mathematics and mechanics were developing not only in constant interactions but also in close intimacy. But, in the nineteenth century, relations between mathematics and mechanics or rather between mathematics and theoretical physics began to be different. The interactions continued to be no less in importance, but they began to manifest themselves in parallelisms of development rather than in direct interpenetrations. An analysis of their new relations would require categories of description other than those applicable to previous stages.

Modern mechanics came into being soon after the Renaissance towards the end of the sixteenth century and its stages of development, taken by centuries, have been as follows. The seventeenth century in mechanics extended from Stevin to Newton and culminated in Newton's *Principia*. The eighteenth century in mechanics extended from Newton and Leibniz to Lagrange and culminated in his *Mécanique analytique*. The nineteenth century in mechanics extended from Lagrange and Laplace up to the first stirrings of quantum theory in 1900, and in a certain qualified sense it culminated firstly in Maxwell's *Electrodynamics*, inasmuch as the latter theory imitated hydrodynamic wave theory, and secondly also in leading treatises on statistical mechanics. In the twentieth century the leading new theories of mechanics have become, more than before, direct and full representatives of the new physical theories of which they are a part and can hardly be isolated from them for separate analyses.

We venture to characterize the past centuries of modern mechanics by the following headings. The seventeenth century was an age of revelation, the eighteenth century was an age of patristic organization and the nineteenth century was an age of canonical legislation. We dare not continue and suggest that the twentieth century is an age of reformation, or perhaps an age of reformation and counter-reformation in one.

* This research was supported by the Air Force Office of Scientific Research.

and not all segments of industry afford equal opportunity for exploiting the results of scientific research. The chemical, electrical, communication, electronic industries, and others have all had their origins in the laboratory. Never before has scientific research done so much in so little time as during the past ten years. The expenditures are described today in billions of dollars instead of millions of dollars as was the case before World War II.

By catapulting the scientist into a position of greater prominence on the public stage, the atomic bomb demands of him greater stature and steadier courage. He has more than ever an obligation to stress the importance of science to social progress and the power of science to add to the dignity of the human spirit.

We see constant changes all about us in such fields as transportation, communication, food, fuels, shelter, clothing, health services, and education. In every field competition is keen. Mass transportation in our cities is a real problem, and, in fact, travel service is undergoing a rapid change affecting the railroads, airplanes, and automobiles. This requires a vast amount of research, not only to provide faster, safer, and more comfortable service to the passenger, but it means intensive research on the part of industry to make equipment more attractive to the user.

A look at agriculture shows that scientific research, not governmental controls, is the mainstay in keeping agriculture solvent. Future food production goals would be impossible without modern chemicals. The achievements in this field during the past ten years have been highly dependent upon research in mechanization, fertilization, and chemicalization.

Synthetic fibers blended with natural fibers have added a quality and variety to textiles which make it possible for more people to be better dressed at less cost and, also, provide textiles for household use of better appearance and wearing properties.

The synthetic rubber industry has saved the American people many millions of dollars. It was a war development made possible by American scientists and engineers who had gained their specialized knowledge through years of research and through the expenditure of several millions of dollars by the American rubber, chemical, and petroleum industries. The fusion of their many scientific skills, along with the financial support of the government, materially reduced the time factor for the expansion of a laboratory process to large-scale commercial production from the usual period of ten to twenty-five years to a period of only two years. Today the synthetic plants are in the hands of private industry, and more rubber of better quality is being produced than at the peak of the war effort. We are constantly becoming more independent of the need for natural rubber.

The fuel problem is being attacked by studies of atomic and solar

Before the Seventeenth Century

In Greek antiquity, hellenic and hellenistic, if viewed in retrospect and if compared with the seventeenth century, say, there seemingly was an imbalance between mathematics and rational mechanics. The Greeks had a systematic mathematical astronomy but outside of this they seemingly did not arrive at a theoretical mechanics or theoretical physics that was equal in status to their mathematics. Their mathematics, whatever its limitations, had the size and organization of a system. Their mechanics however was only an assortment of achievements, whatever their eminence.

Some of the difficulties that kept Greek mechanics from developing into a system may have been linked to peculiarities which kept Greek mathematics from advancing beyond the stage at which it was arrested. Anybody who appraises the mechanical work on the balancing of a lever in Archimedes and in his successors, hellenistic and medieval, cannot fail to notice that in some covert and tacit manner each of them had the concept of the statical moment in his reasoning, but that none of them reached the point of formulating the concept expressly as the mathematical product $P \cdot L$ in which the factor L represents the length of an arm of the lever and the factor P represents the size of the weight which is suspended from it. Now the concept of statical moment and of other moments became a great leaven in modern mechanics, and one might be tempted to ask how it came about that the concept of statical moment was not expressly conceptualized long before the seventeenth century and especially why Archimedes himself, great intellect that he was, was not forcibly led to introducing it. Now, a historical question like this may be deemed to be methodologically not admissible. But, in fact, if it be admitted, then a specific answer to it quickly suggests itself. The answer is this that even if Archimedes the physicist would have "wanted" to introduce the statical moment expressly, Archimedes the mathematician would not have been able to do so. If L_1 and L_2 are two quantities of a certain kind, say both lengths, and P_1 and P_2 are two quantities of another kind, say both weights, then Greek mathematics could envisage the proportion

$$P_1 : P_2 = L_2 : L_1$$

and decide whether it does or does not hold for special values of the quantities at hand. But it could not convert the proportion into an equality

$$P_1 \cdot L_1 = P_2 \cdot L_2$$

and verify its validity in this form. Greek mathematics could not or "would" not form a product $P \cdot L$. If P and L are general real numbers it could not form the product because it simply did not have real numbers. And if P and L were two general magnitudes, even if it did "have" the

energy sources which are being heavily supported by industry.

Research is vital to the success of an organization but it must always justify its existence. We know that the stability of our economy and of our civilization is dependent upon continued growth and improvement.

There is a widespread belief that research is a process which only the larger, richer companies can afford, and that small businesses are thereby barred from many new fields of enterprise. This is far from being true. There are available to the small industry consulting laboratories, research institutes, university facilities, and the opportunity to join with similar industries to establish an association research laboratory.

The importance of strong and continued private support of science can scarcely be overemphasized. As in the past, private foundations, either by operating facilities of their own or by supporting university research, can move with a speed and boldness that government will always find hard to initiate. The private foundation is especially able to attract administrators, deeply devoted to science, who can make a rewarding career of searching the universities for exceptional young men, and exceptional programs that need and deserve developing.

The expansion of industrial research is very noticeable by the number of new laboratories dedicated every year, and the building of separate laboratories adjoining the applied facilities, devoted to fundamental research. But there is still room for universities, institutes, government and consulting laboratories as partners in cooperative research with industry.

The endless frontiers of science now stretching to the stars can provide rich opportunities for the best creative minds. They must be given the opportunity to contribute to science.

magnitudes separately, it still would not conceive of forming their product, especially if the factors P and L represented magnitudes of heterogeneous kind as weights and lengths are. And so Archimedes the physicist and generations of physicists after him could not have a statical moment in mechanics as they should have had.

In a certain sense, and again as viewed from the seventeenth century, the quick answer we have just given does no more than raise another question in the place of the first. If all that was wanting was the mathematical conceptualization of a product like $P \cdot L$ then why was Archimedes, the great mathematician, not driven to doing just that? To this question, if it be not ruled out as inadmissible, there is no quick answer. In fact, in the seventeenth and eighteenth centuries, if mechanics would edge up to a conceptualization which the then mathematics could not articulate then mechanics would simply bid mathematics to learn to do so; and mathematics, after having learned to do so, would react on mechanics in return by suggesting to it more than originally required of it. In other words, mathematics and mechanics were then erecting and exploiting parallelisms and correspondences between mathematical and mechanical entities, conceptions and articulations. The opening pages of Newton's *Principia* are impregnated with such parallelisms. They exasperate expounders of the *Principia* who insist on hierarchical orderliness and unequivocality of meaning in depth, but they also generate the vigor and the speed with which the stream of assertions and implications rushes by.

S. Sambursky in his recent book on *Physics of the Stoics* (1959) rightly observes in passing that Stoic attempts at building up a mechanics of continuous media ultimately failed because, among other reasons, mathematics did not aid them. In this Professor Sambursky is right. But we wish to qualify his finding by adding that, to a Stoic physicist, Archimedes was a mathematician, and a mathematician only, and not somebody who could also be a mathematician and a "theoretical" physicist in one.

The Greeks did not themselves introduce mathematics as a technique of mastering problems that arise in the consciousness and thinking of man. This had been done by others before, as the Greeks themselves fully acknowledged. But they did recognize in mathematics a universal arché of knowing. They so proclaimed it to be and they would not have allowed this universality to be qualified by differences of eras and of philosophical settings. We think therefore that it is legitimate to probe, even in retrospect and even against the different philosophical spirit of the seventeenth century, the extent to which the Greeks themselves succeeded in realizing and exploiting the force and effectiveness of this universality.

Archimedes also accomplished basic work, perhaps his most famous one, in the mechanics of floating bodies. Here again he did not introduce

THE STATUS OF THE STUDY OF ORGANISMS

By GEORGE GAYLORD SIMPSON

"The goal of biophysics is to formulate an equation and state the boundary conditions of a given living system. The working out of the equation under the given conditions would predict the future behavior of the organism."

"Biology starts with biochemistry and goes on to neurophysiology and genetics. All else is stamp collecting."

"Genetics, the central discipline of biology, has as its ultimate subject a group of chemical compounds. Genetics requires knowledge of the physical sciences but no other knowledge in the field of biology itself."

"Biology is the study of organisms. Physical and chemical research on organic materials has no biological significance unless it is related to whole organisms and populations of organisms."

THESE four statements above are based on specific sources, but they are paraphrased and the sources are not cited. They reflect widespread attitudes among biologists and that, not their ascription to particular authors, is the important point. When I was asked to write an article on the status of biology, the request, although put in other words, was essentially for a defense of the point of view in the last of the preceding statements. The word "defense" was used, and that is significant. What I am prepared to defend is a somewhat broader thesis: there are many special disciplines and several different approaches in biological research; all are important and their integration is most important; to be effective and to be in fact biological all approaches must take into account the

EDITORIAL NOTE: In the past few years the content of what we call biology has been changing rapidly. This is largely because of the great progress of "molecular" biology. The change is the result of many advances: the knowledge of the structure of some of the large molecules found in organisms, the roles of DNA and RNA in protein synthesis, the kinetics of enzyme activity, the details of cell structures as revealed by electron microscopy and many others.

But the shift has been so rapid that it has been followed by cracks and fissures and all the signs of growing pains. The question of whether some aspects of biology should simply be eliminated from our curricula is not an idle one. To teach the next generation, for instance, we have to decide what is the most important core of information; what constitutes a balanced diet for the training of the biologists of the future. The temptation to take an extreme position is great, but perhaps it is more important first to think the problem through.

With these notions in mind, the Board of Editors of AMERICAN SCIENTIST decided that it would be appropriate for this Anniversary Issue to ask these questions of two distinguished biologists who are themselves concerned with different aspects of biology. One, Professor George Gaylord Simpson, is a paleontologist and an evolutionary biologist; while the other, Professor Clifford Grobstein, is an experimental embryologist. Their work, therefore, is widely divergent, yet it is of interest that their points of view on this basic problem concerning the future of biology are very close, although arrived at by somewhat different arguments. They both show the value of asking oneself, with careful deliberation, what kinds of questions one wants to answer; this will automatically force one to think of the subject of biology in a balanced fashion.

the physical concept which is central to the subject matter, namely, the concept of hydrostatic pressure. But in this case Archimedes may be "excused." Modern mechanics had great difficulties in conceptualizing the notion of pressure, although Stevin immediately mapped out the task of doing so and almost everybody after him was pursuing it. Even Newton was not yet quite certain of it. In a sense the clear-cut mathematization of the concept of pressure was arrived at only in the course of the nineteenth century beginning perhaps with work by A. Cauchy on equations of motion for a continuous medium in general. In the nineteenth century the mathematical "image" of pressure became a tensor, albeit a very special one, and the actual formalization of the concept of tensor and a full realization of its mathematical status took a long time to emerge.

The Schoolmen of the thirteenth and fourteenth centuries were inspired to attempt to conceptualize notions from mechanics like velocity, acceleration and force, by introducing corresponding elements of the infinitesimal calculus and by even trying to formulate coordinate geometry. It has not yet been fully established to what extent these medieval efforts were a truly anticipatory phase of seventeenth century developments. The fact is that in the fifteenth and sixteenth centuries the pace of mechanics slackened whereas mathematics was quite active but in directions of its own. By the end of the sixteenth century mechanics somehow became very vital again. It showed great vigor, acted as if it made a fresh start, and suddenly there existed a Rational Mechanics in the full sense of the word. Mathematics was quite active during the sixteenth century, but after 1600 it quickened its tempo even more and also entered on developments which were different from those of the preceding century.

During the Renaissance period, mathematics was preoccupied mainly with what is commonly called arithmetic and algebra. The initiative for this arithmetic and algebra came from Northern Italy and a most distinguished very early representative of it was Leonardo da Pisa (Fibonacci) around 1200. It is not quite clear how the great interest in these subjects came about and then sustained itself so unflaggingly. Economic deterministic claim that the Rise of Commerce was a great stimulant. Now, whatever the origins of this mathematics may have been, about the effects of it there can be no doubt. It brought about momentous innovations which gradually wrought a change-over from the inertness of traditional syllogistic schemata of Greek mathematics to the mobility of symbols and functions and mathematical relations. These symbols and functions and relations have penetrated into many areas of systematic thinking, much more than sometimes realized, and they have become the syllables of the language of science. What mathematical symbols and functions are nobody can tell, other than in tautologies, that is. The Greeks have found that mathematical thinking has a

organization of *organisms*, and so must both depart from and lead to the level of whole organisms.

If one tries to classify the numerous biological disciplines, it quickly becomes apparent that no linear arrangement or single hierarchy will do. The subjects are too diversely characterized and are too intricately interrelated in too many different ways for categorization on any one principle. A multidimensional key, with categories of different dimensions or scales intersecting, is required. But I, at least, have not found any of the proposed keys satisfactory. All fail in adequate representation of biology as an integral science and of the interconnections of its constituent disciplines. The value of such attempts is that they do make explicit some categorizations significant of different attitudes and approaches in modern biology.

A dimension that inevitably appears is that of level of organization, a scale running from subatomic particles to multispecific communities. The expression "biology of whole organisms" (the subject originally assigned to me) implies a sharp dichotomy of that scale. Another dimension, usually less evident or clear-cut, has to do with the kinds of generalizations or explanations that are sought. That does not involve quite so definite a dichotomy, but it does tend to reinforce the previous dichotomy. There do tend to be differences in approach and aims between infraorganismal and organismal-populational research.

Reduction and Composition

One method of explanation in biology is to relate structure and events at one level of organization with those at lower levels. For example, food chains in a community may, in this sense, be explained successively by the adaptations of specific populations, by the functioning of individual members of each species, by the physiology of their various organ systems, by the enzymes initiating and mediating the underlying chemical and physical processes, by DNA specification of the enzymes, by the structure and properties of DNA molecules, and ultimately by those of the atoms comprising such molecules. That is, of course, the well-known principle of reductionism. In its extreme form, the principle would imply that, for example, the attack of a lioness on a zebra could be not only explained but also predicted from knowledge of the structure and properties of the atoms composing the lioness. Perhaps no one accepts quite so extreme an application, but the first of the statements introducing this article certainly approaches it.

In fact, pure reductionism offers no adequate explanations and permits no sound predictions. The action of an atom, a DNA molecule, an enzyme, etc., can be neither explained nor predicted outside the context of the system in which it occurs. This will not be news to the biophysicists and it is, indeed, involved in my first paraphrase, but biophysicists do

universality *sui generis*, and investigations into foundations of mathematics are not much more than systematic explications of what this implies.

The Renaissance age also initiated the theory of probability. From being a gambler's calculus of finitely many combinatorial possibilities it eventually developed into an analytical theory of laws of large numbers and of infinitely many possibilities in discrete and continuous distributions. During the past 100 years, Probability entered Mechanics in a major way, and it did so twice. In the second half of the nineteenth century it created statistical mechanics for the kinetic theory of matter; and in the twentieth century it was called upon to mediate between two conflicting approaches to quantum mechanics, corpuscular and undulatory. The mediator's judgment was unprecedented and at first quite disturbing.—Also, as if in return, a phenomenon from mechanics which was first observed by a naturalist in the early 19th century, the so-called Brownian motion, has in the twentieth century stimulated the emergence of the probabilistic concept of a general stochastic process. The concept is of undetermined definitory scope, but it has been in the center of probabilistic thinking for several decades past.—Except perhaps for combinatorial speculations, there are no records of quantitative probabilistic pre-concepts in Greek antiquity. There is a provocative section on Chance and Fortune (or rather Chance and Fortuitous) in Aristotle's Physics (Book II, Ch. 4, 5, 6) but regrettably it has no relevance to Mathematics or Physics as we understand them. There seem to be no antecedents to modern Probability in earlier eras and civilizations, and in view of Probability's link to Chance and Fortune this is most remarkable.

The Seventeenth Century

The seventeenth century produced the greatest single statement on the relation between mathematics and physics. It is Galileo's dictum that mathematics is the language of science. It is more than an aphorism. It asserts that although mathematics and physics are inextricably linked they nevertheless have separate identities and are hierarchically not comparable. Attempts to bridge the separateness of their identities and to put them into hierarchical dependence have been undertaken frequently.

Attempts to subordinate mathematics to physics (or even mechanics) usually center in the statement that "geometry is a branch of physics." On the other hand, certain contemporary trends, especially those which trace themselves to Leibniz' plan for a universal mathesis, may be interpreted as giving to mathematical knowledge and to logico-mathematical formulations a supremacy over the realm of science, not only natural science but even social science including perhaps history too. Leibniz'

often underestimate its force or overlook its significance. It means that explanation by reduction simultaneously implies or must be accompanied by the opposite, which might be called composition: that meaningful biological explanation goes up as well as down the scale of levels of organization. It is just as explanatory and just as essential for understanding to say that lion enzymes digest zebra meat *because* that enables the lion to survive and to perpetuate the population of which he is a part, as to say that the digestive event occurs *because* of specific chemical reactions between lion enzymes and zebra fats, proteins, and so on.

One direction of explanation goes down the scale, and because it leads into their domain it is naturally emphasized by the biochemists and other lower-level biologists. The other goes up, and is as naturally emphasized by the organismal and populational biologists. Whatever labels may be adopted for these two approaches or frames of mind, all biological researchers are aware that they exist and that they currently tend to divide many biologists into two often cooperating but sometimes opposed camps.

Question and Answer: Explanation in Biology

Reduction and composition are not the only approaches in biology. As in all sciences, research in biology consists of asking a series of questions and seeking answers. Biological questions can be reduced to a few simple and colloquial forms: What? How? What for? How come?

"What?" does not elicit an explanation. It suggests the further questions and supplies the data for answering them and checking the answers. It calls for observation, whether in nature or in the laboratory, and it is the question asked in the descriptive branches of biology. Although elementary and (because nonexplanatory) unsatisfying in itself, it is the indispensable basis of all scientific endeavor. It has equal pertinence to all levels of organization and all conceptual approaches.

"How?" means how does this thing work? What makes it go, and how does it function? The functional biologist examines at varying levels such things as the mechanical working of the lion's bones and teeth, the digestive action of his enzymes, the production of those enzymes in cells, and so on. Almost inevitably the direction of research is reductionist, and as the direction is followed the answers tend more and more to be in physical and chemical terms. Ultimately the investigator is not dealing with a lion, or any other organism at all, but with a chemical reaction in a test tube or some form of physical model. This is the typical question of biophysics, biochemistry, molecular biology, cell biology, and the other mainly reductionist, lower-level subsciences of biology. An answer, as has been noted, is one form of explanation, or a partial explanation. This kind of explanation has been called physiological, using that term in a somewhat special way.

plan had some antecedents in the fourteenth and fifteenth centuries. Also, some Schoolmen of the thirteenth century, above all Roger Bacon, have become known for their esteem for mathematics, but it would not be easy to attribute to them a reasoned metaphysical judgment on the relation between mathematics and physics.

In Greek antiquity, according to Aristotle, Pythagorean circles maintained that in nature "all is number" meaning apparently that "all is mathematics." Some of them also maintained that space, meaning apparently the space of physical events, is made up of discrete points. Before evaluating the degree of sophistication of this alleged Pythagorean view, one should realize that over 100 years ago the mathematician, Bernhard Riemann, in a famous address on differential geometry, also suggested that space might turn out to be discrete. Plato's doctrine or doctrines also incline in general to give to mathematics a supremacy over much of other knowledge. However, Aristotle also reports, with some emphasis, that Plato assigned to objects of mathematics a hierarchical position intermediate between sensibles and pure Forms (Ideas), which obviously circumscribes the supremacy of mathematics, and, what is more important, gives it an identity of its own. This was a very fine glimpse of the true nature of mathematics, but Aristotle, within his general condemnation of Plato's theory of Ideas, battles this platonic view and advances instead his own that mathematics is an abstraction from nature. Aristotle's view clearly does not fit modern mathematics. If π is, as usual, the area of the circle of radius 1, and e is the basis of natural logarithms then the number e^{π} is hardly an abstraction from anything in nature, although π so is, and e by an expenditure of casuistry, could be made to be so too. Also the complex number $i = \sqrt{-1}$, although a solution of the equation $x^2 + 1 = 0$, is certainly not an abstraction from anything in nature, and yet complex numbers have been used successfully by A. J. Fresnel in the theory of refraction in 1818 and have spread into all parts of physics for many purposes.

In the seventeenth century almost everybody excelled in more than one area. Galileo the physicist had a standing as philosopher. Pascal was scientist and "penseur." Leibniz was philosopher, mathematician, logician. Kepler was astronomer and mathematician. Descartes was evenly divided between philosophy and mathematics. And Newton was equally great in mathematics and in physics. A notable exception was Huygens. He was only a physicist. But he was a physicist's physicist, although he did not even write a treatise.

Some leading works of the seventeenth century give the impression of continuing, by direct succession, works of a distant past, thus spanning gaps of long stretches of time. For instance, Galileo does not take much notice of fourteenth century Schoolmen but he acts as if he were a direct

"What for?" also relates to function but in a different sense or in the opposite direction. It means, what function or purpose does this serve? What need does it fill in the cell or the individual? What functional effect does it have in the population or the community? In this sense the function of the digestive enzymes is to provide materials and energy for cells (and hence for the whole organism); the function of bones and their muscles is to permit behavior adapted to the needs of the organism; and so on. Almost inevitably the direction of research is compositionist, proceeding from lower to higher levels of organization. Like "How?" the question is appropriate at all levels but it has an opposite tendency. The answers tend more and more to be in terms of whole organisms, populations, and communities. Ultimately the investigator has no direct concern with the physical sciences but is dealing with phenomena that are exclusively biological. This is a typical question of the biology of whole organisms, or of the compositionist approach at any level. An answer is another form of explanation or another partial explanation. Since there is reference to purpose (fulfilling of needs, functioning in a larger context) which nevertheless is not teleological in the philosophical sense, it has been proposed to call this kind of explanation teleonomic.

"How come?" means how did this originate? What course has it followed through time and what were the causes of that historical development? The question is equally appropriate at all levels, and answers need not inherently be either reductionist or compositionist. We could (if we had enough information) follow the history of DNA without necessary reference to nonmolecular levels, just as we can follow the evolution of species (populations) without necessary reference to nonpopulation levels. Nevertheless, more complete answers tend to cut across all levels and to be simultaneously reductionist, compositionist, and more. The evolution of DNA becomes most fully meaningful and explanatory when it is related to the processes in populations that cause specific information to be coded in the DNA, and, conversely, the evolution of species is best understood when related to the changes in the DNA codes. It is nevertheless true at present that the question "How come?" is more often asked by organismal biologists and that they have produced most of the available answers, even including those involving levels below the individual.

Historical and teleonomic explanations have close conceptual relationships. The most important single element in historical explanation, natural selection, is teleonomic in principle. Confronted by any intricately organized system, anyone inevitably asks both "What for?" and "How come?" and feels that these are mutually dependent problems. For entirely physical systems in nature, the questions "What for?" or "How come?" as distinct from simply "What?" or "How?" often seem purely philosophical or unanswerable and therefore sterile as approaches to

successor to Aristotle and he "corrects" Aristotle on specific statements made in the *Physics*. Descartes in his geometry acts as if he were a direct successor to Apollonius (210 B.C.) and Pappus (320 A.D.) and in a certain sense he even taunts these "predecessors" of his for not having achieved themselves what he is about to do now. And it is indeed inexplicable why Greek mathematics which lasted over 1000 years, namely, from 600 B.C. to 400 A.D. and beyond, and which had a geometric tenor did not arrive at such simple things as placing figures into preintroduced coordinate systems and plotting curves on graph paper. Fermat who was mainly a number theorist considered himself to be direct successor and disciple to the ancient number theorist Diophantus (250 A.D.). He revered him, and as a gesture of reverence he even quoted Diophantus in a non-arithmetical context in a paper on maxima and minima (Fermat, *Oeuvres*, Vol. I, p. 133). But the most notable instances of such "leaping backward" are in the achievements of Kepler and Newton.

Kepler was an assistant to Tycho Brahe, a disciple to Copernicus, and a spiritual descendant of Nicolas of Cusa (fifteenth century) who is commonly considered to have been the harbinger of an important phase of modernism. These are straight facts and could not be gainsaid. But if, from a distance, and in retrospect, one elects to look at the bare mathematics and physics only, and only at the sharp lines in the contours of development, then one may also find that Kepler's discipleship goes back directly to Apollonius and also Ptolemy (150 A.D.) and that Kepler was a successor directly to them. To generations of astronomers, from Eudoxus (370 B.C.) to Copernicus and Tycho Brahe, the basic celestial figure was a circle and they took it apparently for granted that a celestial trajectory is a figure composed of circles kinematically. Kepler broke with this supposition. He introduced ellipses instead, and, guided by astronomy, he grasped the true meaning of foci of conics which others before him had no occasion to recognize. Conics, and their theory, were in no way Kepler's private mathematical invention and discovery. They had been in the public domain for nearly 2000 years for anybody to find and use. Also, in point of literal fact, Kepler did not find his ellipses in the tables of Tycho or the "revolutionary" writings of Copernicus or the "learnedly ignorant" utterances of Cusanus. He found them by searching, untiringly, in the work of Apollonius all his life, and by being, apparently, also thoroughly familiar with the second half of Apollonius' treatise which by then had not yet been translated from the Arabic into Latin, as far as is known. Apollonius knew astronomy and Ptolemy undoubtedly knew conics, but they did not use conics as celestial orbits. Kepler did so, and the mathematics which he pre-required should have been readily available to Apollonius and Ptolemy. Therefore, from a certain pragmatic approach, Kepler was a direct successor to Apollonius and Ptolemy.

scientific research. In biological systems the pervasive fact of evolution makes scientific answers accessible. For these reasons the fields of both teleonomic and historical explanations are often and significantly united under the rubric of evolutionary biology.

The point about explanation in biology that I should particularly like to stress is this: to understand organisms one must explain their organization. It is elementary that one must know what is organized and how it is organized, but that does not explain the fact or the nature of the organization itself. Such explanation requires knowledge of how an organism came to be organized and what functions the organization serves. Ultimate explanation in biology is therefore necessarily evolutionary.

Aim and Progress

The aim of biology is to understand the structure, functioning (both as "How" and "What for?"), and history of organisms and of populations of organisms. Biology is the study of life; life occurs only in living things; "living thing" is synonymous with "organism"; organisms that are not whole are not alive. (The expressions "organismal biology" or "biology of whole organisms" are thus really tautologic.) Moreover, organisms live only in the context of populations. Clearly, and even as regards any one phenomenon, biology requires study at all levels of organization and explanations of all three kinds. Nevertheless, the name and nature of the science itself center its aim at the level of organisms in populations. To be truly biological, all approaches, reductionist, compositionist, and historical, must not only include but also depart from or lead to organisms, structured and functioning as such in themselves and in reproducing aggregates.

The whole of this extremely complex goal cannot be achieved or—for the individual biologist, at least—even aimed at all at once. The rate of progress is uneven, and rapid advances take place now in one direction and now in quite another. Once a shove has been given in one direction, perhaps by a technological or conceptual breakthrough, perhaps by individual enthusiasm, perhaps by what seems pure chance, a band wagon effect ensues. Students flock to the accelerating front; money is poured into it; professional advancement, fame, and fortune follow it. That is only natural and is in one respect desirable, for the band wagon effect feeds into a circle by which the rate of discovery in some one field is indeed increased.

Fortunately, more than one aspect of biology may accelerate at the same time, and with the great increase in numbers of biologists not all crowd onto the same band wagon. The gaudiest band wagon just now is manned by reductionists, is traveling on biochemical and biophysical roads, and carries a banner with a strange device: DNA. There are, however, a good number of other, perhaps less gaudy band wagons, going

The Isaac Newton of the *Principia* is by the principal subject matter of the treatise a direct successor to Kepler and by the manner and style of its conceptualizations a direct successor to Archimedes. What makes the *Principia* the book that it is are not its Definitions and its Laws and its views on time and space and not even its central forces if taken by themselves and on their own merit. The *Principia* are what they are because with this congeries of definitions, laws, concepts of time and space, and his gravitational force, and by mathematically controlled reasoning, Newton constructed and deduced what Kepler had only divined and posited, and because into this design of Kepler's celestial orbits, and from the identical blueprints, Newton also fitted the (then) earthbound trajectories of Galileo and Toricelli. The twentieth century has apparently verified that even artificial celestial bodies conform to the Newtonian theory of the seventeenth century, and, what is most remarkable, nobody in the twentieth century doubted that they so would (except of course for "relativistic" adjustments if such should be called for, eventually).

Now, outwardly the *Principia* are written in a pure idiom of Archimedes and Apollonius, and outwardly there is hardly any mathematics invoked or presupposed which should not have been quickly accessible to them. Newton was well familiar with Descartes' work, and in his book on plane curves Newton masters the method of Descartes in analytical detail much better than Descartes himself. Also Newton was quite skilled in the use of symbols and functions from the beginnings of his mathematical career, as his early work on power series and also on fluxions shows. But in the *Principia* no attempt is made to modify the Greek idiom by the use of Descartes' innovations or to employ symbols and functions analytically; even the inviting trait of discursiveness that animates the *Opticks* is hardly present in the *Principia*. Furthermore, and this we would like to stress, Newton's definitions of limit and derivative ("ultimate ratio") as presented in the *Principia* seem to us to be such, in syntax and in content, that a personal disciple to Archimedes should have been able to compose them. At any rate, it would be a major task to substantiate philosophically that a student of Archimedes could not have possibly arrived at them because of their being removed from and alien to the hellenic-hellenistic milieu of natural philosophy within which the developments from Eudoxus to Archimedes and Apollonius had taken place.

And yet, inwardly, the Euclidean Space that underlies the *Principia* is mathematically not quite the same as the Euclidean Space that underlies Greek mathematics (and physics) from Thales to Apollonius. The geometry of the Greeks emphasized congruences and similarities between figures, that is, in analytical parlance, orthogonal and homo-

down other, perhaps less rapid roads, under banners perhaps less vehemently saluted. At any rate, we do salute them all, and yet may fear that these band wagons diminish travel on still other roads, which are falling into neglect but which are also essential to reach the destination toward which all are, or should be traveling.

The danger for biology is the assumption that some of its subjects, some directions of approach, some kinds of explanation are no longer important: their contribution, such as it was, is thought to have been achieved or is not now considered necessary. But the assumption that any of the biological disciplines has fully accomplished its aim or that any is unnecessary to achieve the goal of biology is completely wrong. The error is compounded when it directs attention away from the real center of that goal: the understanding of organisms. All four of the paraphrases introducing this discussion err to the extent that they insist on one approach at the expense of others, but the fourth errs least in that respect, and it has the merit of concentrating attention on the *biologically* most significant level.

Classification and Systematics

It is obviously impossible in this brief essay to review the particular status of the many separable high-level biological disciplines, even in the most general way. A few examples, necessarily treated without detail, must suffice to suggest the general position of such subjects in current biological research. Classification will serve as one example that is extreme in several respects: apart from strict description, it is the oldest of all biological subjects; it is clearly organismal, although its compositionist approach draws data and explanations from lower organizational levels; and it is now the subject most often considered relatively unimportant by reductionist biologists.

Each biological discipline or subscience has a goal of its own that is part of the total goal of biology. The goal of classification and of taxonomy (the theoretical and operational basis of classification) is to produce a stable, biologically meaningful arrangement of all knowable organisms. To state the goal is to demonstrate that it is not yet anywhere near achievement. Classification is as yet far from stable. Current classifications in some groups have achieved considerable biological significance but could surely be improved. In many other groups, current classifications still have unsound or insignificant foundations. Only a small fraction of knowable organisms is as yet known and classified.

It might seem offhand that classification is essentially a descriptive science, its one question "What?" (What organism is that? An animal; a bird; a passerine; a tyrannid; a phoebe, *Sayornis;* Say's phoebe, *Sayornis saya.*) Even some taxonomists, bemused by confusion of identification and classification, of observation and interpretation, have been

thetic transformations of the underlying space. The Greeks themselves were half-aware of this. Late Greek traditions about the origin of their mathematics, whether true or legendary, ascribe to Thales mathematical exploits which involve congruences and similarities emphatically. The Euclidean Space of the *Principia* continued to be all this, but it was also something new in addition. Several significant physical entities of the *Principia*, namely, velocities, momenta, and forces are, by mathematical structure, vectors, that is, elements of vector fields, and vectorial composition and decomposition of these entities constitute an innermost scheme of the entire theory. This means that the mathematical space of the *Principia*, in addition to being the Greek Euclidean substratum, also carries a so-called affine structure, in the sense that with each point of the space there is associated a three-dimensional vector space over real coefficients, and that parallelism and equality between vectors which emanate from different points are also envisaged. In the course of the eighteenth century the vectorial statements of Newton and others were gradually transferred and re-interpreted into analytical statements. This involved Euclidean coordinates and eventually also "generalized" coordinates of Lagrange. In these analytical developments the vectorial character of the space of Newton ceased to be featured. But in the nineteenth century, partly within the concepts of Hamilton's theory of quaternions, the underlying vectorial trait was gradually recalled, and even taken account of by a symbolism of its own which has remained in use. The twentieth century widened the concept of a vector into the broader concept of a tensor. This called forth readjustments in mechanics and physics and affected pure mathematics even more profoundly.

Newton asserts the vectorial character of mechanical force in "Corollary II to the second law" in Book I of the *Principia*, and his assertion consists of two parts. The first part states that any two forces give rise to a resultant force which is the vectorial composition of the two. The second part is in the nature of a converse to the first part and it states that in a plane a force can be decomposed into two component forces along any two non-parallel lines. The second part of the assertion is much more recondite than the first, even if applied to the simpler case of velocities rather than forces. It is a simple observational fact that if two persons pull at a load in different directions then the load moves along some suitable diagonal, and this fact was undoubtedly known to Aristotle as commonly asserted. But it is an entirely different assertion to say that if in a plane a force is given "concretely" in a well-determined line of action, then purely speculatively it can be imagined to be a vectorial sum of two "fictitious" component forces along "fictitious" lines of action of arbitary choice; and there is nothing to suggest that such a possibility was in the minds of natural philosophers before the seventeenth century, say.

that naive. In fact, however, any attempt to arrange the data in a rational or natural way, to classify the observations, brings in some element of explanation. In eighteenth and early nineteenth century classifications the explanatory element tended indeed to be minimal or more metaphysical than scientific. Then, in 1859, Darwin made the first great breakthrough by defining or rather demonstrating the application and meaning of "How come?" in this field. Taxonomic orderliness, on which the possibility of meaningful classification depends, is caused by evolution. Darwin started a taxonomic band wagon on its way, enthusiastically if not too well manned. By the early twentieth century the impetus was being lost and attention was being diverted to other band wagons, especially one under the banner of Mendel.

In the last generation, through the work of numerous biologists still active, there has been another breakthrough, less abrupt than Darwin's and less sharply definable but with equally important potentialities. Darwin's principle is no less involved, but it is related more especially and explicitly to the fact that it is varying populations that evolve and that are classified. Apparently simple in itself, that principle changes approaches to classification and the actual forms of classification. More importantly, it brings classification into more significant contact with other biological disciplines, and it both intensifies and expands the explanatory content of the subject. "How come?" is still asked, and more complete answers are being found. "How?" becomes equally pertinent. (How do species arise? How are they maintained as separate entities?) "What for?" is hardly less so. (What are the functions of differences among species and other taxa? What is the function of a particular species in its community?) Thus, not only historical but also reductionist and compositionist approaches enter classification, and while continuing to operate at its level of populations it extends upward and downward through all levels. It becomes the core discipline of the science of organic diversity and interrelationships: systematics.

A geneticist friendly to the subject has remarked that taxonomy is currently near the bottom of the biological pecking order. That is perhaps temporarily true in terms of relative acclaim and emphasis. Nevertheless, taxonomy does have a band wagon advancing energetically toward its goal and contributing very significantly to the advance of biology as a whole. The proportionate share of taxonomy in total biological activity just now is below the optimum, but in absolute terms taxonomy is at an all-time peak; in numbers of working taxonomists; in rate of discovery; in accomplishment; in publication; in available support.

Biogeography

As a second example I select biogeography because it is still more obviously at a high organizational level and because it is a complex using

The Eighteenth Century

The eighteenth century was an age of organization, for mechanics and even for mathematics. It produced several so-called Principles of mechanics and some special theories and methods. Books from the eighteenth century are frequently still readable and many of their notations have maintained themselves into current or relatively recent use. The organizing activity was not all spent on a continuation and elaboration of what the seventeenth century had achieved, but a large part of it also on a recreation and re-establishment of the subject matter from its roots.

The eighteenth century introduced, more or less formally, the concept of a mass particle in the sense of a geometric mass point, and of a finite system of mass particles with general forces and constraints acting between them, and it recognized that the theory of such finite systems is a guide and pilot theory for rational mechanics as a whole. But it also studied extensively the so-called "mechanics of continua" which comprises large and detailed theories of its own, chief among which are hydrodynamics, acoustics, and the general theory of elasticity. Acoustics on a very elementary level is as old as the Pythagoreans, a hydrostatistical theorem is one of Archimedes' great claims to fame, and rudimentary beginnings of elasticity have been ascribed to Stoics.

The organization of mechanics necessitated the creation of a considerable amount of mathematics, mainly in Analysis. Of this mathematics, the calculus of variation was instigated largely by mechanics of particles, but other theories were instigated largely by mechanics of continua. Virtually all of partial differential equations started in this way, especially the mathematical theory of waves which eventually became the hallmark of theoretical physics in all its parts. Mechanics of continua also created half of Fourier analysis. The other half was created by the theory of heat which also contributed the name of the theory. Mechanics of continua also had a fair share in the promotion of potential theory. But it should be said that the concept of potential energy originated in the Lagrangean theory of finite systems of particles, and that, on the other hand, through the work of Poisson and others, the concept of potential acquired the status of a commanding physical entity not within mechanics but within electrostatics.

Finally, it appears that mechanics of continua had a share in the emergence of tensor theory which after slow stages of nascency began to take shape towards the end of the nineteenth century, and in the twentieth century has been penetrating various areas of physics and mathematics. Important entities in the mechanics of continua, at whose mathematical conceptualization the emergent theory of tensors was aiming, were what are nowadays the pressure and stress tensor and the accompanying deformation tensor. Pressures, tensions, stresses and such like entities are species of "force" but phenomenologically they seem

the approaches and explanatory methods of several other biological disciplines. Thence it tends to ramify through many fields of biology, and it is among the most highly syndetic of biological subjects. In spite of that fact, it is one of the most poorly supported and most slowly progressing.

In a primitive, pre-scientific way, biogeography, like classification dates from prehistoric times, but it belatedly reached fully scientific status only around the middle of the nineteenth century, largely under the influence of Darwin and Wallace. Although Darwin, particularly, aimed at explanatory biogeography and adumbrated almost all later approaches, work in this field was at first largely descriptive. One aim was the production of increasingly generalized descriptions of animal and plant distribution in the form of hierarchies of delimited and denominated biogeographic regions (zoochoria and phytochoria). The higher categories were quickly mapped out, and although there have been many valuable studies of detail in the present century there has been little fundamental advance at that descriptive level.

Explanatory biogeography, most active in the present century and at the present time, has two inseparable but distinguishable aspects. It treats, on one hand, with the requirements (tolerances and preferences) of species of organisms, the geography of environments meeting those requirements, and the consequent distribution of species. The approach is ecological; the questions are mostly "How?" and "What for?"; and explanations are largely reductionist, by individual adaptations within populations, by functioning and functional requirements of organs within individuals, and so on down ultimately to the molecular level. On the other hand, explanatory biogeography also treats of the origins of particular groups of organisms in specified regions and their changes in geographic distribution through time. The approach is historical; the primary question is "How come?"; and explanations are evolutionary.

Geographic considerations enter into many other biological subjects. They are, for instance, crucial in the "How?" of the origin of species (and other taxa), hence also in the "How come?" of systematics. Conversely, although the ecological biogeographer more often asks "How?" he cannot overlook the ulterior "How come?" for the adaptations that explain how.

The status of biogeography is peculiar in that it is rarely pursued as a discipline or profession in itself. Its data, principles, and methods come mostly from systematics, ecology, and paleontology, and its practitioners are primarily systematists, ecologists, and paleontologists, not zoogeographers *per se*. Ecological biogeography has been most intensively developed by botanists, historical biogeography by vertebrate paleontologists. As far as I know there have been no professorships or journals of biogeography in this country, and only one or two anywhere. Development

not to subordinate themselves to the concept of force as introduced in Book I of the *Principia* the mathematical substratum of which is a vector of three components issuing at a point. In fact they seem to be entities which associate themselves with the various surface elements through a point in space rather than with a point of space by itself. In the outgoing eighteenth and beginning nineteenth centuries efforts were made to overcome these disparaties by reducing all surface forces to purely vectorial forces through appropriate molecular approximations; but there were also parallel efforts to conceptualize things as they present themselves phenomenologically. In this conceptualization there are two types of forces, point forces and surface forces and they are linked by certain basic laws which are modeled on Newton-Lagrange prototypes. A surface force is a space tensor of two indices. Such a tensor in general has 9 components. In surface forces of the customary kind a symmetry property reduces the number of components "effectively" to 6. Special symmetry requirements may reduce it further. The prime instance of this arises in the case of an incompressible fluid. In this case the pressure tensor reduces effectively to only one component, and this is the so-called pressure. Although it is a one-component quantity, it is definitely not a scalar. In fact, as a mathematical object it is far removed from anything which Archimedes might have been able to conceptualize in his work on floating bodies against the background of the mathematics then alive.

A particular situation arises in the case of rigid bodies. They are continuous media, but tensorial forces need not be envisaged although a tensorial setting does illumine the context. The theory of rigid bodies is a doctrine by itself. Large parts of it are geometrical, but it is a "modern" geometry of analytical variability, and not of Greek stationarity. The theory was fully started in the eighteenth century by L. Euler and is a showpiece among the many masterpieces of this great master. In retrospect, his kinematics is a first meaningful study of groups, namely, of one-dimensional continuous groups of orthogonal transformations in Euclidean space, well over 100 years before Sophus Lie, Erlanger Programs and such like. His dynamics was a transfer, with suitable elaborations, of appropriate statements for finite systems of points to the continuous case, without a trace of brooding over the melancholy fact that there are no truly rigid bodies in nature.

All told, the eighteenth century achieved very much for the theory of continuous media and mathematics involved in it. And yet, the historical "image" of eighteenth century mechanics is composed of achievements which relate to finite systems of particles primarily.

Of great originality are two so-called "differential" principles, the principle of Bernoulli and the principle of d'Alembert. They can be stated and appreciated separately. But what has given them their status

of the subject is dependent on the personal interests of specialists in related but distinct disciplines, and support for it is largely incidental or marginal. Under one aegis or another, it is making progress, but that progress is slower and more erratic than would be desirable. No band wagon flies this banner.

Genetics

As a final, brief example I select a subject that is just as much infraorganismal as organismal and populational, that currently has some tendency to split along those lines, and that by the irrationality of that fission demonstrates the artificiality of the dichotomous tendency in biology and the impracticality of emphasizing one approach at the expense of others.

Modern genetics started at the level of individual organisms, individuals taken collectively and studied statistically (that was of course Mendel's great contribution to methodology in the study of heredity) but not considered as biological populations. Although many geneticists now use the term to include all study of biparental heredity, Mendelian genetics, strictly speaking and in the original sense, is concerned with the inheritance of particular characters in individuals. Explanation is physiological, and approach is largely reductionist, e.g., from organisms to chromosomes to genes, the latter (as originally defined) deduced but not observed. This field is still extremely active, although no longer so clearly delimited in subject matter or in approach.

Population genetics focused attention on the next higher organizational level, and it complemented the original Mendelian approach by balancing reduction with composition, physiological with teleonomic explanation. The teleonomic aspect is involved in the explanation of genetic information in organisms by adaptive processes, notably those of differential reproduction, or natural selection in the modern sense, at the population level. Thus genetics was able to reach more complete explanation by asking not only "How?" but also "What for?". Systematics was as instrumental as genetics in reaching this more fully explanatory combined approach, and the present synthetic theory of evolution was born from the union of the two disciplines. In conjunction with paleontology, "How come?" is also asked and, in part, answered. (The theory turns out to be triparental, at least!)

Population genetics was unusual, if not unique, in biology in that it was largely developed by deduction from mathematical models. Its recent tendency has been to outgrow the models, or to become too complex for that method alone, and to proceed more extensively by induction from observation in field and laboratory. The development of the synthetic theory was a major breakthrough, and population genetics in that framework and in company with systematics, paleontology, and

is the fact that jointly they are the foundation for the general Lagrange equations of motions for finite systems of particles. In any rationalization of these equations the two principles are involved deeply and inseparably.

The principle of d'Alembert mathematically "transforms" a dynamic system of particles into a static one, and it views the so-called Newtonian equations of motion as if they were conditions for equilibrium. If a single point moves on a straight line under the influence of an "external" or "impressed" force F, then the principle of d'Alembert puts the equation of motion in the form

$$-m\frac{d^2x}{dt^2} + F = 0$$

and, as many in the nineteenth century understood the principle, it "interprets" the term

$$-m\frac{d^2x}{dt^2}$$

as representing also a force, the so-called inertial force. Thus it interprets the equation of motion to mean that the "total" force, that is the sum of external force and of inertial force, is zero. In a general system of points it asserts that suitable vectorial sums of external forces and of inertial forces are each zero. Since the notion of an external force is usually an "abstraction," for instance if it is a gravitational force, the inertial force of d'Alembert is in a sense an abstraction from an abstraction, or rather an abstraction vertically imposed on a previous one. Such "multifold" abstractions, that is, abstractions from abstractions, abstractions from abstractions from abstractions, etc., if performed in cognitive depth, generate much of the motive power of modern mathematics and also modern science; and we think that not many of such will be found among all the "ideations" from Plato to Plotinus to Abélard to Ockham, especially not from Plato to Plotinus.

The principle of Bernoulli is more technical, and in a sense even more daring. For our purposes we will present it in two stages. The first stage will be seemingly a mathematical tautology in preparation of the second stage. But we should point out that we will follow the presentation of the principle by Lagrange rather than Bernoulli's own.

If in a finite system of mass points we denote the totality of all components of all mass points in a uniform lettering by

$$x_1, x_2, \ldots, x_N \tag{1}$$

and the corresponding components of the external forces by

$$F_1, F_2, \ldots, F_N$$

then in the case of equilibrium all these components of force are zero. That is, we have

$$F_1 = 0, F_2 = 0, \ldots, F_N = 0. \tag{2}$$

other related disciplines continues to be extremely active and exuberantly productive.

Most recently, the original tendency of Mendelian genetics has carried another line of genetical research more and more into the realm of reductionist biology. There, a breakthrough in molecular biology had occurred, and there has been a meeting ground and a deepening of the reductionist part of genetical explanation. This centers on DNA and emphasizes genetic processes within cells and at the molecular level, including nonchromosomal heredity. The extent of enthusiastic emphasis in some quarters is suggested by my third introductory paraphrase, which implies, at least, that genetics as a whole can be reduced to chemistry and that organisms can be explained by chemical genetics, without in turn helping to explain the latter and without explanations also from and to other levels.

I do not at this point need to restate all the reasons why I disagree with that point of view and consider it a hindrance in current advance toward the proper goal of biology. Explanation in genetics and in the whole field of the biological sciences must answer all the questions, use all approaches, and recognize that all biological disciplines are inextricably interdependent. Organismal and population genetics can no more reach their goal without consideration of DNA than molecular and cell genetics can reach theirs without taking whole organisms and populations into account.

Conclusion

Systematics, biogeography, and population genetics are far from being the only biological disciplines involving organismal and higher levels of organization, largely compositionist in approach, and characterized by teleonomic and historical explanations. Among other subjects primarily at these levels are paleontology, ecology, and behavior. Still more subjects have essential aspects that are organismal in the same sense, notably comparative anatomy, physiology, and embryology. All those disciplines are no less, and I believe no more, important than the three I have used as examples. It would be contrary to my whole thesis to suggest that these organismal or evolutionary aspects of biology should be stressed at the expense of any others or pursued without reference to others. It is, however, my conviction that this general group of disciplines is focal for biology and that it now offers the greatest opportunities for integration and progress in fully *biological* science.

Acknowledgments: Much that is here expressed derives from discussions with Th. Dobzhansky, Ernst Mayr, and Colin S. Pittendrigh and from their publications. All three also read the first draft of this essay and offered helpful suggestions which have led to extensive rewriting and, I believe, improvement.

Purely mathematically this set of N conditions can be replaced by the one condition

$$F_1 \cdot \lambda_1 + F_2 \cdot \lambda_2 + \ldots + F_N \cdot \lambda_N = 0 \tag{3}$$

which is assumed to hold for all possible choices of real numbers

$$\lambda_1, \lambda_2, \ldots, \lambda_N.$$

Now the principle interprets each number λ_n as a "virtual" displacement of the corresponding space component x_n. Such a displacement is usually denoted by δx_n, and thus condition (3) is now written as

$$F_1 \cdot \delta x_1 + F_2 \cdot \delta x_2 + \ldots + F_N \cdot \delta x_N = 0. \tag{4}$$

The first interpretative gain of this formulation was as follows. The expression on the left side, that is the sum

$$F_1 \cdot \delta x_1 + F_2 \cdot \delta x_2 + \ldots + F_N \cdot \delta_N, \tag{5}$$

was conceived as a certain "action" of the given forces along the given virtual displacement—which the nineteenth century eventually called "work"— and equation (4) is now interpreted to state that the system of forces is in equilibrium if and only if this action is 0 along any virtual displacement whatsoever.

This is the first stage of the Bernoulli principle. For the second stage we reconsider matters from the following approach. Thus far we have tacitly assumed that our system is a "free" system of particles, that is that the presence of external forces is all that influences the motion of the particles. But now we take a system of particles in which so-called "constraints" are present, where constraints are certain primary conditions which prescribe that only certain geometrical configurations of the mass points can occur. Also the constraints are assumed to be independent of the external forces and to be superimposed on these. Now, the second stage of the Bernoulli principle states that for a system with constraints the necessary and sufficient conditions for equilibrium continue to be the same as in a free system. Thus, it is again equation (4), and $F_1, \ldots, F_N$ are again the components of external forces, and $\delta x_1, \ldots, \delta x_N$ are again virtual displacements. But, and this is decisive, *only such virtual displacements are to be considered as are compatible with the constraints.*

It took a long time before it was generally understood that the principle of Bernoulli if applied to systems with constraints is a far-reaching hypothesis of axiomatic nature. L. Poinsot in an angry note in 1838 (*Journal de Mathématiques pures et appliquées*, Vol. 3, pp. 244–248) castigated Laplace for misrepresenting this point in his *Mécanique Céleste*. And C. G. Jacobi when stating matters very clearly in his *Vorlesungen über Dynamik* (1842–43, published 1865) still deemed it appropriate to invoke the great authority of Gauss in support of his statement.

LEVELS AND ONTOGENY

By CLIFFORD GROBSTEIN

I HAVE THREE objectives in writing this essay—to reiterate the importance of multi-leveled analysis in biology, to illustrate the particularly crucial connection between the concept of levels and the process of development, and to join those who advocate the thesis that recent spectacular biological progress through application of molecular approaches will be self-defeating if it leads to atrophy of interest in, and understanding of, supra-molecular problems and approaches.

I am certainly not pioneering in stressing the importance of multi-leveled biological analysis. The long history of controversy between extreme reductionists on the one hand, and extreme organismalists on the other, has educated most biologists toward a pragmatic middle ground, with healthy respect for what has been called hierarchical organization. This recognizes that biological phenomena exist in interlocked superordinacy and subordinacy, and that a phenomenon of given order is fully comprehended only when it is examined by techniques not only appropriate to its own level but to those above and below it. Sophisticated biologists know that "horizontal" isolation of biological phenomena is essential—and yet dangerous. While it simplifies and clarifies the properties of components obscured in the complexities of the higher order, it frequently drastically alters the behavior of these components and thus requires extreme caution in extrapolating back to the undisturbed system. On the one hand, modifications induced in components by isolation—properly interpreted—can illuminate the nature of the regulatory mechanisms of the whole system; on the other hand some knowledge and control of these mechanisms frequently is necessary to preserve in isolated components their most interesting and crucial behavior for comprehending the properties of the whole. Sophisticated biological investigation thus involves a continuing cross-feed of information between analyses proceeding at several levels.

I should like to illustrate these fairly elementary considerations by referring to the problem of cytodifferentiation (Grobstein, 1959), particularly as it has been approached *in vitro*. Even preliminary definition of cytodifferentiation requires clarification of levels, for our concept of differentiation has a very different emphasis when applied to organisms, to constituent cells, and to intracellular components. Referring to whole organisms, emphasis is on increasing heterogeneity—the origin, localization and amplification of differences within an originally relatively homogeneous system. The early embryo acquires heightened diversity and complexity through such differentials as polarization, regionalization, organogenesis. In the process there are drastic changes of properties at all subordinate levels—cell groups, individual cells, cell organelles,

Lagrange obtained his equations of motion in the following manner. He superimposed the principle of d'Alembert on the principle of Bernoulli, that is he applied the second stage of the principle of Bernoulli to forces which are sums of external and inertial forces. Then, by a purely mathematical argument, which is one of his leading mathematical achievements, he transformed the constraining *conditions* also into constraining *forces*. As a result, his equations of motion for a finite system with constraints have the same analytical form as for a system without constraints except that in addition to the external forces there are some further "ideal" forces which account for the constraints. These ideal forces are perpendicular to those virtual displacements which are compatible with the constraints so that they make no contribution to the expression (5). Also, the actual sizes of the constraining forces are measured by the values of so-called "Lagrange multipliers."

This was only half of what Lagrange did in this area, and the second half is the one that really matters. Lagrange subjects the original space coordinates (1) to a general analytic transformation of variables. This gives rise to new variables, again of the same total number N, and they are frequently denoted by the letters

$$q_1, q_2, \ldots, q_N. \tag{6}$$

These are his so-called "general coordinates," and Lagrange translates the previous equations of motion into new equations for the new coordinates. Now, if there are constraints in the system, and if they are expressible by s "independent" equations between the original N coordinates (1) then it is possible to choose the general coordinates (6) in such a way that the constraining conditions become the requirement that s among the new coordinates shall be zero, say

$$q_{N-s+1} = 0, \; q_{N-s+2} = 0, \; \ldots, \; q_N = 0. \tag{7}$$

The remaining N—s coordinates

$$q_1, q_2, \ldots, q_{N-s} \tag{8}$$

are then unconstrained and these are Lagrangean "free parameters" of the system. If general parameters have been so chosen, then the system of equations of motion decomposes into two parts, a primary part and a secondary part. The primary part involves only the free parameters (8), does *not* make express reference to constraining forces, and completely determines the solutions of the problem. The secondary part, on the other hand, only determines the constraining forces and nothing else. Thus the secondary part may be ignored altogether, and textbooks usually omit it.

Constraints and constraining forces, and attempts to understand their nature and effects, are as old as mechanics itself and a full history of the genesis of these concepts would be virtually coextensive with a

states of molecular aggregation, molecular species. When the focus drops to the cell—cytodifferentiation—emphasis is on change of cell properties, appearance of "new" cell types. "Holodifferentiation" connotes diversification, "cytodifferentiation" connotes change and stabilization of cell properties—frequently with decreasing rather than increasing diversity *within* the cell. Subcellular differentiation, on the other hand, emphasizes specialization, the concentration of diverse activities toward particular ones—especially those which lead to new synthetic products, sometimes "exported" as secretion, sometimes accumulated as "structure" between or within the producing cells. The general term "differentiation" denotes any of these things at all of these levels, but it has special and different connotation with respect to each.

Against this background it may be asserted that understanding of cytodifferentiation has been clouded by failure properly to relate studies of supercellular and subcellular phenomena to the primary level of reference—the cell. It was recognized a number of years ago that culture of cells *in vitro* provides an approach to understanding their differentiation through simplification and control of cellular environment. Early observations were exciting in showing that when a bit of relatively advanced embryonic tissue was isolated in a nutrient jelly, the number of specialized cells soon decreased relative to the number of apparently generalized and undifferentiated ones. This suggested the possibility that cultured tissues revert to a more embryonic stage, that tissue culture might provide a method for recovering, propagating and preserving cells in an undifferentiated state, ready to be converted to any and all adult tissues at will. Subsequent experiments showed that the matter is not nearly this simple. Some differentiated cells in the embryonic explants were found quickly to die in the cultures, others lost their overt specialized characteristics but appeared to persist. Yet no one was able to show convincingly, in these early studies, that propagating cultured tissue could be made to differentiate in a direction alternative to the original tissue type. In those few instances in which redifferentiation was induced in cultured tissues, cells appeared which seemed to be of the original type. The proponents of dedifferentiation in cultured tissues by simple reversal were driven from the field. However, it was asserted by their opponents—and came widely to be accepted—that the fundamental differentiative state of cells in culture remains unaltered, though concealed, through generations of propagation. This is a conclusion with extremely important implications. It implies that whatever controls and stabilizes cytodifferentiation is propagable in some form, and hence is at least quasi-genetic. It points attention at intra-cellular genetic mechanisms for the explanation of cytodifferentiation and its integration into general biological theory.

Re-evaluation of these considerations has been enforced by accumulat-

history of the genesis of mechanics itself, at least of the mechanics of weights and ordinary bodies. One would certainly have to take into account the work of Jordanus (thirteenth century), and perhaps even go back to Heron of Alexandria. Stevin and Galileo operate with constraints and constraining forces in the open, and Newton does so even more expressly. And yet their analytical formulations in the eighteenth century, apparently more-or-less due to Lagrange, constituted a major novelty nonetheless.

The emergence of general coordinates and free parameters in the eighteenth century was intellectually a prelude to the rise of multi-dimensional geometries, Euclidean and non-Euclidean, in the nineteenth century. Some of the creators of these geometries were not only experts in rational mechanics but even leaders in it. We think that, by evolution of ideas, the geometries of the nineteenth centuries were, in good part, descendant from eighteenth century mechanics, and not only in the general sense that any period of mathematics is descendant from the preceding one. The total development was an evolutionary process. In the seventeenth and eighteenth centuries the structure of space and place was Euclidean as a matter of course. In the nineteenth century the possibilities of non-Euclidean structure were being explored for geometry and for geometry alone. By the end of the century, when the mathematics was ready for it, first steps were taken towards inserting non-Euclidean features into mechanics also. And in the course of the twentieth century some non-Euclidean (that is "relativistic") features of mechanics have already become basic and commonplace.

Some philosophers and mathematicians unduly single out the emergence of non-Euclidean geometry as the decisive mathematical event of the nineteenth century. Non-Euclidean geometry is all important for any confrontation between modern mathematics and Greek mathematics. But it was not the only great achievement of the nineteenth century, far from it; and against the background of what the eighteenth century had been preparing for, it was not a startling innovation but rather more a "natural" event, albeit a very notable one.

It was providential that the seventeenth and eighteenth centuries, especially the seventeenth, took the Euclidean structure of space for granted and knew of no other. Newton did find the law of gravitation for Euclidean space. But if, by some inspiration, he would have known about non-Euclidean possibilities and tried to find the law of gravitation directly for a space with curvature, he might have been led woefully astray. Greek mathematics, viewed in retrospect, undertook to develop things out of order and came to grief. Instead of developing a concept of real number as a common mathematical measure for physical quantities, Greek mathematics undertook to develop directly a mathematical theory of general physical quantities. They seemingly succeeded, as

ing information from a number of sources. Increasingly, it seems likely that indefinite propagability of differentiated states at the cell level cannot safely be assumed from the earlier evidence, and that the widely accepted conclusion is based on a confusion of levels in both concept and technique. It is now recalled that none of the tissue culturists of the early period dealt with individual cells or with populations of free cells. The technology of the period yielded *tissue* cultures, cell populations at varying levels of integration which "grew" in total mass. Little or no information was available as to the proportion and character of the original cells which survived and actually underwent propagation, on the number of cell types present in the original explant, on possible covert changes of type during cultivation, on the degree of interaction affecting differentiation going on between cells of like and unlike type. Observations at the level of complex and integrated populations gave rise to reasonable, but not required, assumptions about the differentiative state of individual cells, and these assumptions in turn lead to reasonable, but not demonstrated, conceptions about mechanisms operating at subcellular levels.

This kind of easy extrapolation from level to level is logically indefensible and, in the present case, the question must be raised whether it did not lead to inaccurate conclusions. Studies of increasing cogency are now being directed at the question whether differentiating or differentiated cells can divide repeatedly to give rise to many cells of the same differentiative type. Focusing on this question *in vitro* immediately emphasizes the difficulties of recognizing and defining differentiative states in cultured cells, and the complexities of the "adaptation" undergone by fresh explants before they yield stable, vigorous cultures. It is now clear that only a highly selected sample of the original cell population acts as progenitor of stable cell strains, and that even the progenitors may not have the same properties as the cells of a derived stable strain. For example, most, if not all, of the cell strains isolated up to a year or two ago, have drastically altered chromosome number and morphology. While such strains are extremely useful for certain purposes, it is hardly surprising that it is difficult to establish differentiation—in the *in vivo* sense—or the capacity to recover it. As more information has become available on cells grown *qua* cells, it has more and more appeared that they show commonness rather than diversity with respect to criteria normally applied to differentiation. Cells derived from liver, for example, fail to show the characteristic conversion of phenylalanine to tyrosine, cells derived from connective tissue fail to produce collagen. Persistence of specialized characteristics in long-term proliferating cultures has been reported in only a few instances, each with its complicating circumstance which makes interpretation difficult. Long-term cultured cells certainly are not the product of simple reversal of differentia-

the fifth book of Euclid and the monumental works of Archimedes and Apollonius testify. But the resulting mathematics was too inflexible to adjust itself to needs of mechanics and physics, and too cumbersome and heavyweight to propel even itself far beyond the platform on which it had gained its crown of laurels.

In the evolution of modern mathematics and mechanics a contingent dis-harmonious feature was the timing of the rise of the great Kantian philosophical system. Having come into being in the eighteenth century it tied itself to Euclidism, and in fact to a doctrinaire seventeenth century version of it, and adherents in later generations wished it were not so.

The Principle of Least Action

A lively activity in the field of eighteenth century mechanics and mathematics was the search for a suitable version of the principle of least action. The search was beset with difficulties and misunderstandings because, as seen in retrospect, it involved the search for the concepts of both kinetic and potential energy on a level of quantitative precision. These concepts however were apparently difficult and recondite, and, in fact, only the nineteenth century was equipped to master them.

The search for the concept of energy began early. A notion of energy, or perhaps a set of companion notions, was a dominant and ever recurrent feature of Aristotle's philosophical system. One may even claim for him that the distinction between kinetic and potential energy was already foreshadowed in his work, especially in his famous statement that "motion (that is the Aristotlean kinesis) is the actualization of that which is potentially, as such." But in cold fact it must be said that there is a long distance between his notion of energy and a concept, or even preconcept, of energy that fits into mechanics as we understand it. Some of his medieval followers may have come closer to it, but the important researches of Anneliese Maier suggest that the Schoolmen had great difficulties in keeping apart force, energy, and momentum.

Almost everybody in the seventeenth and eighteenth centuries had something to say about *vis viva*, that is kinetic energy, except Isaac Newton who most prudently avoided getting involved in it, perhaps because, unlike force, energy could not be spatially exhibited. Huygens, on the other hand, was keenly interested in it and made an important application of it. He reasoned that, in an elastic collision of two masses, not only that total translational momentum remains continuous throughout the collision, which fact had already been known before, but the total kinetic energy too.

Lagrange was the first to introduce potential energy. He also gave a satisfactory version of the principle of least action, and in it he also uses the concept of total energy as the sum of kinetic and potential energy. The Lagrange version of the principle has become overshadowed by the

tion, but there is considerable question whether their properties, and the manner in which they alter, have a simple relationship to differentiation *in vivo*.

One of the limitations in approaching this problem has been the scarcity of objective, quantifiable criteria for differentiative states—particularly in their early or weakly manifested phases. Early culturists found that shape, pattern, and other relatively gross morphological characteristics, relied on in the fixed preparations of classical histology and cytology, varied widely depending upon culture circumstance. The early culturists took refuge in an assumed underlying "chemodifferentiation" to explain the populational stability observed in their cultures. The absence, at the time, of suitable techniques to observe and measure differentiation at the molecular level left this a plausible but untested hypothesis. Today, possible molecular approaches have expanded enormously, and are being successfully applied to the problem of cytodifferentiation—particularly to heightening the sensitivity of its detection. For example, the differentiation of cartilage is known to involve the secretion of an intercellular matrix rich in sulphated mucopolysaccharides. Sulphate, in radioactive form, can be administered to tag these materials and follow them as they are synthesized within the cell and secreted into the matrix. To the extent that this behavior is characteristic of cartilage it can identify and measure cartilage differentiation objectively and reproducibly. Employing this criterion at the molecular level, a recent study (Holtzer, *et al.*, 1960) shows that cartilage cells cultured for periods as short as five days lose their recognizable cartilage-like properties, not only at the morphological level but at the molecular as well. It is important to note that the test is conducted not under conditions in which the cells propagate, but when they are transferred to circumstances under which uncultured chondrocytes, or those cultured for periods less then five days, will undergo cartilage differentiation. This recent multi-leveled study, therefore, gives no support to the conclusion from earlier single-leveled studies that differentiative states are indefinitely propagable. It suggests rather that the capacity to synthesize sulphated mucopolysaccharides disappears fairly promptly in cultured chondrocytes. Further technical refinement of the approach may make it possible to say through how many cell generations, if any, the specific synthetic capacity may pass.

If it were found, in these experiments at cell and molecular levels, that chondrocytes lose their specific synthetic capacity as soon as they divide, would it follow that culture in any form of proliferating cartilage-forming cells is impossible? Not necessarily, if we keep in mind that cytodifferentiation *in vivo* occurs in the context of a higher level of integration and interaction between diverse tissues. The experiments referred to were carried out using nutrient media, and under conditions elaborated

Hamiltonian principle, but Lagrange can claim credit for the fact that his version, which he found much before Hamilton, is in basic mathematical structure the more difficult one.

Although Lagrange introduced potential energy and applied it, the theory of it was elaborated by others (Laplace, Poisson, Gauss, Green, etc.). Also the actual interest in potential energy came not from mechanics but from electrostatics in which it is the identifying concept, almost. Similarly the concept of total energy of a system, although originating in mechanics, acquired its status in the theory of heat to which, again, it is the dominant concept.

Theoretical Physics, as against rational mechanics, fully began only with the concept of energy, after mechanics had completed its main phase without yet concentrating on it. The (prequantum) energy was measured by arbitrary real numbers, and it is perhaps more than a coincidence that the proper conceptualizations of real numbers and of energy were taking place in the nineteenth century virtually simultaneously.

Simple cases of the Lagrange version of the principle of least action are rarely presented in textbooks in analytical detail and we are going to append one.

Let T denote the kinetic energy of a conservative system, U its potential energy and let t denote time. The principle states that the equations of motion are obtained by the variation

$$\delta' \int_{t_0}^{t_1} 2T\, dt = 0 \tag{9}$$

under the side condition

$$\delta'(T + U) = 0, \tag{10}$$

where δ' denotes the variation not only of the space variables but also of the time scale, both between fixed endpoints.

Consider the case of a mass point m moving on the x-axis. Its kinetic energy is

$$T = \frac{m}{2}\left(\frac{dx}{dt}\right)^2$$

and we have to vary the integral

$$m \int_{t_0}^{t_1} \left(\frac{dx}{dt}\right)^2 dt. \tag{11}$$

Since t is also to be varied we must introduce a function $t = t(u)$ over an interval $u_0 \leq u \leq u_1$. The expression (11) is then

$$m \int_{u_0}^{u_1} \left(\frac{dx}{du}\right)^2 \left(\frac{dt}{du}\right)^{-1} du. \tag{12}$$

We must now replace x by $x + \epsilon\xi$ and t by t $+ \epsilon\tau$ where ξ and τ are finite displacements and ϵ is a small real parameter.

over the years to *grow* cells rather than to achieve their differentiation. When somite tissue about to form cartilage is cultured by these procedures it will not form cartilage—unless there is included with it, in intimate association, a bit of embryonic spinal cord. A number of instances of this kind of developmental dependency between tissues are known, both *in vivo* and *in vitro*. We have recently been examining in our laboratory a tissue interaction in the development and differentiation of the mouse pancreas (Golosow and Grobstein, 1962). The intact pancreatic rudiment includes a central epithelial portion—the source of enzyme-secreting acinar cells, duct cells, and hormone-secreting islet cells—and a surrounding mesenchyme, precursor of the interstitial connective or packing tissue of the adult gland. When the intact rudiment is placed on a porous platform with an underlying nutrient solution, it continues reasonably normal development. In particular, after several days in culture, acinar cells appear with characteristic, conspicuous zymogen granules in their cytoplasm. If, however, the epithelial component is separated from its mesenchyme before being cultured under otherwise identical conditions, it shows little or no growth and entirely fails to differentiate acinar cells. If it is recombined with its mesenchyme, and cultured, the behavior of the epithelium is indistinguishable from that of the intact rudiment.

It is clear that if one wants to study the differentiation of pancreatic epithelial cells *in vitro* one cannot, in the present stage of culture technology, proceed with isolated cells. One cannot even proceed with seemingly relatively homogeneous cell aggregates such as the epithelial component of the early rudiment. Such aggregates require an external contribution which is not supplied by existing culture media, but is supplied by the associated mesenchyme. We do not yet know what the mesenchymal contribution is, but we do know that the effect can be exerted not only in direct combination of the two tissues but when they are placed on opposite sides of the thin porous platform. In studies of other, but perhaps similar, interactions we have found that they occur over only limited distances and in the presence of living cells. This suggests that they may involve materials normally associated with the surfaces of cells, and not present in large quantities. From studies of this sort at the tissue and organ level may come information as to molecular materials to be added to culture media so as to achieve differentiation at the cell level.

The fact is, of course, that the early tissue culturists quickly discovered that to obtain differentiation reliably they should resort to "organ culture"—procedures to maintain *in vitro* cell populations in the complex and heterogeneous state found *in vivo*. Culture of early embryonic rudiments as a whole, or small pieces of later stages so treated that they did not disperse to cells but remained "whole," yielded remarkably

If we substitute this in (12) we obtain a function

$$A(\epsilon) = m\int_{u_0}^{u_1}\left(\frac{dx}{du}+\epsilon\frac{d\xi}{du}\right)^2\left(\frac{dt}{du}+\epsilon\frac{d\tau}{du}\right)^{-1}du \tag{13}$$

and the requirement (9) is

$$\frac{dA(\epsilon)}{d\epsilon}\Big|_{\epsilon=0} = 0.$$

By carrying out the differentiation in (13) and then putting $\epsilon = 0$ we obtain

$$2m\int_{u_0}^{u_1}\frac{dx}{du}\frac{d\xi}{du}\left(\frac{dt}{du}\right)^{-1}du - m\int_{u_0}^{u_1}\left(\frac{dx}{du}\right)^2\left(\frac{dt}{du}\right)^{-2}\frac{d\tau}{du}du = 0. \tag{14}$$

Next, for the requirement (10) we have to introduce the function

$$B(\epsilon) = \frac{m}{2}\left(\frac{dx}{du}+\epsilon\frac{d\xi}{du}\right)^2\left(\frac{dt}{du}+\epsilon\frac{d\tau}{du}\right)^{-2} + U(x+\epsilon\xi),$$

and the requirement itself is

$$\frac{dB(\epsilon)}{d\epsilon}\Big|_{\epsilon=0} = 0.$$

By carrying out the differentiation and putting $\epsilon = 0$ we obtain

$$m\frac{dx}{du}\cdot\frac{d\xi}{du}\left(\frac{dt}{du}\right)^{-2} - m\left(\frac{dx}{du}\right)^2\left(\frac{dt}{du}\right)^{-3}\frac{d\tau}{du} + \frac{\partial U}{\partial x}\xi = 0.$$

From this we obtain

$$m\left(\frac{dx}{du}\right)^2\left(\frac{dt}{du}\right)^{-2}\frac{d\tau}{du} = m\frac{dx}{du}\frac{d\xi}{du}\left(\frac{dt}{du}\right)^{-1} + \frac{\partial U}{\partial x}\xi\frac{dt}{du},$$

and if we substitute this into (14) the result is

$$\int_{u_0}^{u_1}\left[m\frac{dx}{du}\frac{d\xi}{du}\left(\frac{dt}{du}\right)^{-1} - \frac{\partial U}{\partial x}\xi\frac{dt}{du}\right]du = 0. \tag{15}$$

Thus, the time displacement τ has been eliminated and we can now return from the variable u to the original variable t. Then, (15) becomes

$$\int_{t_0}^{t_1}\left[m\frac{dx}{dt}\frac{d\xi}{dt} - \frac{\partial U}{\partial x}\xi\right]dt = 0. \tag{16}$$

Now, by partial integration we have

$$\int_{t_0}^{t_1}\frac{dx}{dt}\frac{d\xi}{dt}dt = \frac{dx}{dt}\xi\Big|_{t_0}^{t_1} - \int_{t_0}^{t_1}\frac{d^2x}{dt^2}\xi\,dt. \tag{17}$$

Next, we assume that the displacement ξ is 0 at the time endpoints t_0, t_1, and we substitute (17) into (16). The result is, after changing the sign,

$$\int_{t_0}^{t_1}\left[m\frac{d^2x}{dt^2} + \frac{\partial U}{\partial x}\right]\xi\,dt = 0.$$

Now, for a given function $x(t)$ between the limits t_0, t_1 this must hold for any displacement $\xi(t)$ which is 0 at t_0 and t_1. This can only occur if the factor of ξ under the integral is 0, that is

$$m\frac{d^2x}{dt^2} + \frac{\partial U}{\partial x} = 0.$$

But this is indeed the equation of motion of a mass point m on the x-axis subject to a force which is derived from a potential $U(x)$.

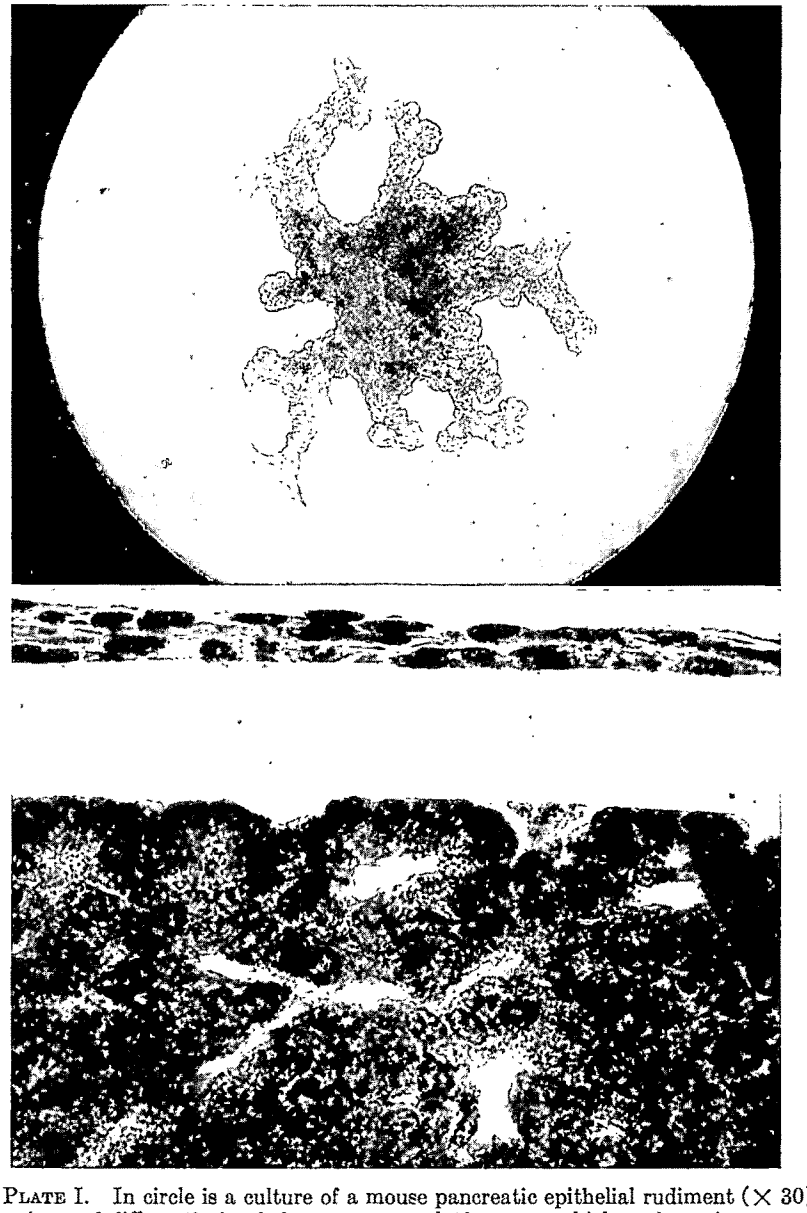


PLATE I. In circle is a culture of a mouse pancreatic epithelial rudiment (× 30) growing and differentiating below a porous platform on which embryonic mesenchyme is spreading. Below is a section (× 800) through such a culture on the fifth day showing the thin mesenchyme on top of the platform with clusters of pancreatic cells (acini) containing prominent secretion granules (zymogen granules) below the platform.

TELLURIUM

By ALLEN NUSSBAUM

TELLURIUM is an elemental semiconductor for which the supply exceeds the demand. This, combined with the fact that it is dangerous to handle and difficult to understand, explains why only a few laboratories throughout the world are studying its physical and electrical properties. In contrast, the semiconducting elements germanium and silicon have been intensively investigated ever since World War II. Another semiconducting element, selenium, which is closely related to tellurium (see Column VI of the Periodic Table) has been used as a rectifier in communications equipment for many years.

TABLE I

PERIODIC TABLE, COLUMNS IIIA TO VIIA AND INERT GASES

IIIA	IVA	VA	VIA	VIIA	Inert Gases
5	6	7	8	9	10
B	C	N	O	F	Ne
13	14	15	16	17	18
Al	Si	P	S	Cl	A
31	32	33	34	35	36
Ga	Ge	As	Se	Br	Kr
49	50	51	52	53	54
In	Sn	Sb	Te	I	Xe
81	82	83	84	85	86
Tl	Pb	Bi	Po	At	Rn

Tellurium was discovered in some gold ores in Transylvania in 1782 by F. J. Müller (Baron von Reichenstein) [1] but he apparently confused it with antimony. In 1798 M. H. Klaproth extracted the element from the gold-bearing mineral and named it after the Latin *tellus*, the earth. Referring again to the Periodic Table, we see that tellurium is in the same column as oxygen and sulfur, which are insulators, and with polonium, which is a metal. A similar sequence is found in Column IV, where carbon in the diamond form is an insulator, silicon, germanium, and gray tin are semiconductors, and white tin and lead are metals. This analogy carries over into the crystal structure, since silicon, germanium, and gray tin have the diamond form and selenium, tellurium, and polonium are related in a way to be described. Figure 1 shows a sketch of a scale model of tellurium; one form of selenium has the same structure, but the dimensions are somewhat different. To help visualize the involved sym-

normal differentiation, including the characteristic cellular changes. An important point emerged from these studies, in agreement with many observations on developing systems: cytodifferentiation takes place most readily, (1) in cell aggregates beyond a minimum size, and (2) in the presence of cells of unlike type. This suggests that the changes of cell properties occurring in cytodifferentiation are strongly tied to the intimate environment found in supercellular aggregates of some complexity. To understand the cell changes better we certainly would like to free them from the complexities of the supercellular system. However, without better understanding of the conditions supplied to the cells by the supercellular system we do not seem to be able to supply to dispersed cells an environment in which they will show the phenomenon under investigation. Given these difficulties we must *neither* concentrate on the complex level and draw gratuitous conclusions through the maze of complexity to the cell, nor concentrate on the cell, under grossly oversimplified conditions of culture, and attempt to interpret the changes it undergoes as differentiation—whether or not there is objective evidence that these changes are similar to those seen in the organism. What we *must* do is to recognize that we have a multi-leveled phenomenon, and that part of our problem is to unravel the relationship between the levels. This we can *only* do by approaching the phenomenon with tools and concepts appropriate to each of the several relevant levels, carefully avoiding transfer of conclusions from one level to the other until we fully understand the conversion factors between them.

In saying this, I come to my second main point and to the heart of the problem of levels as it relates to development. In studying most biological phenomena, approaches are made at several levels, and questions frequently arise as to the relationship between the conclusions drawn at each of these levels. But *in development the organism passes from level to level*, and the conceptual problem, in part, precisely is to understand *the process of conversion of properties at one level into those of the next.* To illustrate this, let us begin by considering the implication for development of recently derived hypotheses of biochemical genetics. Developmentalists have long known that they were observing the product of a complex interaction of hereditary endowment and environmental conditions. Geneticists increasingly assign the hereditary component to desoxyribonucleic acid in the chromosomes, believing that hereditary characteristics are expressed at this level in particular sequences of only four component nucleotides: adenine (A), guanine (G), cytisine (C) and thymine (T). At fertilization, in these terms, the union of sperm and egg deposits in the zygote nucleus a hereditary "program" for development, written in a molecular code of nucleotide sequence, e.g., AAGTCT.... The zygote of a hereditary human hemophiliac differs from a normal, on this hypothesis, only in a relatively small alteration of nucleotide se-

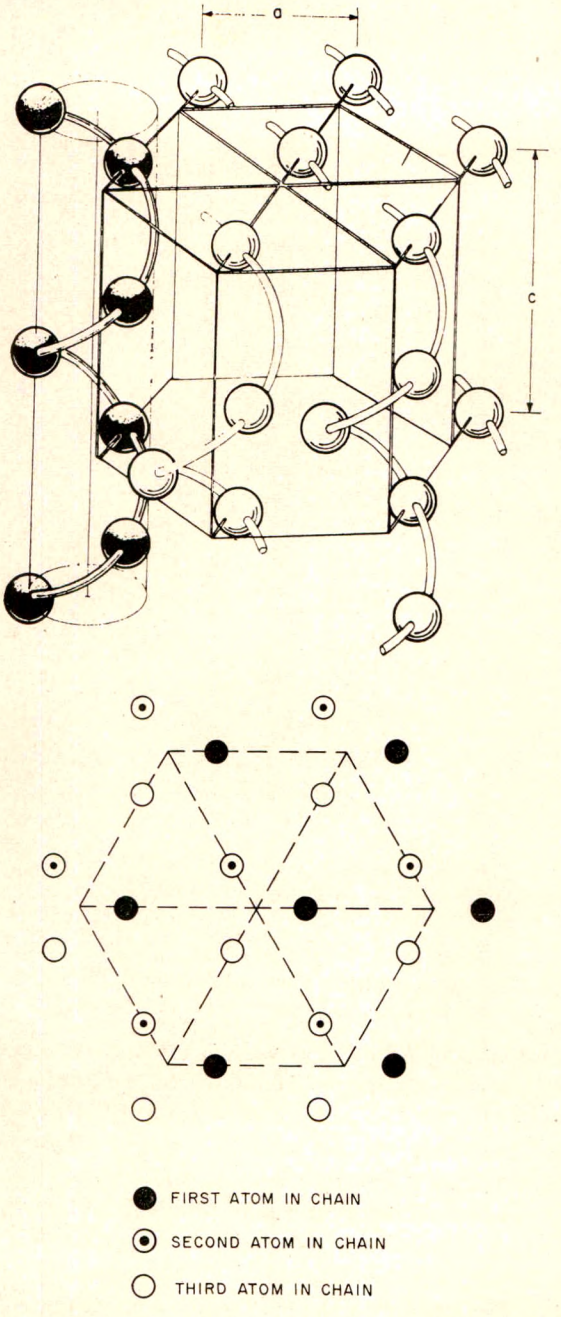


FIG. 1. The arrangement of the atoms in a tellurium crystal, drawn to scale. The end-on view shows the three-fold nature of the spiral chains and the hexagonal arrangement of the chains. The lines connecting opposite corners of the hexagon are axes of two-fold symmetry.

quence. During the course of development this small difference in properties at the molecular level is translated into the major, and frequently critical physiological, behavioral and social differences represented by the disease of hemophilia. It is clearly our business to understand fully this process of translation.

The biochemical geneticist and cytologist have made spectacular progress in working out some of the intracellular steps in the translation. There is strong support for the hypothesis that the first step involves the close relation between DNA and the synthesis of other key molecules of the cell. In particular, it is highly probable that the nucleotide sequence of the nucleic acid determines the amino acid sequence of proteins and that these, acting enzymatically, are critical factors in establishing and regulating cell properties. The chain of translatory steps from the level of molecular properties to that of cell properties is by no means completely revealed, but sufficient outline has emerged to give confidence that time will paint a satisfying picture.

Now development, in the usual sense, begins with the cell, i.e., virtually all modern organisms of some complexity return at some time to the cell level of organization to begin their individual life histories. In so doing they rest their confidence, in the biological sense of survival, on their ability to retranslate cell properties into those of higher organisms. As noted above, the cell properties of the zygote are in part set by the molecular properties of its genetic DNA. They also are set by the character of the materials supplied and the conditions imposed during maturation of the egg in the maternal ovary, and in the subsequent history prior to fertilization. The total complex of cell-properties which is the zygote begins its translation into the property-complex of a multicellular organism when the zygote begins to divide or cleave. At this point something new emerges—multicellularity—upon which everything subsequent depends.

Let me take a moment to examine the expression "something new emerges." The notion of emergence has been badly distorted in some philosophically oriented treatments and, in consequence, is rejected in many quarters. I submit that a theory of development cannot be complete, or even adequate, without the concept of emergence in some form, and that it is the business of developmentalists not to avoid the concept but to give it specific and concrete meaning. It is, indeed, at the very root of the translational devices which gradually convert the bare abstraction of genetic programming into the fully realized living organism. In the instance of cleavage of the zygote a new property—cohesiveness—emerges which is the *sine qua non* of multicellularity, and hence of the entire complex of properties dependent on multicellularity.

Is cohesiveness truly a *new* property, materializing in an instant at the moment of cleavage? In one sense, yes, in another sense, no, and in this

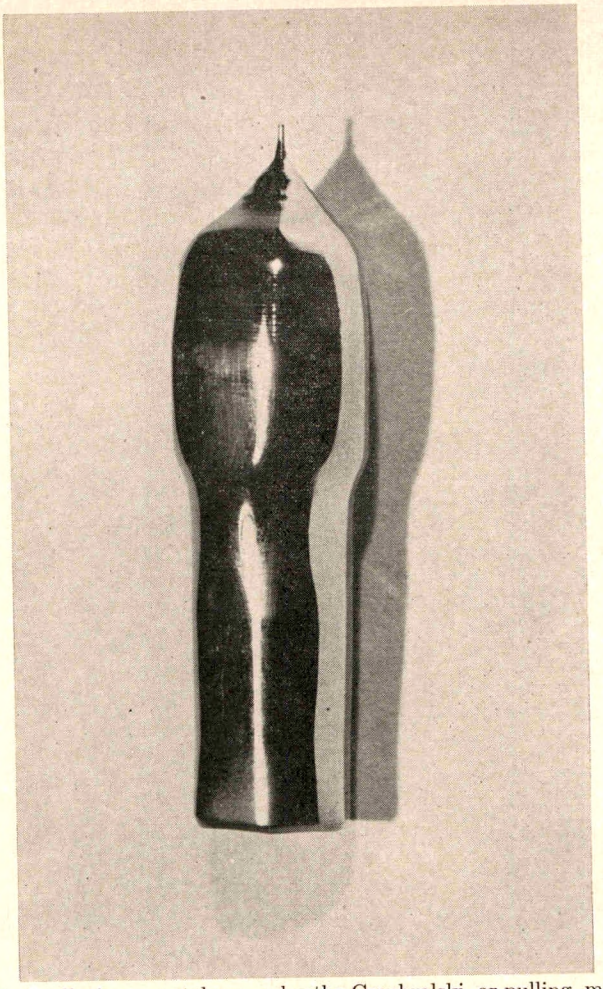


FIG. 2. A tellurium crystal grown by the Czochralski, or pulling, method. The seed is the narrow region at the top.

metry properties, an end-on view is also shown, and it will be seen that the atoms are arranged in spiral chains. Each spiral has three atoms to a complete turn, so that atom number 4 lies directly over atom 1, atom number 5 is over number 2, and so on. The chains are joined together in hexagonal bundles, but because there are only three atoms to each turn of the spiral, the tellurium crystal has threefold, or trigonal, symmetry. However, the hexagonal arrangement is seen in the external form of tellurium crystals, as the photograph (Fig. 2) shows. The other main symmetry feature of tellurium is the presence of twofold axes which are perpendicular to the central axis of the spiral and pass through each atom in the chain.

Crystals of the size and perfection shown in the photograph were first

ambiguity lies part of the answer to the puzzle of emergence. Cohesiveness among cells clearly is a new property, for it cannot be displayed by a single cell, e.g., a zygote. It is only when the cell divides that the question of coherence arises; it emerges as a factor *in the new context* produced by cleavage. At this point it is critical—noncoherence leads to an increase in the number of isolated individual cells, as in bacteria or protozoa. Only coherence can lead to an increase in number of cells within a single organism, as in metaphyta and metazoa. In this sense, coherence is a vital new property emerging at the moment of cleavage. But the new property is, of course, not totally unrelated to what went before; it does not arise *de novo*, without history. Rather the contrary, the new property was already imminent and implicit in what went before; it is only translated—in this case to a new level—at the moment of emergence.

To make the matter more concrete, let us look at some of the information available on cohesiveness among cleavage products in the sea urchin egg. It has been known for many years that major surface changes in the egg accompany fertilization (Runnström, *et al.*, 1959). These changes are conspicuously manifested in the sea urchin by the lifting from the surface of the so-called fertilization membrane. The appearance of the membrane is a convenient indicator of the fertilization process, and it is within the fluid-filled confines of the membrane that subsequent cleavage normally occurs. It was natural to assume that the membrane plays an important role in holding the blastomeres together. This simple and obvious hypothesis of cohesion of blastomeres is not tenable, however, in light of the following facts.

Surgical removal of the fertilization membrane does not affect either the cohesion of the blastomeres or the character of further development. The membrane cannot, therefore, be essential either as a mechanical binder or as an insulator of the cohesive mechanism from disruptive effects of the general sea water environment. The chemical composition of the sea water *is* relevant, however; particularly its content of divalent cations. It was shown at the turn of the century, that cleaving sea urchin eggs placed in calcium-free sea water fall apart into individual blastomeres and subsequent development is blocked. More recently it has been found that treatment with the proteolytic enzyme, trypsin, or exposure of the cleaving egg to somewhat alkaline sea water, has similar effects (Hagstrom and Hagstrom, 1954). Particularly interesting is the fact that treatment of unfertilized eggs with urea or isotonic sucrose at alkaline pH suppresses fertilization membrane formation, and yet allows cleavage to proceed (Moore, 1954). Under these cicumstances, however, the cohesiveness of the blastomeres is altered so that instead of a three-dimensional embryo there results a thin two-dimensional plate whose development is abortive. These results suggest that even though the fully formed fertilization membrane is not essential to cohesion of blasto-

grown in this country by T. J. Davies [2] of the Honeywell Research Center. These crystals were grown by the Czochralski method, and the procedure involves dipping a small piece of single crystal, called a seed, into a crucible of molten tellurium. By slowly withdrawing the seed, and carefully maintaining the crucible at a temperature just above the melting point of tellurium (about 450°C), the atoms in the melt will solidify onto the seed with the same orientation as its own atoms. In the photograph, the seed is the narrow region at the very top. Prior to the application of this method, which for obvious reasons is also known as

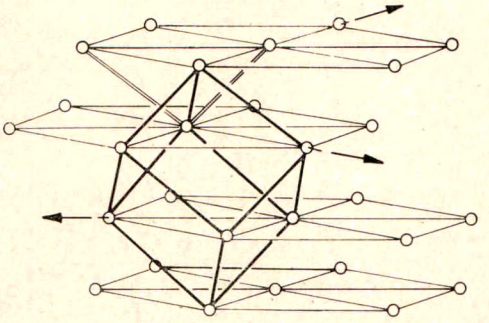


FIG. 3. The simple cubic lattice of polonium and its relation to the trigonal screw-axis structure of tellurium. Sliding each of three adjacent planes in directions making angles of 120° with each other will result in the structure of Figure 1.

crystal-pulling, the Bridgman technique has been used. This consists essentially of lowering a small-diameter tube through a furnace which is hot at the top and cooler at the bottom, so that the polycrystalline tellurium pieces melt and then solidify into a single crystal. These crystals are small in diameter (a few mm at the most), but, recently, the group at the Ecole Normale Superieure in Paris, under Prof. Pierre Aigrain, have succeeded in producing large crystals by this method.

Tellurium has some interesting, and highly annoying, physical properties [3]. It is a brittle material and cleaves very nicely in directions parallel to the hexagonal faces by merely tapping lightly with a razor blade, yielding bright, mirror-like surfaces. On the other hand, it is also soft, and it has been noticed that carrying it nested in cotton-wool will scratch the surface. It is also highly sensitive to any mechanical action, and it is very easy to introduce dislocations into the lattice. Dr. Robert J. Stokes of the Honeywell Research Center has discovered that sprinkling silicon carbide grains from a height of three inches will introduce a large number of tiny microcracks on the surface of a tellurium crystal. It has also been found that dislocations are introduced by merely picking up samples with tweezers and the best method of carrying tellurium is to slide it onto a sheet of paper.

meres, there may be some connection between its formation and the cohesive mechanism.

Electron microscopy, coupled with certain cytochemical procedures, has provided clues as to what that connection may be (Afzelius, 1956). In the unfertilized egg there exists a layer of small granules in the stiffer periphery, or cortex, of the egg. These granules disappear in a wave running around the egg from the point of sperm entry, in close correlation with the appearance of the fertilization membrane. The granules, rich in mucoid materials, undergo partial solubilization, apparently under the influence of substances introduced by the sperm. Some of the solubilized materials reaggregate on the inner surface of the thin cytoplasmic membrane, thickening it into the conspicuous fertilization membrane. Increased colloid osmotic pressure resulting from the solubilization may be responsible for attracting water below the membrane and lifting it off the surface of the egg. In any event, some of the cortical granule material remains on the new surface of the egg as a thin mucopolysaccharide-rich film—the hyaline plasma layer. It appears to be this layer whose stability is dependent upon the presence of divalent cations, and which is hydrolyzed by trypsin. The transformations which produce this layer markedly affect the responsiveness of the egg surface to sperm—are related, for example, to the block to entry of supernumerary sperm which is an important aspect of the fertilization process. Beyond this, however, it is this layer which persists as a continuum binding the cleaving blastomeres. Pretreatment of the unfertilized egg which interferes with normal discharge of the cortical granules affects both membrane formation and subsequent blastomere adhesiveness, because both involve contributions from the granule substance. But it is the inconspicuous hyaline plasma layer, and not the conspicuous fertilization membrane, which is critical for subsequent multicellular development.

When, then, does multicellularity emerge? As a phenomenon, clearly at cleavage and based on the cohesion provided by the hyaline plasma layer. But the material of the hyaline plasma layer does not emerge at that time; it was present in the fertilized egg and even earlier, in precursor form, in the unfertilized egg. There was a translation of *state* as the cortical granules solubilized and, in part, reaggregated as the hyaline plasma membrane. The translation in state occurred in the context of fertilization, as part of important surface changes essential to the biological effectiveness of this process. Subsequent to this, without—so far as is known—further change of state, the hyaline plasma membrane becomes a mechanism of cohesion through a translation of *context* brought about by cleavage.

It is of considerable interest that this primitive form of cohesion established by the hyaline plasma layer of sea urchins probably is not a special case, and probably is only a primary step in the story of multicellular

Referring to the Periodic Table again, the element shown at the bottom of Column VIA is polonium, whose crystal structure at temperatures below 70°C is simple cubic, the only known example of this kind of crystal. It was pointed out by A. J. Bradley in 1924 that there is an intimate relation between the simple cubic lattice and the spiral chain structure of tellurium and selenium, as shown in Figure 3. It is an easy exercise in spatial visualization to move three adjacent planes in directions located at angles of 120° to each other, all by the same amount, to produce the tellurium lattice previously shown.

The presence of the screw axis permits an interesting phenomenon known as *enantiomorphism,* which is simply the existence of right-handed or left-handed crystals. Dr. K. C. Nomura [4] has discovered the existence of the two types of seeds grown more or less by accident when tellurium vapor condensed in the colder regions of a furnace. From these, the corresponding crystals were grown, and they showed the ability to rotate polarized light in opposite directions. This effect was discovered in tartrate crystals by Pasteur in 1848.

The physical peculiarities of tellurium mentioned above are merely by-products of the basic program on this element. The two main objectives of the investigation have been to study the *energy-band* structure and the *scattering* properties. Let us consider these ideas, discussing the physical concepts and the experimental techniques involved.

We start by realizing that, in present-day physics, the electron is considered to be a dual entity, showing both particle and wave properties. Considering now the wave aspect, an electron travelling through a crystal can be compared to a group of identical masses and springs arranged in a ring (Fig. 4) and lying on a frictionless table. The spacing between the masses is analogous to the uniform separation of the atoms in a crystal and the circular arrangement corresponds to a convenient mathematical fiction known as *periodic boundary conditions.* Any real crystal must have a surface and in order to avoid the complications inherent in the sudden termination of the lattice, we regard the crystal as being bent around to join opposite sides, thus giving rise to the closed ring of the illustration.

If one of the masses in the ring is now displaced a slight distance along the circumference and then released, the entire group will oscillate. The amplitudes of oscillation must be related to one another in a fashion which will prevent self-cancellation, and it can be seen from the examples in Figure 4 that these must be spaced at distances corresponding to integral multiples of the distance separating the masses. This can be stated more precisely as the requirement that the wavelength of the disturbance be an integer times the lattice spacing. Now, to each one of these wavelengths there corresponds a distinct frequency of oscillation, and it has been found that if the relation between frequency and wavelength is calculated, a curve like that in the upper part of Figure 5 is obtained. In

cohesion. That it may be a general phenomenon is shown by the fact that in both fishes (Trinkaus, 1951) and amphibia (Holtfreter, 1954) there is evidence for the existence of similar mucoid surface coats, acting as continua to bind together the cleaving cells and integrate their movements in the fundamental dance of germ layer formation. Moreover, the same general factors which disrupt the hyaline plasma layer of the sea urchin egg, i.e., absence of divalent cations, high pH and proteolytic enzymes, are effective in reducing cell collectives to cell suspensions in such diverse organisms as slime molds, sponges and higher vertebrate embryos (Moscona, 1960). That this primary mechanism of binding by mucoid continua is supplemented by other devices is suggested, even in early stages of the sea urchin embryo, by electron microscopic observations showing interdigitation of the surfaces of neighboring blastomeres, and particularly by the presence of what appear to be specialized local adhesive patches. The whole question of the operation and mechanism of intercellular cohesion has been given an added fillip by the remarkable specificity shown in the spontaneous reaggregation of dissociated cells. Intermixed cells from different sources sort out in ways which seem only explicable on the assumption of a high information content—in some instances as to species type, in other instances as to tissue type—in their cohesive mechanisms. Is this borne in materials comparable to the hyaline plasma layer? Is the specificity of cell association—in turn underlying the orderliness of tissue and higher level architecture—a translation of molecular specificity of mucoid materials? Is mucoid specificity a "missing link" in the translation of genetic information into the marvel of phenotypic order and complexity?

These seem to me to be among the most fundamental questions in developmental biology today. They are typical of the kind of questions which demand a multi-leveled approach—because they are trans-level questions requiring simultaneous consideration of molecular, cellular and super-cellular levels. I believe that biological problems increasingly are and will be of this character, with primary focus on the knotty issue of translation of properties between levels. This brings me to my third and final point—the importance of continued interest and competence among biologists in levels above the molecular, and particularly the future requirement for a healthy proportion of biologists who, as individuals, are competent at all levels.

There is growing awareness that biology is progressing through a revolution, as a discipline and as a community. This is not the place to analyze how or why the revolution began, nor to make value-judgments. It is my purpose only to note as facts that certain areas of biology have expanded drastically relative to others; that the community of biologists has increased, and been diversified and strengthened by recruitment from other disciplines; and that the modal core or center of biological

this curve, we have plotted frequency *versus* the reciprocal of the wavelength, since this turns out to give a convenient uniform spacing on the horizontal scale, and the distinct pairs of values are indicated. If we make the physical situation slightly more complicated by replacing every other mass with one which is considerably heavier, it is found that two frequencies are permitted for each possible wavelength, and that these frequencies are grouped into sets, or *bands*, the two bands being separated by a wide region of forbidden frequencies. Turning now to the tellurium crystal, the characteristic of an electron which corresponds to the fre-

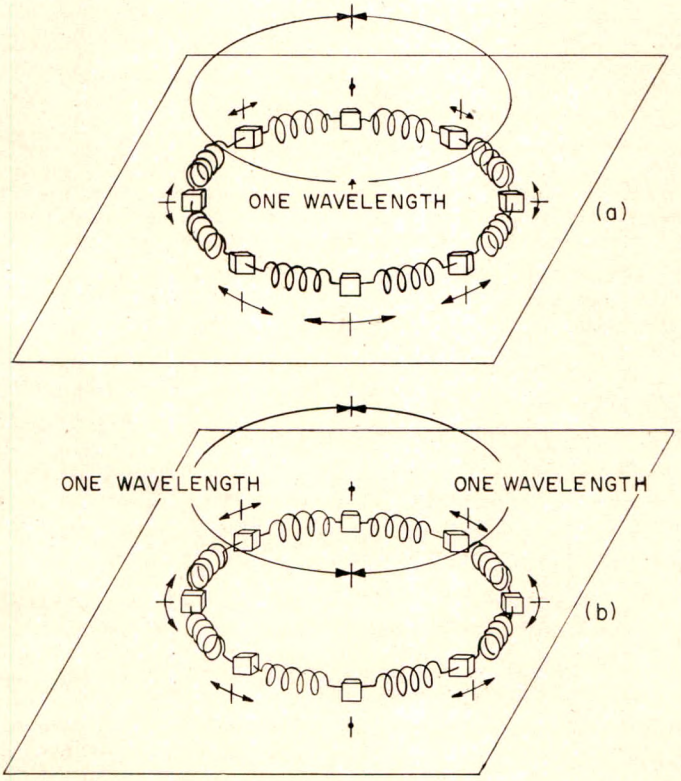


FIG. 4. The vibrations of a group of masses and springs lying on a frictionless table serves as an analogy to the passage of an electron through a periodic crystal. The undisturbed position of each mass is indicated by the vertical mark and the arrows show the displacement each mass has made at the instant we are observing the motion. The upper figure shows one possible type of motion with a wavelength λ equal to eight lattice spacings; in the lower figure, the wavelength λ is four lattice spacings.

quency of the vibrating ring is its energy, and in Figure 6 we show the way in which this quantity depends upon the reciprocal of the electron's wavelength. These curves were obtained partly on the basis of experi-

thinking has shifted accordingly. The consequences of this revolution are now coming to bear on issues of training of future biologists, on undergraduate and graduate curricula, and it is not surprising that much experimentation in these areas has begun, at least in certain institutions.

It is well known that transmission of knowledge inevitably involves selection and ordering, and that this imparts creativity to teaching. The vast diversity of biological knowledge, and the relative tenuousness of its logical structure, have posed in the past a special problem in selecting and ordering. Biologists of the late nineteenth century taught as they thought—predominantly in terms of the successful ordering devices of their time, the powerful concepts of taxonomy, evolution, and phylogenesis. Biologists of the first half of the twentieth century grafted to this structure, essentially by terminal addition, the various specialized disciplines as they emerged—embryology, genetics, physiology, cytology, immunology. The older knowledge came first, the newest tended to come last, and deep indeed was the interest and perseverance of the student who survived to become up-to-date. Clearly, this had to be changed, and it is well that a beginning is being made.

I do not intend to discuss or propose particular solutions to the problems which are thus raised. I wish only to emphasize a point which seems to me to provide general background for particular considerations. The system of training of a biologist must reflect, as it did in the nineteenth century, the generally common knowledge and concepts of the biological community as a whole. This community is now a very different one from that of the nineteenth century, or the first half of the twentieth. Its focal concern is not with *kinds* of organisms, but with the *nature* of living systems. It concentrates less on the peculiar properties which identify kind, and more on the general properties which identify livingness. It takes living systems apart and puts them together again—at all levels—in order to understand the controls which bind components, and their relation to the mechanisms of hierarchical organization. As a population the biological community is multi-leveled and engaged in a multi-leveled task. The community will be more effective and more cohesive as a larger proportion of its members share a common multi-leveled training.

If this is so, and I have no illusions that there is general agreement that it is, then it is not enough to break the archaic mold of training within taxonomic compartments, a feat which, itself, yet remains to be accomplished in many institutions. We must also resist the inclination toward training limited to particular levels, which is accepted in some instances as the convenient enlightened alternative. A molecular biologist is no better equipped than the traditional taxonomically defined biologist to formulate powerful, trans-level concepts, or to appreciate the need for them. Curricula devoted exclusively to molecular biology proudly bear the label of modernity, but the emblem is a shrunken and distorted

ments to be described shortly and partly from considerations of the symmetry of the tellurium crystal. The lower curve in the illustration corresponds to the energies possessed by the outer or valence electrons (there

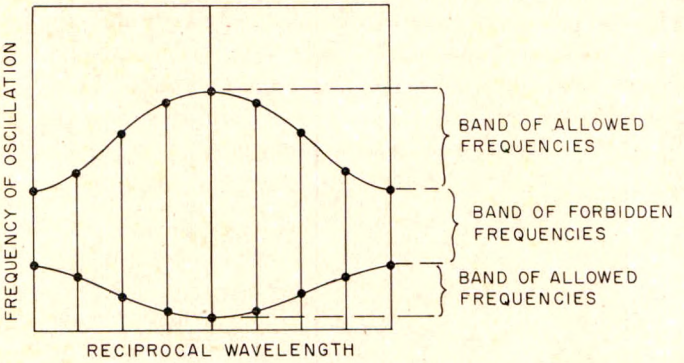


FIG. 5. The relation of frequency to the reciprocal of the wavelength for the oscillating masses. The dots on the curve show the allowed values of the wavelengths as determined from examples like those of Figure 4.

are six in Column VI elements), the middle (unshaded) band corresponds to the additional amount of energy needed to detach these electrons from their parent atoms, and the upper band then represents those electrons which are free to conduct electricity. These facts then explain the names

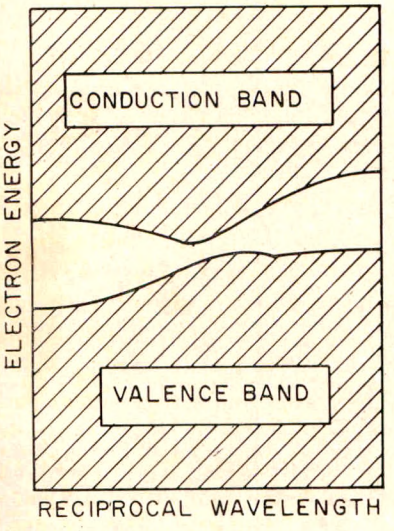


FIG. 6. The relation of the electron energy in a tellurium crystal to the associated reciprocal wavelength. The lower permitted band corresponds to energies of the outer, or valence, electrons, and the middle (unshaded) band is forbidden to electrons in tellurium. The upper band then represents electrons which have received sufficient energy to be detached from their parent atom and are free to conduct electricity.

given to the bands. We have not shown the discrete allowed values of the reciprocal wavelength as we did for the ring of masses. In a real crystal the corresponding energies are spaced so closely that they are virtually a continuum.

caricature of the full range of biological phenomena. On the other hand, curriculum-design dominated by fear that safe traditional moorings may be swept away by the molecular maelstrom—curricula which include nothing which peeks beyond the resolution of the optical microscope—come no closer to reflecting the reality of modern biology. The fascination, power and future of biology lie precisely in its range from molecule to community, specifically in its appeal to minds intrigued by the intricacy of multi-causal and multi-leveled analysis—whether in the complexity of macromolecular aggregation and packing, or of matching temporal and morphological pattern in a complex biotic community. Only wide-range, multi-leveled training can cultivate this fascination. The core of biological knowledge must be so organized and presented as to drive home the lesson of the past decade—that the combined approaches of several levels are required to crack some of the knottiest biological problems. Not least among these, I believe, is the epigenetic emergence of new properties in the course of development.

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Some of the experimental parameters which we have measured on tellurium have been the *resistivity*, the *Hall coefficient*, the *magnetoresistivity*, and the *optical absorption coefficient*. The resistivity of a material is a measure of its ability to carry electric current, and this is dependent on such inherent properties as its crystal structure and electron shell configuration, on the nature and amount of lattice defects, and on the environment. Temperature, in particular, plays a large role in determining resistivity, and we have, therefore, measured this property over the range from room temperature down to liquid air temperatures (89°K) as well as at the boiling point of liquid helium (4.2°K). It is known that the

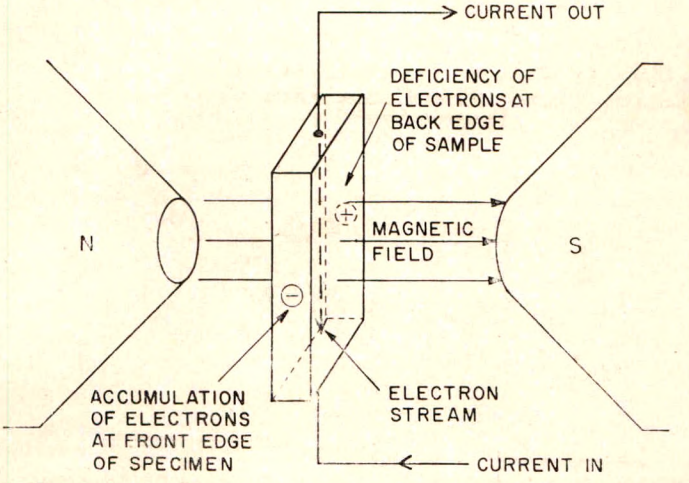


FIG. 7. The experimental arrangement for the study of the Hall effect. A rectangular sample is obtained by cleaving the crystal after it has been dipped in liquid air. A current then passes through it vertically and the deflection of the electrons causes a voltage to appear across the front and back edges.

atoms of any crystal are vibrating in a fashion similar to that of the ring of masses (Fig. 4) and the electrons are scattered from their normal paths by collisions with these vibrating atoms. If we wish to reduce the effects of this scattering and study properties of tellurium more directly connected with the energy bands, we cool the crystal to slow down the vibrations.

The Hall coefficient can be explained by Figure 7. A rectangular piece of tellurium is placed between the poles of a large magnet as shown. The magnetic field deflects the electrons to the edge of the sample in the same way that a compass needle is deflected by a current-carrying wire, as in Oersted's classic experiment. The accumulation of electrons leads to a negative charge at one edge and a deficiency of negative charge at the other edge (a consequence of the condition that electric charge cannot be created or destroyed), producing a voltage called the Hall voltage (after its discoverer). Using the reasonable assumptions that

PLASMA PHYSICS*

By SANBORN C. BROWN

PHYSICISTS define a plasma as a neutral collection of electrons and positive ions, the properties of which will become clearer as we discuss what we know about this state of matter. Unfortunately, its name is not unique in scientific nomenclature. In fact, the field itself has gone under various names since it was first recognized as a discipline of physics. The early and usually fatal experiments with lightning, attempts to study the aurora, or St. Elmo's fire, or even the mechanism involved in producing sparks from stroking the family cat did not contribute much to a consistent body of knowledge. It was not until the technique of obtaining a reasonably good vacuum had developed and current electricity was on a firm footing that men like Crookes, Hittorf, Thomeson, and Townsend could make real progress studying the passage of electricity through gases. For a while this subject went under the name of the physics of gas discharges, and after the last war it was commonly termed gaseous electronics. With the recent emphasis on searching for a way of controlling the catastrophic thermonuclear reaction typified by the hydrogen bomb, the term plasma physics has come to the fore. The name was coined early in 1920 by Irving Langmuir when he introduced the term plasma to denote that part of a gas discharge in which equal numbers of electrons and positive ions preserved charge neutrality, in which the ionized gas was completely passive as far as the mechanism of the discharge was concerned, and in which the visible glow seemed to mold itself to the tube shape or to any probes or electrodes which were introduced into it.

In recent years a great deal of information about the plasma state has been gathered by using high-frequency radio waves as a probe. By measuring the complex admittance of the plasma when it interacts with an ultra-high-frequency electric field, we can measure both the electron density and the collision frequency of the electrons with other particles. These two physical parameters give us a great deal of information about the behavior of the plasma state. Lorenz, early in the 1900's, predicted that if an electromagnetic wave fell on a body, the transition between the body being transparent to the radiation and being a reflector of radiation would occur at what is called the plasma frequency given by the equation

$$\omega_p^2 = ne^2/m\epsilon_0. \tag{1}$$

His calculations of this effect have been amply borne out by experiment. It should be obvious that the same mechanism which prevents radiation from emerging from the plasma also prevents radiation from the outside from penetrating into it. The existence of this critical plasma frequency

* A Sigma Xi-RESA National Lecture, 1960–61.

each impurity atom in the crystal contributes one electron to the conduction process, and that, at liquid air temperature (89°K), virtually none of these conduction electrons come from the tellurium atoms, the Hall voltage provides an extremely sensitive way of determining the impurity content of the crystal.

The transverse current associated with the Hall effect leads to the establishment of a secondary magnetic field in the sample, and this field produces a force which is opposed to the normal motion of the electron current, thus causing the magnetoresistance we have already mentioned. This effect is intimately related to the energy bands in the following fash-

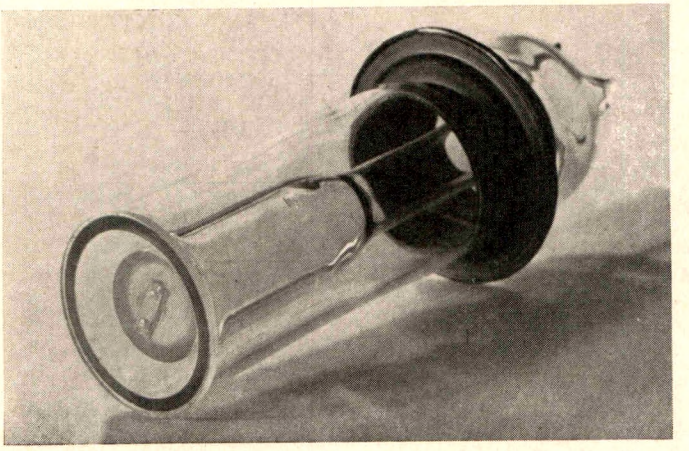


FIG. 8. A tellurium infrared detector. The container is actually a small vacuum, or Dewar flask and liquid air is used to cool the sensitive element. At low temperatures, the electrical noise due to random fluctuations of the current is reduced and the cell is more sensitive. The round window on the end is a one-half inch disc of synthetic sapphire, since glass will not transmit infrared radiation. The tellurium element is directly behind the sapphire window and connections are made to it by evaporating gold on the ends.

ion. Turning back to the band picture of tellurium (Fig. 6), we see that there is a maximum in the curve of energy *vs.* reciprocal wavelength in the valence band and a minimum in the conduction band curve. Since a tellurium crystal is a three-dimensional entity, we realize that these curves are traces in a single plane of complicated energy surfaces. Considering just the region in the vicinity of the two extrema, these surfaces will be approximately cigar-shaped—that is, they will look like the ends of ellipsoids. In view of the fact that tellurium has a threefold and three twofold axes of symmetry, it further follows that there will be six of these ellipsoids for each of the energy extrema, and the theory for such a situation, as worked out by Drabble and Wolfe at the General Electric Company Laboratories in England, shows that, for a few of the

has very important implications both in using microwaves to probe the physical processes occurring in high-density plasmas and in radio communication through a plasma.

Until a very few years ago, most of our information about the plasma state, whether it was by studying the glowing gases in a discharge tube or the structure of the furthest nebula, came from analyzing the visible light and, where possible, cataloging its optical spectrum. Such information comes from transitions of electrons in bound states of the atoms or from free electrons leaving or entering bound states of atoms or ions.

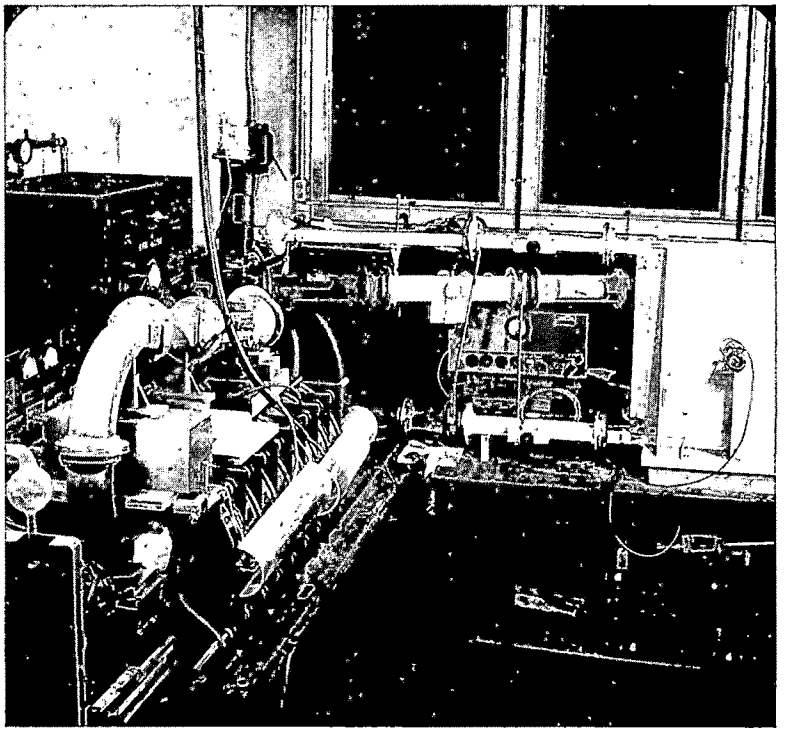


FIG. 1. A typical laboratory arrangement for studying the microwave radiation from plasmas.

As we have seen, the plasma is a neutral collection of electrons and ions, and thus optical spectroscopy does not tell us of the state of the plasma itself; in fact, a completely ionized plasma might emit no light whatever. The electrons and ions in a plasma are in constant motion, and this means that the particles are continually accelerated and decelerated, and in conserving energy the electron radiates. These so-called free-free collisions emit radiation in the radiofrequency range of the electro-magnetic spectrum, and the technological development of high-frequency detectors, brought to a high degree of engineering perfection by the advances

possible arrangements of electric current, magnetic field, and symmetry axes, the magnetoresistance effect should vanish. We have found this to be the case in our liquid helium measurements [5].

The uses of tellurium have not been of any great economic significance. One interesting application has been based on its chemical similarity to sulfur, the traditional vulcanizing agent. The addition of 2% or less of tellurium to rubber compounds improves the resiliency and abrasion resistance. An aesthetic use is in the production of colored glass, the size of the colloidal particles determining whether the result is red, blue, or brown.

The most recent application of these crystals is in the detection of infrared radiation. The energy required to detach an electron from a tellurium atom, corresponding roughly to the separation of the two bands in Figure 6, is supplied by light whose wavelength lies in the intermediate infrared region, at approximately 3.6 microns. It is a fortunate coincidence that this property of tellurium fits in a "window" in the atmosphere where both the brightness of the daytime sky and molecular absorption due to carbon dioxide and water vapor are a minimum. A sketch of a cell using single crystal tellurium designed by C. D. Butter and L. D. McGlauchlin [6] is shown in Figure 8 and its successful operation depended in part on the consequences of some of the basic research described above. It was found that cells made with crystals mounted directly on a supporting block were not very sensitive, and this is caused by a difference in the coefficients of expansion of the two materials. Tellurium, as we have already noted, is extremely sensitive to mechanical handling. The solution to this difficulty is to mount the detector crystal on a second, inactive element.

Although the development of the tellurium infrared detector has been a useful by-product of this basic research, the motivation for studying this complicated crystal has been to gain a better understanding of semiconductors. After silicon and germanium, it is now the easiest of the semiconducting elements to obtain in the form of high-perfection and high-purity single crystals. Our present state of understanding tellurium is something like the situation which existed for the Column IV elements at the start of the transistor era, and we are beginning to form a picture of the interaction between the properties and the structure.

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of radar during the last war, has opened for us whole new orders of magnitude in our appreciation of the plasma state of matter.

A typical experimental arrangement for studying the microwave radiation from a plasma is shown in Figure 1. The tube containing the plasma itself is mounted on the axis of a solenoid so that the discharge may be studied both with and without a magnetic field present. Surrounding the

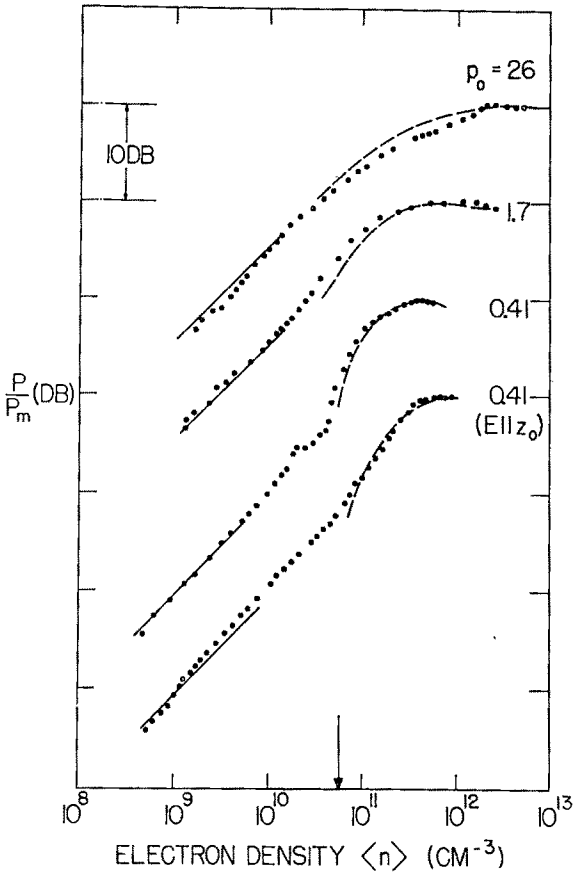


FIG. 2. Ten centimeter radiation from a helium glow discharge as a function of electron density.

plasma is a microwave waveguide shown as the rectangular tube coming from the solenoid and guiding the microwave energy to a detector on the right-hand side of the figure. Data taken with this experimental setup are illustrated in Figure 2. This figure [1] plots the radiation intensity as a function of electron density coming from a positive column of a D.C. glow discharge in helium at various pressures. The measurements were made at a frequency of 3000 megacycles per second. When the electron density is low, the radiation intensity increases proportionately

MIND, DRUGS, AND BEHAVIOR

By PATRICK L. MC GEER

THE MANNER in which the brain gives rise to phenomena associated with the mind is a problem which has fascinated scientists down through the ages. This mind-brain conundrum presents different aspects to different people. Most would now agree that the brain is the substrate organ of the mind, and that all of the functions associated with the mind must, in some way, be mediated through the several billion nerve cells of the brain. The evidence in favor of this point of view is too overwhelming to merit discussion. But, from this fundament, opinions diverge sharply. The physiologically oriented hold that when the brain is better understood most functions associated with the mind will be easily explained in terms of an operating electrochemical machine; the philosophically oriented hold that such a point of view is untenable inasmuch as mind is abstract whereas brain is physical. To most philosophers, even the most detailed understanding of the electrophysiological events of the brain would not permit insight into the mind: mind is of a different, non-physical construct, and physiological measurements of any kind are inappropriate in establishing relationships between brain and mind. This philosophic point of view was well expressed in 1951 by Lord Samuel:

"Watch a chess player cogitating for half an hour whether to move his queen here or his pawn there; at last, stretching out his hand and doing the one or the other. The physiologist may describe the nervous and muscular mechanism which operates the movement, but not the process which has decided the choice. The chess player is evidently of a different order from the chessboard and pieces; in him there is activity going on which is not material; in them there is not. Intellectual creativity, the power of choice, are of a different order from chemical and electrical reactions." [1].

It was the briefest time after this was written that computing machines were programmed to play a tolerable game of chess. Moreover, such machines were being taught to play checkers so that they improved with practice. They learned not to repeat errors and eventually were able to beat their human opponent. Newell has recently shown that computers can be programmed to solve problems in a manner similar to that used by students [2].

To press the obvious analogy between the programming of the mind as a human learns to play chess or checkers, and the programming of a computing machine, would be unjustified. One essential difference is that the human is motivated—he wants to play the game, and, usually, he wants to win. His drive comes from within. Computers to date have not been equipped with drives. They endlessly go through predetermined motions.

Motivations, both desirable and undesirable, are a property of mind

with density, showing that the self-absorption is negligible and the radiation comes from collisions between electrons with atoms. As the absorption increases, the radiation intensity becomes independent of the electron density. This corresponds to a condition of thermal equilibrium of the electrons under conditions in which the plasma radiates as a black body. The observed fact that further increase in electron density does not increase the radiation intensity leads us to suspect that radiation produced in the volume of the plasma is being highly absorbed in the body of the plasma, a fact which is characteristic of this type of radiation,

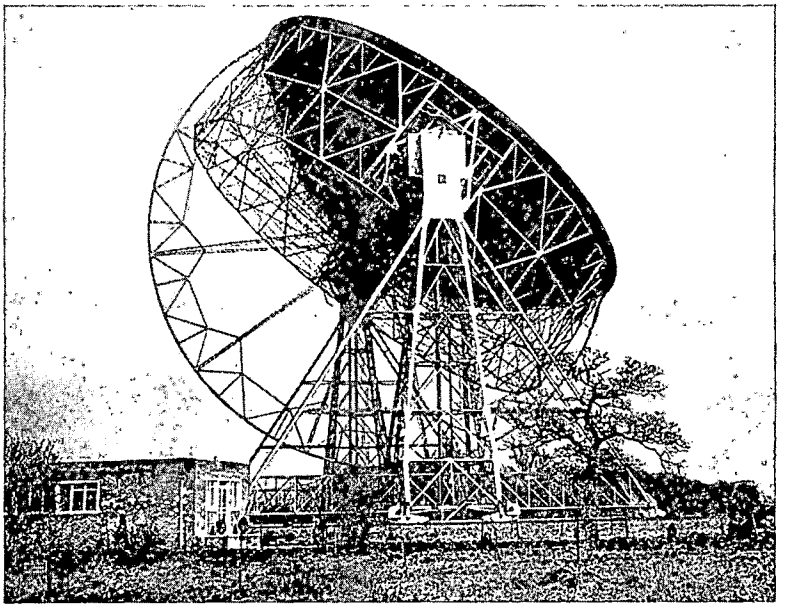


FIG. 3. The radio telescope at Jodrell Bank, Manchester, England.

and leads to the conclusion that the microwave radiation under such conditions comes only from the surface of the plasma and not from the body of the plasma.

Although the plasma state does not occur naturally at the high pressure of our earth-bound atmosphere, ultraviolet light from the sun ionizes the outer region of our atmosphere and encompasses us in a shell of plasma which is known generally as the ionosphere. The usefulness of long-wave radio communication is greatly enhanced by using a frequency lower than the plasma frequency for the naturally occurring electron density in the ionosphere, and thus bouncing our long-wave radio carrier waves off this ionized shell to avoid losing the electromagnetic radiation due to the curvature of the earth. The ionosphere, however, does form a blanket through which we must pass if we are to communicate with outer space. Normally, our microwave generators

and thus may share common parentage with emotional feelings, creative thought, and decision making. The fact that these properties, and others which we associate with mind, are abstract, does not necessarily mean that they cannot be associated with particular cells in the brain. It may be too soon to accept the philosopher's anti-somatic view of mind, just as it may be too pragmatic to accept the physiologist's corporeal one. Too little is at present known of the brain to justify a strong position in regard to this mind-brain problem. Newton did not derive equations to explain the interrelated motion of the planets before others had identified some of the planets and studied their relative courses. Mind-brain scientists are still groping in the confusion of a pre-Copernican sky.

The exciting discoveries made in the last decade, however, encourage the belief that, as greater knowledge accumulates, what now may seem an abstract property of mind may take on a distinctly physical connotation as far as brain is concerned. The discoveries lie in the fields of neurophysiology, neuropharmacology, and neurochemistry, and, although they may give little insight into how creative thinking and decision making occur in the brain, they do give an inkling of how the motivations for such things are provided, and how they may be modified.

Neurophysiological Investigations

Classical neurophysiology was largely concerned with investigating functions associated with the cerebral cortex, partly, no doubt, because the cortex is what confronts one first upon opening the skull. Any doubts regarding its top priority for investigation were dispelled by the knowledge that this is the part of the brain best developed in humans.

It became clear from studying the cerebral cortex that definite functions could be correlated with particular groups of cells. Thus, cells were found that controlled movement and speech; others that subserved sight and hearing, and so on. The significance of these largely mechanical functions was easy to grasp. Cells on the cortical surface were not located, at first, which could be associated with properties of mind, although there were large numbers of cells in the frontal lobes which were thought to be "silent."

The advent of stereotaxic equipment opened the door to investigating the functions of deeper, subcortical cells. Subcortical cells were long suspected of harboring "primitive functions" including emotion and motivational drives.

A spectacular breakthrough came in 1953. Olds and Milner were investigating subcortical centers in rats in the hope of finding areas which, upon stimulation, would provoke the rat to perform some act to avoid the stimulus. To their surprise, they found that, in certain brain areas, the rats seemed to enjoy the stimulation. To confirm this impression, they placed a rat in a box with a pedal which the rat could

used, for example, in radar detection and control of satellites and high-flying missiles, operate at a high enough frequency to exceed the critical plasma frequency of this plasma blanket. It should be obvious, however, that we have within our capabilities the ability to increase the plasma density with high-altitude hydrogen bombs, for example, to block out all communication with and radar detection of high-flying satellites and missiles so that we can neither detect nor control them by microwave signals. This lends an urgency to these types of studies which is very evident to our military research planners.

Turning from our laboratory experiments to naturally occurring plasma sources of radio emission, we find it necessary to use radio tele-

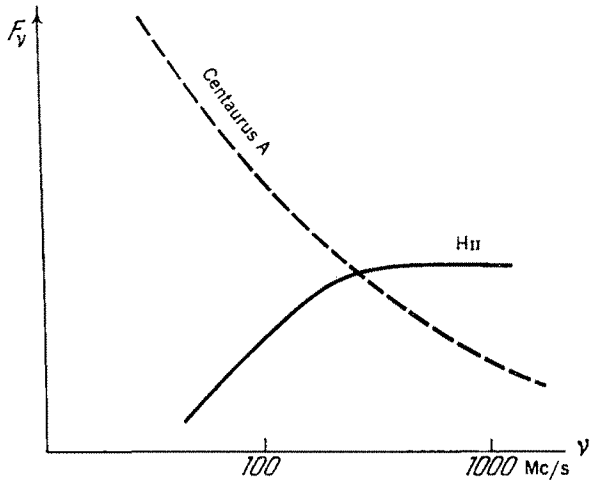


FIG. 4. Radio spectra of two quiet stellar sources.

scopes such as that shown in Figure 3. This illustrates the radio telescope at Jodrell Bank in Manchester, England. The size of these installations is tremendous; the Jodrell Bank one has the impressive parabolic antenna diameter of 250 feet. Figure 4 shows the spectral distribution from two stellar sources [2]. The lower curve is the equilibrium spectrum of a volume of well-behaved interstellar space. This curve is equivalent to that shown in Figure 2 except here we plot the intensity as a function of frequency whereas in Figure 2 the intensity was plotted as a function of density. Thus, here the spectral distribution is flat in the transparent region and decreases in intensity at lower frequencies as we would expect when the plasma becomes black-body below the critical plasma frequency. The radiation from Centaurus A cannot be explained in so simple a fashion. In fact, here we see that the radiation intensity decreases with increasing frequency throughout its region of observation, having quite a different dependence from the equilibrium case. This suggests that

press each time he wished to deliver a brief pulse of current to his brain. With this arrangement, and with electrodes placed in appropriate areas, the rat would stay at the pedal continuously for hours, stimulating his brain thousands of times. As Olds describes a typical rat he had prepared:

"He begins to search and pursue eagerly after his very first stimulation. . . . Sometimes while the animal is self-stimulating, the circuit is cut off by the experimenter, so that stepping on the pedal will not produce any brain shock; in this case an animal that is self-stimulating will give a series of forceful ('frustrated-looking') responses and turn away finally from the pedal to groom himself for sleep; but he will go back from time to time and press the pedal (as though to make sure he is not missing anything)."

The term "reward center" was devised to describe those areas of the brain where such self-stimulation could be elicited.

The avoidance effect, which had been originally sought, was also eventually found. In such brain areas, termed "punishment centers," the rat would never self-stimulate but would work to avoid getting stimulated.

With his simple experimental arrangement, Olds was able to map the rat brain for reward and punishment centers and to establish a hierarchy among the reward areas, based on how seriously the animal would seek such stimulation. He concluded that about 35% of the cells of the rat brain lay in reward centers, about 5% in punishment centers and approximately 60% were neutral. Neocortical areas, i.e., the "newest" part of the brain developmentally, made up the majority of the neutral areas. The paleocortex, or ancient cortex, showed rates of approximately 200 stimulations per hour. This was increased up to 3000 per hour in certain subcortical nuclei, and, in "lower" regions yet, even higher rates were found. Thus, rats would self-stimulate up to 5000 times per hour in the hypothalamus and up to 7000 times per hour in certain areas of the brain stem. The sensory inflow to the brain and motor outflow from the brain pass through the relatively narrow brain stem, and, as one proceeds "lower" from the cortex toward the brain stem, there is usually a concentration of an effect. This principle seems to apply with regard to "pleasure" and "punishment."

Neither self-stimulation, nor avoidance of stimulation by itself, gives insight into the nature of the sensation being experienced by the animal. Different experiments were devised to provide such information. Rats were taught to run a maze for the privilege of self-stimulation. At times they would run faster for electrical stimulation than for food. Furthermore, with electrodes in certain hypothalamic areas, the rats would run faster for an electrical stimulus even when they were starved. Given the choice between food or electrical stimulation at the end of the maze, the rats would choose electrical stimulation.

In other experiments, a rat was obliged to run across a grid which

Centaurus A is a non-equilibrium radio source, and we must look further for an explanation of a behavior of this sort.

It was Michael Faraday and Hans Christian Oersted in the middle of the 19th century who firmly established the fact that a moving charge in a magnetic field experiences a force on it due to the magnetic field. Obviously a force acting on a charged particle accelerates it and it moves in circular motion. The result of this interaction is radiation, some of which is found in the radio or microwave region of the spectrum. We find radio noise surrounding us everywhere even when the electron makes

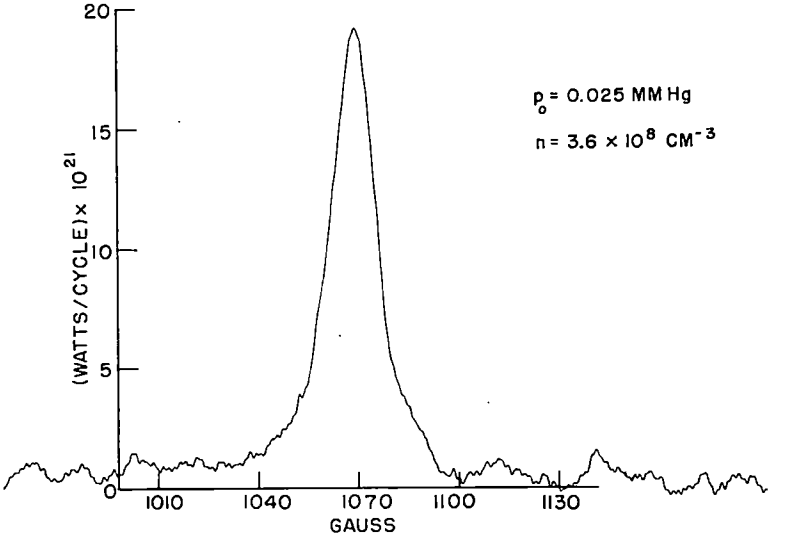


FIG. 5. A cyclotron radiation line superimposed on the bremsstrahlung continuum.

no collision, since not only is the earth itself a magnet, but lines of magnetic flux are to be found throughout interstellar and intergalactic space.

On earth we have a much more practical problem. A great deal of our scientific effort at the present time is spent in searching for a way of harnessing the power of thermonuclear fusion. One of the main problems in controlling a thermonuclear reaction is to prevent the loss of the hydrogen plasma which can support the reaction. When we are dealing with temperatures of many millions of degrees, we must confine the working substance to prevent its coming into contact with any material walls, since they would be instantly vaporized. A magnetic field can obviously confine the charged particles of a plasma and there is no proposed controlled thermonuclear device which does not superimpose on the plasma, strong magnetic fields for containment purposes. The by-product of this is that these machines become sources of radio noise which vie with the radio stars in their interest and complexity.

gave him successively more unpleasant shocks in order to reach the pedal for stimulation. The shock delivered by the grid could be raised to the point where the rat would forego the pleasure of brain stimulation and no longer cross the box. But the animal would accept roughly twice as powerful electrical shocks for self-stimulation as he would for food even if he had been starved [3]. The implication, then, is that electrical stimulation in these areas satisfied the equivalent of hunger centers in the brain, and that the electrical stimulation was more pleasant than the sensation which the brain received following food intake.

Rats in another series were castrated after they had been taught to self-stimulate. In animals with electrodes correctly placed, the rate of stimulation dropped after castration, but increased when sex hormone was given to the rat. In these areas, which are distinct from the "hunger centers," it seemed as though the stimulation was providing sexual satisfaction.

To determine if stimulation of some cells always produced pleasure and other cells always punishment, Olds placed electrodes in each rat of another series both in a presumed pleasure center and a presumed punishment center. Each rat was given various opportunities to self-stimulate; sometimes in the pleasure area, sometimes in the punishment area. The rat was also subjected to brain stimulation gratis on other occasions; sometimes to the pleasure area, sometimes to the punishment area. The rat always had the opportunity of terminating the free stimulation by pressing a pedal. In areas of powerful reward, rats would always self-stimulate but would never interrupt a free stimulation. In areas of strong punishment, the rat would never self-stimulate but would always interrupt a free stimulation. Some equivocal areas were found where both self-stimulation and interruption of free stimulation occurred. Stimulation of specific cells in these areas might be pleasant or unpleasant according to the circumstances, or it might be that the stimulation excited a pool of cells some of which exclusively represented reward and others exclusively represented punishment [4].

A further innovation was introduced by Dr. Larry Stein [5]. He arranged that rats could stimulate their brains in reward centers, but each successive stimulation produced a smaller shock until, finally, the shock was worthless for reward. However, the rat could push another lever to reset the stimulation strength to its initial value. With such an arrangement, a given rat would always self-stimulate until the strength of the stimulation diminished to a fixed point; then he would leave the self-stimulation pedal to push the reset pedal. Such consistency made it possible to test the effects of various drugs upon the strength of stimulation required for reward.

Following Olds' discovery of self-stimulation centers in the brain of rats, it was not long before comparable studies were being undertaken

When a plasma is acted on by an external dc magnetic field, anisotropic radiation mechanisms come into play. The most important of these is cyclotron emission. In a uniform magnetic field an electron travels in a circular path around a magnetic line of force. If an alternating electric field is applied to the electron, it can come into cyclotron resonance with a given strength of magnetic field where the resonant frequency is given by

$$\omega_b = eB/m. \tag{2}$$

Conversely, if an electron is traveling in an orbit around a line of force, it will emit radiation at a frequency ω_b since circular motion implies accelerated motion. Figure 5 shows a typical cyclotron line measured [3] in the apparatus of Figure 1. Here we see the cyclotron line superimposed on the bremsstrahlung continuum from a tenuous helium discharge of a few volts energy. Whenever a plasma is produced in a magnetic field, cyclotron radiation results, and in the ordinary laboratory scale of magnetic fields and plasma densities, the cyclotron radiation can be easily measured.

As we have mentioned, magnetic field confinement is an essential characteristic in all thermonuclear machines so far designed. These machines operate in density regions far exceeding those in simple laboratory experiments, but when the electron density is increased, as illustrated before, the plasma fails to remain optically thin even to the cyclotron radiation. What happens is that the line widens out until it disappears into the black-body continuum, and the radiation is trapped providing an additional mechanism for energy mixing within the plasma. Because of this internal trapping of the cyclotron radiation, the energy lost by this mechanism was for many years considered negligible. In 1958, however, two Russian physicists, Trubnikov and Kudryavtsev, pointed out [4] a very fundamental physical flaw in all existing attempts to control thermonuclear fusion. In ordinary plasmas, equation (2) shows us that the frequency of the cyclotron line is linearly proportional to the magnetic field. However, in plasmas where the temperature is interesting from a thermonuclear point of view, the electrons move so fast that their mass increases due to the effect of relativity. This increase in mass results in equation (2) becoming more complicated. The resulting relativistic calculation shows that, instead of the radiation being contained in a single fundamental line, it generates harmonics, the magnitude of which can be extremely large. Even from mildly relativistic electrons most of the energy can reside in the higher harmonics of the electron orbital frequency. For example, at 25 kev 50 per cent of the radio noise resides at higher harmonics, and at 50 kev about 90 per cent. An illustration of such a calculation [5] is shown in Figure 6 where we plot the ratio of the radiated intensity to that of a black body as a function of the ratio of the

in animals with more advanced brains. Cats, monkeys, dolphins and humans were all studied. The dolphin might seem a curious choice, but it has a large brain, with particularly well developed cortical convolutions. Some scientists feel that the brain of the dolphin may hold as great potentialities as does the human brain [6].

Much of the early work in monkeys was done by Dr. John Lilly. The same general principles, established by Olds and other workers for animals with simpler brains, were found to apply. With the monkey, many electrodes could be implanted in the brain at once. This made it possible to give the monkey a choice between stimulation of different areas, and thereby to establish, in another way from that done in rats, the relative desirability of stimulating any given brain area. The hierarchy established was similar to that found in the rat, with the most powerful zones being found in the brain stem, and progressively less powerful ones being found as "higher" centers were approached. Once brain stimulation was started, it proved difficult to divert the monkey's attention to anything else. Lilly introduced the term "capture" to describe this attitude.

Stimulation of punishment zones produced obvious fright, dilated pupils, pilo-erection, shaking of the body, violent escape attempts, biting and tearing of objects placed in the vicinity even to the extent of breaking teeth out of the jaw. The monkey could be "captured" in such a punishment zone so that his attention was entirely concentrated on performing whatever operation was necessary to avoid the stimulation. According to Lilly, if the monkey is "captured" in a punishment zone for about three hours, there are deleterious effects which may persist for as long as two days afterwards. He becomes irritable, eats poorly and develops a gray and pallid expression. This whole picture can be reversed by stimulating him in a reward center. If this is done promptly, the animal revives in a few minutes.

Are humans different from lower animals? Opportunities for making observations in conscious humans during subcortical electrical stimulation have occurred with patients suffering from epilepsy, brain tumors, and mental illness. The pattern previously described for animals has so far been generally corroborated. Reward centers have been found in the human associated with intense, but non-specific feelings of well-being, with pleasant sensations ascribed to distinct parts of the body, and with sexual arousal. Punishment areas have also been found, the stimulation of which can elicit terror, anger or pain [7].

Reactions observed in humans are, however, far more complex than in lower animals. There is always an engendered association between the sensation produced by electrical stimulation and the people surrounding the patient at that time. Interpretation of the sensation is also colored by the particular person's past events and future expectations. Such

actual frequency to the cyclotron frequency. This is the spectrum of cyclotron emission from a plasma slab containing 10^{14} elec/cc at 50 kev for a typical magnetic field used in a thermonuclear machine. The upper curve is the total emission after summing over all the harmonics.

Of course, self-absorption of the radiation prevents the total emitted

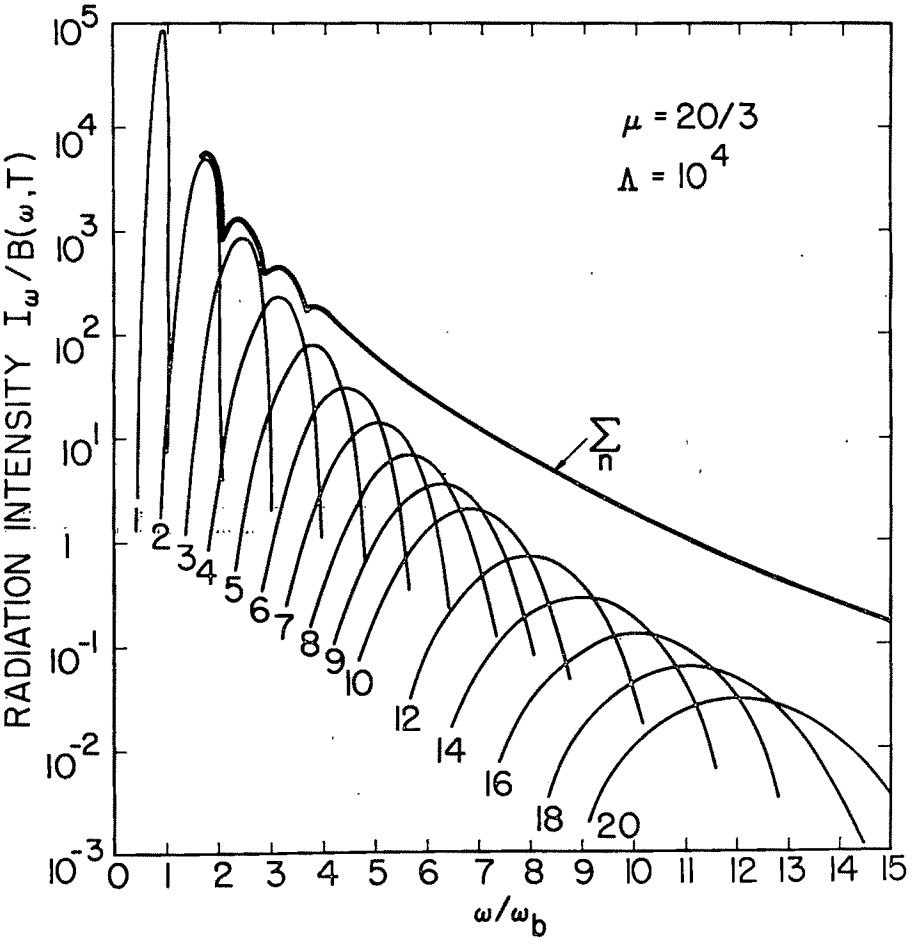


FIG. 6. Harmonics of the cyclotron line resulting from relativistic change in mass of the electron.

intensity from ever exceeding the black-body limit, and all the harmonics up to the 11th are trapped in the black-body continuum of their own making. However, the black-body limit occurs at about three times the plasma frequency in the absence of a magnetic field, which means that 27 times more power is being radiated in the form of black-body energy than would be in the absence of the magnetic field and the relativistic

alloyage presumably reflects the more complicated cerebral cortex of humans which permits far richer feedback to subcortical areas. The contrast between the human brain and that of lower animals thus may lie not so much in the nature of the sensation experienced in these pleasure and punishment centers, as it does in the complicated associations permitted by the cortex.

In every species studied, the specific areas of the brain involved in pleasure and punishment seem to be the same. They include certain areas of the paleocortex, basal ganglia, diencephalon, and upper brain stem.

The implication of all of these studies is that much learning and decision making, which presumably take place chiefly in the cortex, are directed toward stimulating pleasure centers, and away from stimulating punishment centers. In other words, the cells which are "neutral" for reward or punishment are marshalled to direct the animals' behavior in such a way as to provide a sensory input which is "just right" for pleasure and punishment cells. That many of the functions associated with pleasure and punishment should have survival value for self and species is, no doubt, of more than casual consequence to our own presence.

There are at least two further recent advances in neurophysiology which add significantly to our growing concepts of mind-brain relationships. The first of these is the discovery by Magoun and co-workers of the reticular activating system. The reticular activating system of neurons spreads through the upper part of the brain stem and lies, therefore, just below the most powerful reward and punishment centers. This system seems to have an influence on almost all sensory inflow to the brain as well as on the motor outflow from the brain. In particular, the activity of these cells seems to govern the level of consciousness. Irreversible damage to the reticular activating system results in a state of permanent coma. The brain waves in this circumstance constantly show a pattern of sleep. Such human "vegetables" are an occasional consequence of head and neck injuries in motor accidents.

Although an adequately functioning reticular activating system is necessary for consciousness, it does not seem to be responsible for focusing attention. A more rostrally located system in the diencephalon, termed the "diffuse thalamic system," has been generally regarded as responsible for selecting portions of the sensory inflow on which the brain concentrates; we are thus most conscious of what this system chooses. We are less conscious of more extraneous input and, of course, much is diverted into that reservoir for the psychoanalysts, the subconscious. Thus, normal function of both the "reticular activating" and the "diffuse thalamic" systems would be necessary for the pleasure and punishment centers to assert their influence [8].

Prior to these developments, W. R. Hess had largely completed his

effect. To see if a thermonuclear machine can operate with a net gain, one must at least be able to balance the sum of this black-body loss from the surface and the total volume loss by bremsstrahlung against the volume production of nuclear energy. The machines so far designed for controlling the thermonuclear power operate on the nuclear reaction of two deuterons coming together with sufficient energy so that they stick together. The resulting helium nucleus has less mass than the two initial deuterons, and this mass converted into energy is spectacularly demonstrated by the destructive power of the hydrogen bomb. To control this in a practical device is the object of a great deal of research and development. Obviously one can increase the controlled energy coming from the

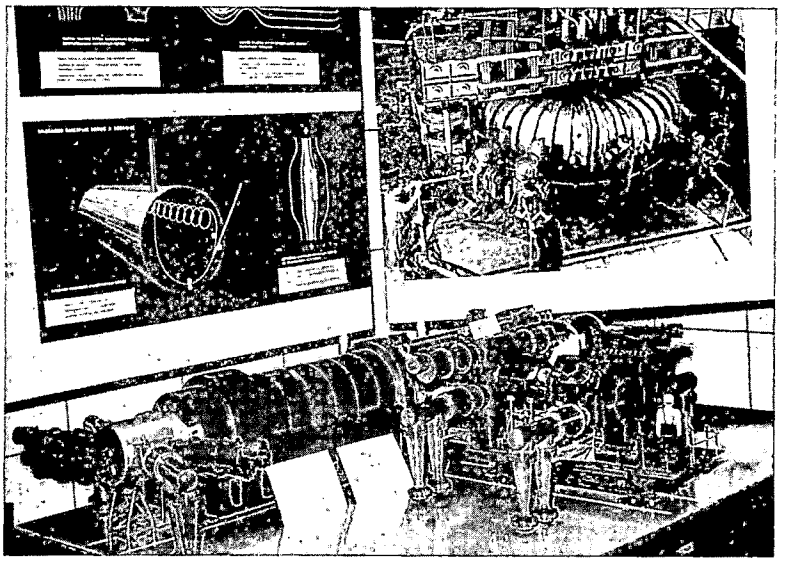


FIG. 7. Two of the Russian thermonuclear machines.

d-d reaction by increasing the volume of the heavy hydrogen available to the reaction. Since the nuclear reaction is a volume effect and the black-body loss is a surface effect, the volume-to-surface ratio becomes important. Unfortunately, the calculations of the harmonic generation of Trubnikov and Kudryavtsev dictate a minimum plasma thickness of many meters to balance the radiation loss with the d-d reaction. This outrageously large size of reactor becomes indeed very discouraging for those who search for practical devices for controlling the energy of the hydrogen bomb.

This conclusion also serves as an illustration of a good object lesson for those who guide the destinies of planned research. Huge installations at the cost of many millions of dollars, many millions of pounds, and many millions of rubles were built in an international race to lead the world in

Nobel prize-winning series of investigations on the diencephalon. He conceived this region of the brain as integrating two opposing systems: the ergotropic system and the trophotropic system. As the name implies, the ergotropic system prepares the body for action, increasing muscular tone and activating the psychological state. Such activity, of course, requires the participation of both the reticular activating and diffuse thalamic systems and, as the studies of Olds and others clearly indicate, may easily involve seeking pleasure or avoiding punishment. The trophotropic system integrates recuperative functions, culminating in satisfaction of various sorts, including drowsiness and sleep. Thus, the ergotropic and trophotropic systems are visualized as coordinating visceral, somatic, and psychological activity, with over-all integration taking place in the diencephalon [9]. The pattern of behavior reflects, in part, the balance between these two systems.

Most of the brain mass is taken up by the cerebral cortex (Table I) and much of this is comprised of "silent" areas. Creative thinking is presumed to go on here, and analogies have been drawn between the way the human brain tackles a deductive problem and the way a computing machine does. But analogies so far do not exist between a computing machine and the subcortical neurophysiological systems we have discussed. These systems occupy a much smaller area of brain, and, as we have seen, account for such activities as the focusing of attention, the drives to undertake a task and the pleasures or displeasures associated with various situations. (As one of my colleagues tells his students, one finds represented here the patterns of activity which are daily reported in the "society pages" of the press, namely, feeding, mating, and migration.)

Mental problems, in the psychiatric sense, are almost entirely associated with emotional aspects of mind. Mental patients, who occupy one half of all our hospital beds, include the brilliant as well as the stupid, and difficulties in handling problems in creative thinking are seldom listed in the usual tedious roll of supposed "causes" of mental illness. Symptoms shown by mental patients fit more closely with upsets in the subcortical systems; thus, new concepts such as reward and punishment centers, ergotropic and trophotropic zones, and reticular activating and diffuse thalamic systems have greatly interested psychologists and psychiatrists. They wish to know if the activity of these systems can be adjusted or modified in any way, and if practical benefits might accrue from such control. For this we will probably have to look to brain chemistry.

In the final analysis, the brain, like other organs of the body, is a chemical machine. It is a very special chemical machine, of course, because each unit does not duplicate the function of every other unit as in the alveoli of the lung or the glomeruli of the kidney. The difference

the peaceful use of thermonuclear fusion power without first laying a sound foundation of fundamental research in plasma physics. As our detailed knowledge of the plasma state has continued slowly to emerge, we find that none of the presently engineered thermonuclear machines will work because of some fundamentally wrong assumptions which had not been checked before the machines were designed. We are now learning much of the basic physics and many of us have confidence that eventually our knowledge of fundamentals will progress to a point where the physicist can turn over the basic concepts to the engineers with the con-

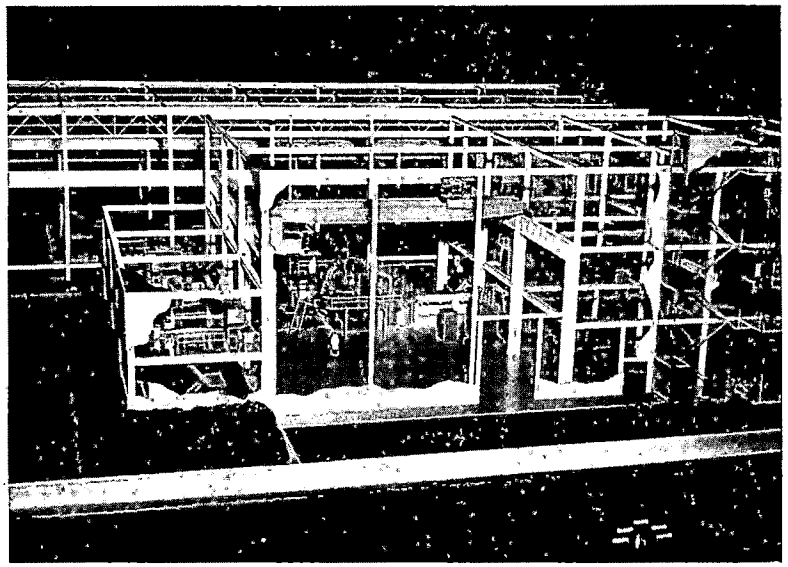


FIG. 8. Model of the Princeton C-Stellarator.

fidence that the behavior of the plasma is understood, and firmly based on this knowledge, practical devices can be designed and built. Such immense installations as the Russian Ogra and Alpha shown in Figure 7 or the Princeton Model C Stellarator, a model of which is shown in Figure 8, must await small-scale and much more mundane measurements in such details as the experimental verification of the Trubnikov theory before they can be made into practical generators of thermonuclear power. Although these are destined to fail in their present form as practical power generators, it is obvious that their construction is necessary in our search for the basic physical behavior of plasmas. We are studying the basic mechanisms in hot dense plasmas, and unless we have hot dense plasmas to study, progress will not be made. These big machines should be looked on not as nuclear power reactors but as laboratory sources of the plasma state.

Let us now turn our attention to some of the problems of non-thermal

between the cells of the brain, and those of many other organs of the body might be likened to that between the individual tubes of a television set and the individual floodlights in a bank at a football stadium. If one of the floodlights at the stadium ceased to function, a relatively minor loss would ensue; but if one of the tubes in the television set ceased to function, a whole series of deficits might follow depending on the exact location and connections of that particular tube. Although this would be expected to happen even if all the tubes in the television set were identical, there are typically many different types of tubes in a television set, each basically similar but with distinct characteristics depending on the specialized job it is expected to perform.

So it may be with cells of the brain. We know that all the neurons of the brain have basic similarities just as do the tubes in a television set. The same energy-producing metabolic cycles exist in all brain cells, and these differ little from the chemical cycles in cells of pigeon breast or any other mammalian tissue. But, as with tubes in a typical television set, important differences in detail may be present. Such differences are essential if cells of one functional system are to be affected in preference to cells of another.

Is there any positive evidence that such differences exist? Evidence is available in the selective action of many drugs with powerful effects on the central nervous system. Let us briefly consider some drugs which are thought to act primarily on the systems we have been discussing and which produce pronounced alterations in mental functioning.

Neuropharmacological Studies

Two broad classes of drugs have linked the study of mind with pharmacology: Those drugs which disturb the mental functioning of normal people and those drugs which improve the mental functioning of abnormal people. The hallucinogens make up the former class, and were so named, in a provocative paper by Hoffer and co-workers in 1954 [10], because of their pronounced tendency to induce various types of hallucinations. The tranquilizing agents and the psychic energizers constitute the majority of the second class. Tranquilizing agents have quite different effects from psychic energizers; which type of drug is used by the physician depends upon the symptoms he desires to alleviate.

Hallucinogenic drugs have been in use from time immemorial but, until recently, they were in the province of witch doctors and shamans rather than that of their more modern scientific counterparts. The accidental discovery of the hallucinogenic properties of a synthetic chemical, lysergic acid diethylamide (LSD), by Hofman in 1943 focused scientific attention on such agents. Many hallucinogens have since been discovered, some being synthetic and others natural products, but all having in common the ability to disturb mental processes and thereby

radiation. Observations of the radio-frequency noise from plasmas, both those produced in the laboratory and those of extraterrestrial origin, show the presence of two types: one is the steady radiation resulting from random emissions, absorptions and scatterings of the radiation which are in equilibrium with the radiating matter. Superimposed on this relatively well-behaved radiation are great bursts of noise somehow connected with various types of instabilities. One of the best plasma sources we have is the sun, and if we replace optical telescopes by radio

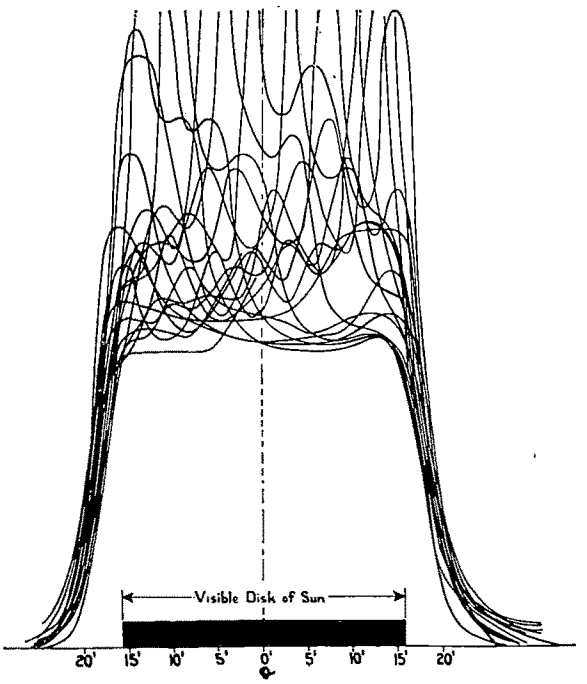


FIG. 9. Scans of the radio noise from the sun.

telescopes, we find a most interesting behavior. Figure 9 shows a number of identical scans with a radio telescope across the sun's disc, taken at consecutive times at a frequency of 1420 megacycles per second [6]. Peaks from individual bright areas are obvious as well as the lower envelope of the curve which delineates the contribution from equilibrium components of the noise. This behavior is by no means restricted to the sun, and the same kind of bursts superimposed on a quiet background has been observed from the Princeton stellarator. Great bursts and flashes of radiation are not a prerequisite for the existence of non-equilibrium emission processes, so that there is really no clear-cut distinction between equilibrium and non-equilibrium emission, and many steady components of radio noise are observed which do not fit any known equi-

to mimic, to a degree, certain aspects of mental illness.

As the name implies, the most striking disturbance is the production of hallucinations, usually visual. Particularly noteworthy in this respect is mescaline, the active principle of the cactus peyote used by North American Indians. Aldous Huxley has written much of the "other-worldly" experiences produced by hallucinogenic seeds, mushrooms, roots, and plants, and has vividly described the striking color patterns which are often seen after mescaline ingestion; these give rise to some of the pleasant sensations associated with its use.

In addition to visual hallucinations, disorders of time are often experienced under the influence of hallucinogens. It is as though an individual's innate time clock had its regulator readjusted, usually radically toward the slow side. Such an effect may be due to the "impedance" to trans-synaptic transmission which has been shown to be an effect of hallucinogens. Fabing has compared the emotional effect on the individual involved with that which would be aroused in baseball fans if a double play ball were lobbed indifferently from base to base instead of being thrown with maximum speed and precision. Even more unpleasant sensations may be excited by hallucinogens. Feelings of depersonalization, anxiety and even terror are characteristic of high doses of LSD. Many individuals, nonetheless, enjoy the milder sensations associated with ingestion of low doses.

The fact that pharmacological agents can produce aberrations in mental activity with only minor effects on other systems has added strength to theories that mental illness might have a simple chemical or physiological basis. But no one has yet found the physiological or chemical keys to this Pandora's box of troubles.

Simpler questions remain without definitive answers. Which brain cells are affected by hallucinogens and what chemical mechanisms are concerned? Extensive studies have been made on the distribution of various of these hallucinogens after administration. These studies indicate that the hallucinogens are taken up by all the tissues of the body with only a very small proportion reaching the brain. Again, in brain itself, the distribution is widespread so that the manner in which the hallucinogens assert their effects cannot be determined from a knowledge of their geographical spread. There must be something particular about certain cells in the brain which makes them susceptible to these agents, i.e., these cells must have something distinctive about them chemically. Neurophysiologists tell us that their evidence indicates that the hallucinogens act in the lower regions of the brain, i.e., those regions which loom so importantly in the already described work of Olds, *et al.*, Magoun and Hess. It remains for the neurochemists to learn the chemical peculiarities of cells in this area which render them sensitive to hallucinogenic agents. Some limited progress has been made.

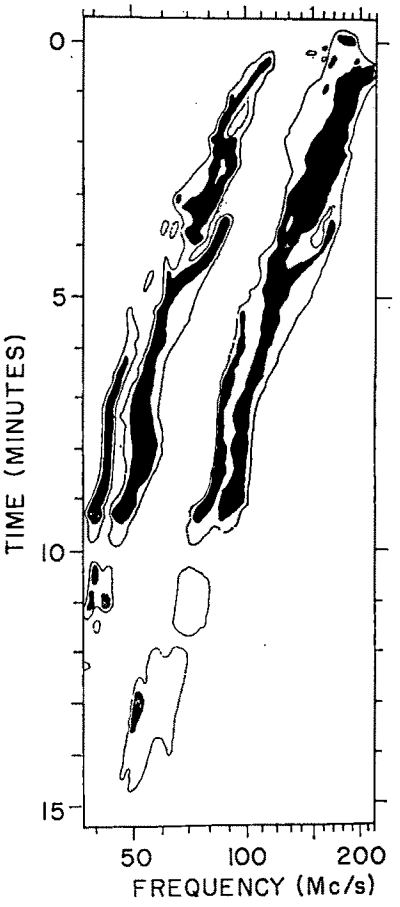


FIG. 10. Frequency bands in the radio noise from the sun.

librium process. However, these bursts are of great interest, and the frequency spectrum of one type of such bursts observed [6] at times of strong flare activity on the sun is shown in the next figure (Fig. 10). The dark areas are areas of constant noise power. The most outstanding characteristic of this emission is that at any one time the emission occurs in two frequency bands which differ in frequency by about a factor of 2. The great similarity of the spectra in the two bands suggests that the noise comes from the same source, possibly a non-linear oscillation of a section of plasma in which the fundamental and second harmonic frequencies have appreciable amplitudes. The shift of the radiation spectrum from higher to lower frequencies with increasing time suggests that the disturbance travels away from the sun from regions of high electron density to regions of lower density. We can calculate the apparent speed with which the disturbance travels from the known distribution of the electron density in the corona if we assume that the signal originates at a point in the corona where the frequency of the disturbances equals the local plasma frequency. The velocities are of the same order as the velocities of the corpuscular streams of matter in solar prominences, thus supporting this interpretation of the phenomenon.

Longitudinal electron oscillations at frequencies equal to the plasma frequency and its harmonics have been excited in laboratory plasmas and show many of the characteristics of this radial emission from solar flares. Shooting an electron beam through a plasma provides a mechanism for setting up standing waves of longitudinal electron plasma oscillations. If one plots the frequencies of these oscillations as a function of the electron density, one finds one can determine particular densities at which the plasma oscillation occurs at two different frequencies with the same density. This is illustrated [7] in Figure 11, where one can see these mul-

The aromatic structures of LSD, mescaline, and many other hallucinogens suggested that aromatic constituents of the brain should be given particular consideration. The identification, in 1951, of one such material, serotonin, by Erspamer and, independently, by Rapport and his co-workers, was the culmination of several years of effort to isolate a principle with powerful activity against smooth muscle in such tissues as the uterus, gut, and vascular tree. Shortly thereafter, Twarog and Page discovered that serotonin was present in brain, being concentrated in the paleocortex, diencephalon, and brain stem. It remained for Woolley and Shaw to propose that LSD exerted its mental effects by interfering with serotonin and that schizophrenia could be due to abnormal serotonin metabolism [11]. This ingenious and heuristic idea is yet to be proved or disproved despite eight years of research effort.

LSD is indeed a very powerful inhibitor of serotonin on some systems but so, too, is 2-bromo-LSD which also penetrates the brain but has little or no hallucinogenic activity. Intensive research effort has led to the accumulation of a mass of confusing and often conflicting data on the interactions of LSD and other hallucinogenic agents, not only with serotonin, but also with norepinephrine and other catecholamines, and with acetylcholine and histamine; all these amines occur in the central nervous system and have been proposed, at one time or another, as chemical transmitter substances. Some hallucinogens, at least, have been shown to modify carbohydrate metabolism and phosphorylation in brain slices, to increase phosphorus uptake by brain, to alter membrane permeability to sodium and potassium ions, and to affect various other metabolic systems. Which, if any, of these actions is important to the hallucinogenic effect? Although no one has yet found an Ariadnian thread to lead us out of this maze, the apparently selective action of the hallucinogens on certain areas of the brain seems most consistent with a mode of action involving one or more of the brain amines since these are concentrated in the appropriate areas. Further support for this position has been derived from a study of the interaction of these same brain amines and a number of newly discovered psychopharmacological agents.

Within the past decade, three families of potent psychopharmacological agents have been introduced into clinical medicine. These have had an inestimable effect on the treatment of the mentally ill. By 1957, their wide usage in North America had resulted, for the first time in recorded history, in a decrease in mental hospital populations. These new drugs have generally lessened the time required for hospital treatment and have even allowed the return to society of some cases previously judged as "hopeless." They have, moreover, changed the whole atmosphere of mental hospitals, have converted the all-too-frequently understaffed back ward from a dangerous bedlam to a place fit for human

tiple frequencies occurring for different standing wave patterns in the experimental tube. Specifically, one can observe two different frequencies for the same density in the same manner as is observed in the solar flare experiments. Although these laboratory experiments do not meet many of the conditions which are present in the solar experiment, they suggest the possibility of carrying out laboratory experiments of solar phenomena to clarify our physical understanding of these interesting

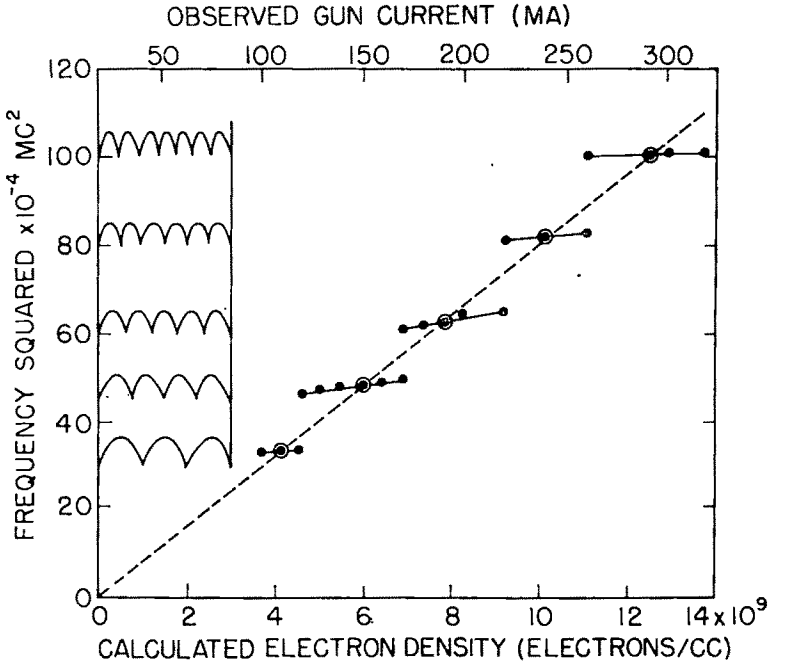


FIG. 11. Plasma oscillation frequency bands in an experimental laboratory tube.

effects. It should be pointed out in passing that if these types of plasma oscillations are purely longitudinal, they cannot generate electromagnetic waves which we observe with our radio telescopes. Coupling can be achieved, however, in the presence of gradients in temperature or density. Although the efficiency of transfer of longitudinal to transverse energy is small, transverse electromagnetic energy is observed from these oscillations in the laboratory, and there appears to be sufficient energy stored in a plasma oscillation in the sun to account for the large bursts of radio noise which we observe from this source.

A very widespread form of non-thermal emission both from ours and other galaxies has a spectrum illustrated [2] in Figure 12. Except at very low frequencies, the intensities fall off monotonically with increasing frequency. This form of radio noise may well originate from electron emission of highly relativistic electrons orbiting in weak interstellar

beings, and even conducive to recovery of their sanity. In addition, these drugs have permitted the alleviation of non-pathological anxiety and tension in many persons under temporary stress. Their discovery must be rated one of the most beneficial strokes of serendipity in medical history.

The progenitors of the three important families—phenothiazines, rauwolfia alkaloids, and monoamine oxidase inhibitors—were tried initially for pharmacological purposes other than psychiatric ones, and their mental effects were a completely unexpected dividend. The phenothiazines and rauwolfia alkaloids soon became known as "tranquilizers" and the monoamine oxidase inhibitors as "psychic energizers." These terms, used to describe the clinical effects, have become part of almost everyone's basic English vocabulary.

Reserpine, the first tranquilizer to be widely used, is an alkaloid derived from the plant *Rauwolfia Serpentina*, the powdered roots of which have been used for centuries in India for various ills. It was introduced in 1953 as an antihypertensive agent. Its tranquilizing properties were considered a nuisance until its introduction into psychiatric medicine. For a time, not even a reasonable hypothesis was available as to how it produced its tranquilizing and hypotensive effects. However, in 1955, Pletscher and others showed that reserpine depletes serotonin from its storage depots in the body [12], whether these be in the gut, blood platelets, brain, or other tissues. It does this by affecting the binding sites for serotonin in intracellular granules. For a short subsequent period, the actions of reserpine were thought to reflect simply a deficiency of serotonin. It was soon shown, however, that reserpine also depletes catecholamines from their storage sites in all parts of the body including sympathetic nerves and brain. The action of reserpine is now thought to reflect a depletion of one or more central amines but, despite numerous ingenious experiments, it is not known whether a deficiency in serotonin or in catecholamines plays the greater role.

Chlorpromazine, the first of the phenothiazines to be introduced clinically, was initially tried out as a basal anesthetic and an antinauseant. It was introduced into clinical psychiatry at about the same time as reserpine and met with instant success. Again, the mechanism of action was completely unknown even after chlorpromazine had come into wide clinical use. It was soon learned that chlorpromazine, like reserpine, acts on subcortical brain centers but only later was it determined that the phenothiazines were powerful blockers of some of the actions of catecholamines and serotonin. Many different phenothiazines have now been introduced clinically: some of these, such as chlorpromazine, have a blocking action against another central amine, histamine; others do not. Those phenothiazines with little or no antihistaminic action have a greater tendency to produce Parkinsonian-like reactions

magnetic fields. An electron of 100 Mev energy, rotating in a magnetic field of 10^{-5} gauss, radiates its maximum energy at a frequency of 100 megacycles/sec., which is right in the region observed. To fit the theory to the measured spectrum, a highly non-Maxwellian distribution of electron energies must be postulated. The distribution of energy which we must take does agree with the form of energy distribution of primary cosmic ray particles. Also, the concentration of relativistic electrons required to explain the observations is of the order of 10^{-11} elec/cc

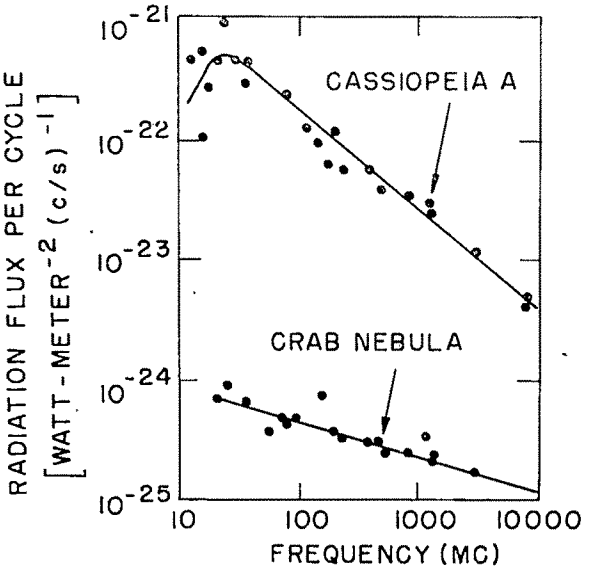


FIG. 12. Non-thermal radio emission from stellar bodies.

which agrees with the density of primary cosmic particles from our galaxy. However, there is a serious difficulty which shows up when we measure the radio noise from the Crab nebula. While the Crab nebula shows high degrees of polarization of its optical spectra, no polarization can be detected at radio frequencies although theory predicts a large effect. Perhaps the lack of r.f. polarization results from some depolarization effects, such as the twisted, irregular, magnetic fields in neighboring volumes of space which succeed in randomly orienting the polarization axis, but we have no real proof of such ideas and the problem remains unsolved.

Although our interpretation of the origin of non-thermal radio noise is convincing for many sources, including our own galaxy, there are some intense sources which need additional explanation. Centaurus A has an emitting power of about 100 times that of our galaxy, and Cygnus A about 10^5 times. To explain these intensities by our current theories

and a lesser tendency to produce sedative reactions than those with strong antihistaminic action. Thus, not only do the phenothiazines provide another link between central amines and tranquilization, but they suggest new approaches to the enigma of Parkinsonism.

The psychic energizers were introduced in 1957. Iproniazid, the prototype of the series, proved to be unsatisfactory as an anti-tubercular agent, but the euphoria it produced soon suggested its value for depressed mental patients. Zeller had already shown that iproniazid had the ability to inhibit the enzyme monoamine oxidase and, shortly after, Kline and co-workers [13] proposed this as the reason for its effect on mental patients. Monoamine oxidase is an important enzyme for destroying the catecholamines and serotonin, and it was reasoned that inhibitors of this enzyme might delay the destruction of these amines and thus lead to increases in their brain concentrations. This has been shown to be the case. Thus, we have three important new classes of psychopharmacological agents whose advent has been of tremendous value in treating the mentally ill. These agents, like the hallucinogens, affect many metabolic systems and it has so far proved impossible to determine the exact chemical mechanism of action. They all appear to act, however, at subcortical levels and the hypotheses most consistent with present knowledge link their psychopharmacological actions with their effects on the central amines, particularly the catecholamines and serotonin, possibly at synaptic sites. In general, blockade or depletion of these amines is accompanied by tranquilization, reduction of fear or anxiety and a diminution of disturbed thought processes. Conversely, an increase in the effective level of these amines is accompanied by an enhanced psychic state and an elevation of mood.

Neurochemical Investigations

As, in the 1950's, electrophysiological investigations provided fascinating new insight into the function of subcortical areas which might be construed as being involved in mental illness, so did the introduction of new classes of psychotropic drugs provide evidence suggesting that these agents also might be acting on these same areas of the brain. Independently of the neuropharmacological and neurophysiological studies, neurochemical researches were being conducted on the location and physiological actions of certain central amines. Some, such as acetylcholine and norepinephrine, had been well studied in peripheral tissue where they had a well-defined role as neurotransmitter substances. The others, dopamine, serotonin, and histamine, were also familiar as peripheral humoral agents but with a much more vaguely defined role. For years, interest in discovering chemical neurotransmitter agents in brain has been running high, but unequivocal data are difficult to obtain.

The neurophysiologist has delicate electrical techniques with which

would require much greater magnetic fields than we have any evidence for in interstellar space. The possibility of amplification of radiation as it passes through plasmas could explain some of these effects and would have important consequences on the total radiant energy loss from thermonuclear devices.

The possibility of such an amplification lies again in the relativistic change of mass of hot electrons. In the explanation of the Trubnikov theory, we invoke the increase of mass with increasing velocity of the

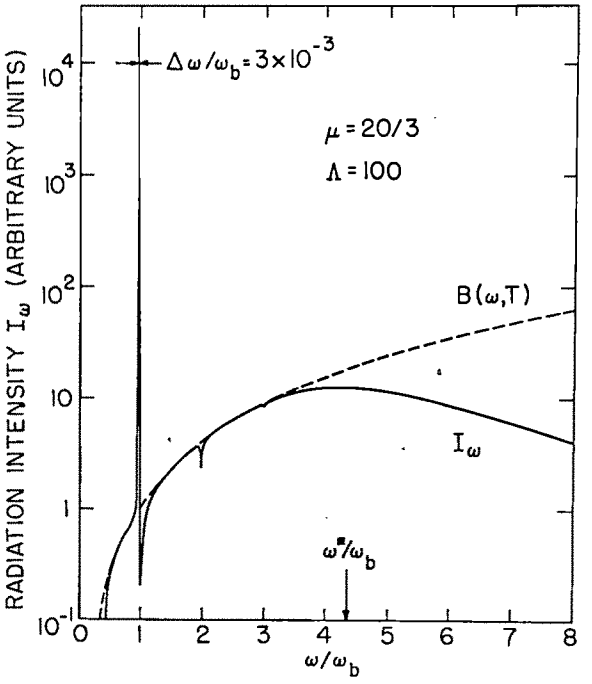


FIG. 13. Theoretical prediction of plasma amplification at the cyclotron frequency.

electrons produced by a properly phased electromagnetic field. Supposing we reversed the argument, we find that, under particular circumstances associated specifically with the case where the electromagnetic field reduces the velocity of the electron, this slowing down of the electron reduces its mass. If the frequency of the applied microwave field is slightly greater than the cyclotron frequency of the electron in question, then the field when it slows the electron down will increase ω_b as seen in equation (2) and ω_b will approach ω. This process involves a loss of mass for the electron, and since the conservation of mass and energy must hold, we must ask what happens to this lost mass. From the Einstein relation between mass and energy we suppose that this loss in mass has gone into energy at the frequency ω of the applied field. If this physical explanation

to explore the function of small groups of cells in the brain. The neuropharmacologist has an ever-increasing armamentarium of centrally acting drugs with which to modify brain function and can use neurophysiological techniques to study their effects. But what weapons can the neurochemist bring to their common battle front? Unfortunately, he is still fighting with a bow and arrow in an atomic age. It can be said, as a general principle of neurochemistry, that, whenever the chemistry of the brain is perturbed, mental symptoms are an early and prominent result. This is seen, for example, in the anoxia of flyers, the hypoglycemia of the diabetic heading for insulin shock, or the early stages of anesthesia. In each case, mental symptoms are only a phase on the way to unconsciousness; if unconsciousness represents a mountainous perturbation in the chemistry of the brain, then mental symptoms represent the insignificant foothills. Minor perturbations in the chemistry of the brain are therefore probably all that would occur in mental disturbances. Furthermore, mental illness of all types is characterized primarily by emotional disturbances, except in tne case of schizophrenia where emotional disturbance is overshadowed by a peculiar disorder of rational thinking often described as a "loosening of association." In most mental illness, therefore, one may expect the minor perturbations in brain chemistry to be limited to those subcortical areas shown by the neurophysiologist to be important for emotional response, and by the neuropharmacologist to be the primary site of action of psychopharmacological agents. The neurochemist, then, must look for minor changes in the chemistry of small, discrete brain areas with techniques which are barely capable of measuring accurately the concentration of many substances in the total brain.

It is small wonder that the only good correlations so far made between mental state and biochemistry involve constituents of the urine, where concentration mechanisms yield much larger amounts of material for analysis. In one study, Elmadjian [14] measured the epinephrine and norepinephrine excretion in the urine of the members of the Boston Bruins hockey team before and after their games. After a hard-played game, most players showed approximately a six-fold increase in norepinephrine excretion. Two injured players, who were unable to play and were concerned about their future with the team, excreted increased amounts of epinephrine instead, and one player who was ejected from a game for fighting, excreted increased amounts of both epinephrine and norepinephrine. The coach was variable in his excretion of epinephrine and norepinephrine depending on the performance of the team. The general finding that epinephrine tends to be excreted in anxiety-producing situations and norepinephrine in anger or action-producing situations has been well corroborated in other studies. Years ago, the great physiologist, Walter Cannon, had produced a view of the sympathetic nervous system,

corresponds to the conditions of the plasma in question, this provides an amplification mechanism. Rather peculiar energy distributions are necessary for the specific conditions of the process to occur, but these energy distributions are not impossible and may be very likely.

Calculating theoretically [8] the details of this kind of mechanism lead to a spectrum illustrated in Figure 13. Here we can see a very large peak occurring at the cyclotron frequency due to this amplification mechanism. Because of the logarithmic nature of the effect, the area under the cyclotron peak far exceeds the total area under the rest of the curve, corresponding to very large amplification factors. This process awaits experimental verification, but, if it is true, it may explain the copious radiation from apparently anomalous radio stars, and it also can play a controlling role in the total radiation loss from thermonuclear reactors.

In conclusion, let me hope that you have caught the basic concept of this newly appreciated state of matter. Our planet is whirling through space surrounded by a shell of plasma. Interstellar space is full of it and whole galaxies tell us of their existence from the radio signals emitted by their plasma structure. The study of this radiation coming from ionized plasmas has in recent years become the focus of three hitherto fairly divergent disciplines, ionospheric physics, astrophysics, and research on controlled thermonuclear fusion. We are beginning to realize that we may study the tremendous energy sources which are the stars by laboratory studies of this medium, and perhaps when we have pushed our fundamental research further, we will even be able to control the energy conversion which accounts for the sun. Most of all, however, man has just awakened to the fact that a new state of matter surrounds him on all sides, and he is challenged to study this in the stars, in interstellar space, in the rim of his own atmosphere, and in the laboratory all at once and with every tool at his disposal.

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with its hormone "sympathin," as preparing the body for "fight or flight." It now appears that norepinephrine is the component of "sympathin" more closely associated with fight, whereas epinephrine is more closely associated with flight.

Is there an analogous correlation between specific mental states and the release of certain chemicals or groups of chemicals in the brain? None has been well demonstrated to date, but the technical difficulties are great. One cannot have the brain and measure it too, nor is it technically feasible to measure the chemical output, in the blood, of circumscribed brain areas. Nonetheless, some highly suggestive evidence is available, as to correlations between brain chemistry and behavior, again involving the central amines.

The central amines receiving most active attention are shown in

TABLE I

	Neocortex,	*Paleo-cortex,*	*Basal Ganglia,*	*Diencephalon,*	*Brain Stem,*
Approx. wt humans.	1150 g.	60 g.	50g.	30 g.	20 g.
Dopamine	Trace	Trace	++++	++	++
Norepinephrine	Trace	Trace	+	+++	++
Serotonin	Trace	+	+	+++	++
Acetylcholine	++	++	+++	++	++
Histamine	+	+	+	+++	+
Pleasure or punishment	0	+	++	+++	++++

Table I, along with an approximate indication of their localization. The over-all concentration of each of these amines in whole brain is approximately 1/1000 the concentration of energy-producing intermediates associated with glucose and oxygen metabolism. The enzymes for manufacturing and destroying these amines are also present in brain tissue. Furthermore, amines manufactured in other parts of the body do not seem to gain access to the brain except, possibly, in minute concentrations. The blood/brain barrier apparently assures that the brain uses only its own supply of amines. Their exact role so far has defied definition, although it is clearly unrelated to energy metabolism.

To go back to the analogy of the television set, where the tubes all worked on the same basic principle but had slightly different operating characteristics, the cells of the brain all work on the same basic principle in that they derive their energy from glucose and oxygen consumption. But they may differ in their operating characteristics by using different chemicals to modulate their excitability and to relay messages across cell junctions or synapses. The central amines are eminently suited to such roles, both by their distinctive localization and by their known

CHEMICAL EQUILIBRIA AND REACTIONS IN SEMICONDUCTORS

By N. B. HANNAY

THE UNIQUE electrical properties of semiconductors are largely due to the presence in the solid of a low concentration of chemical impurities. Ideally, these would be distributed in some prescribed manner in a perfect crystal of the semiconductor which was free from all other impurities. It is not surprising that a great deal of scientific and technological effort has gone into the attempt to achieve this ideal and that this effort has engaged the attention of people representing a number of disciplines. Such semiconductors as germanium and silicon have been brought to a very high degree of purity and crystalline perfection, and the chemical and physical effects of impurities in these materials are understood in a very broad way; other semiconductors are following closely behind in this understanding. Thus, the last decade has seen the development of such powerful purification techniques as zone-refining, new techniques for the growth of large single crystals as well as the extension and improvement of older methods, and the unraveling of many of the complexities of dislocations and other crystal imperfections. The interest in the controlled introduction of chemical impurities has led to advances in such areas as phase relationships and atomic movements in the solid. An enormous amount has been learned about the electrical, optical, and magnetic effects of impurities in semiconductors.

The combination of these circumstances has also provided for the first time both the system and the means for the study of a variety of chemical interactions in solids. Both equilibrium and nonequilibrium processes have been investigated; some of these are processes which occur in all solids but which are most conveniently studied in semiconductors, while others occur only because of the unique properties of semiconductors. It is the purpose of this article to point out some of the ways in which semiconductors are serving as media for these studies, which are giving us new insight into some of the subtler and formerly less well-understood aspects of the physical chemistry of solids.

Analogy to Solutions of Electrolytes

In a number of respects the solid solution of appropriate chemical impurities in a semiconductor is analogous to an aqueous electrolytic solution, with conduction electrons and holes* playing the role of

* Elementary semiconductor concepts are treated in a number of references [1]. A hole may be regarded as the absence of a valence electron in the particular location in a semiconductor, while a conduction electron is one in excess of the normal complement of valence electrons. Semiconductors are called *n* and *p*-type when the number of conduction electrons is greater, or less, than the number of holes.

peripheral actions. This suitability has inspired presumptive descriptions of the amines as neurotransmitters, neurohormones, or neurohumoral agents, but only indirect evidence exists to support such a role.

One important line of evidence comes from the ability of the amines to modify the electroencephalogram (EEG) [15]. Each of the amines has been shown capable of exciting the resting EEG pattern when injected in minute concentrations into the arterial circulation of the brain. Although the effect noted on the EEG is the same in each case, the in-

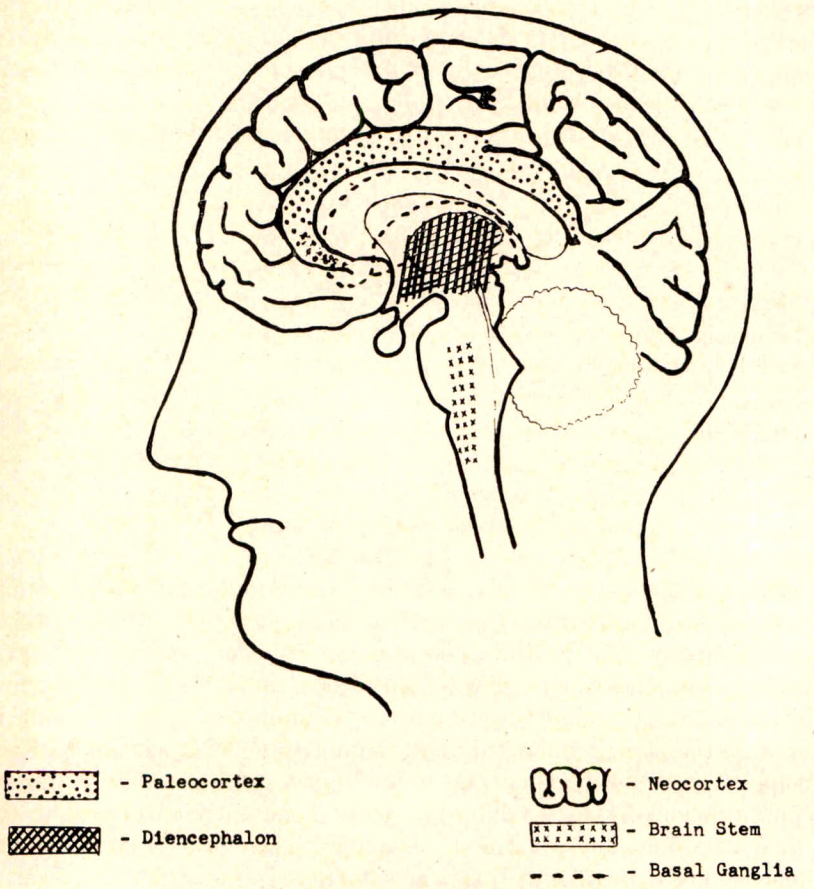


FIG. 1. Sagittal view of the human brain.

dividual amines need not be affecting the same cells. The EEG is recorded from the cortex and exciting any neurons in a chain passing through successive subcortical levels might produce such an effect. Evidence from lesions placed in the brain stem strongly suggests that the catecholamines exert their greatest influence on the EEG at this point, whereas acetylcholine acts considerably more rostrally. Exactly where histamine and serotonin exert their greatest effect remains to be determined.

hydroxyl and hydronium (or for simplicity, hydrogen) ions, respectively. This analogy has been pointed out by Fuller [2], for example. The ionization of water,

$$H_2O \rightleftharpoons H^+ + OH^- \tag{1}$$

is governed by the law of mass action,

$$[H^+]\,[OH^-] = K_w \tag{2}$$

where K_w is the equilibrium constant, a function of the temperature. At 25°C., K_w is approximately 4×10^{27} cm^{-6} (expressing ionic concen-

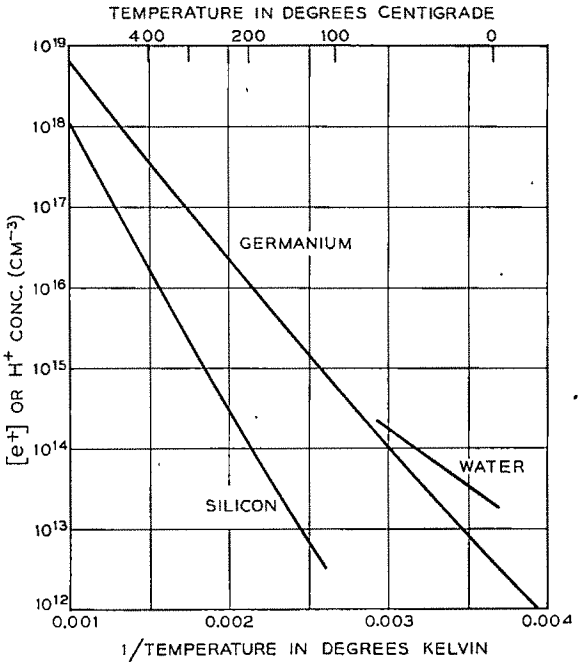


FIG. 1. Ionization of germanium, silicon and water as a function of temperature. Ordinates for the first two cases represent $[e^+] = [e^-]$, and for the last $[H^+] = [OH^-]$.

trations in numbers of ions per cm^3). The thermal breaking of bonds in a semiconductor to produce conduction electrons and holes is an analogous process, and is also governed by the law of mass action. Thus, the ionization of the silicon lattice may be written as

$$\text{Si lattice} \rightleftharpoons e^- + e^+ \tag{3}$$

where e^- and e^+ represent conduction electrons and holes. The corresponding mass action relation is

$$[e^+]\,[e^-] = K_{Si} \tag{4}$$

The EEG gives no indication of the behavioral effects of these amines; a better indication has been given in the discussion of the effects of drugs which influence them. Table II gives a summary of some drug-amine interactions. As can be seen from the table, the spectrum of action of the drugs is too broad to permit any strong correlations. Some additional information may, however, be added by experiments in which precursor amino acids of certain amines are combined with drugs.

Dihydroxyphenylalanine, the precursor amino acid of the catecholamines, when administered alone will enhance the ability of an animal to avoid an electric shock, but will not encourage his efforts to obtain a food reward. The precursor amino acid of serotonin, 5-hydroxytryptophan, when given alone will cause both a deterioration in an animal's ability to avoid shock, and his desire to work for food[16]. Large doses of dihydroxyphenylalanine in animals will cause obvious signs of excitation along with marked heightening of response to any stimulus. Large doses of 5-hydroxytryptophan will cause gross tremors, irritability, and an inattention to the environment. Dihydroxyphenylalanine is quite effective at counteracting reserpine-induced sedation while 5-hydroxytryptophan is not. The effect of a monoamine oxidase inhibitor in combination with either of these two amino acids is to enhance the physiological result of the amino acid administration. Thus, it would appear as if the catecholamines are primarily responsible for changes in the degree of alertness of the animals, while serotonin is correlated with sensations of moods which are much less obvious. Similar experiments using histidine, the precursor amino acid of histamine, have not been tried, while acetylcholine, not having an amino acid precursor, does not lend itself to a comparable type of experiment.

It is evident that insufficient information is at present available to make strong correlations

TABLE II

	Acetylcholine	*Histamine*	*Dopamine*	*Norepinephrine*	*Serotonin*	*Action*
Phenothiazines	0	0 or blocks	Blocks	Blocks	Blocks	Tranquilizer
Reserpine	0	0	Depletes	Depletes	Depletes	Tranquilizer
Monoamine oxidase inhibitors	0	?	Enhances	Enhances	Enhances	Psychic energizer
Lysergic acid diethylamide	Weak block	?	?	Weakly mimics	Strong block	Psychotomimetic
Mescaline	0	?	?	Weakly mimics	?	Psychotomimetic

where the equilibrium constant, K_{Si}, is again a function of temperature. At 25°C., K_{Si} is approximately 10^{20} cm^{-6}, so it may be seen that silicon is a weakly ionized medium, like water. Figure 1 shows the ionization of silicon, germanium, and water as a function of temperature, and the exponential dependence of K on temperature is evident. In the pure materials, this intrinsic ionization of the medium produces the charged particles which are responsible for the electrical conductivity of the semiconductor or water.

Impurities which are donors or acceptors in semiconductors are analogous to bases and acids in water. Thus, phosphorus in silicon is a donor and produces an excess of conduction electrons over holes by the ionization process

$$P \rightleftharpoons P^+ + e^- \tag{5}$$

just as ammonium hydroxide, NH_4OH, for example, adds OH^- ions to water

$$NH_4OH \rightleftharpoons NH_4^+ + OH^- \tag{6}$$

Correspondingly, acceptors can be compared with acids. In all these cases the law of mass action governs the ionization, with the equilibrium constant an exponential function of temperature. A quantitative difference between semiconductors and water solutions arises because the electron and hole mobilities are generally orders of magnitude greater than ionic mobilities in aqueous solutions, while the ionic mobilities in solids (e.g., of the P^+ ion) are usually very much less.

The treatment of the electron and the hole as chemical entities was originally due to Wagner and Schottky [3], and has been examined in some detail by Reiss [4], Reiss, Fuller and Morin [5], Longini and Greene [6], Brebrick [7], and Garrett [8]. To a good approximation, the law of mass action is a valid approach to the semiconductor equilibria which have been of most interest experimentally. We will next examine some simple consequences of the hole-electron equilibrium discussed above.

Effects of the Hole-Electron Equilibrium

An example of the importance of the hole-electron equilibrium in chemical equilibria in solids is its effect on the solubility of an impurity in the solid in contact with an external phase. Just as, for example, the solubility of a slightly soluble base in water solution can be increased by the addition of an ionized acid, correspondingly the solubility of a donor in a semiconductor will be enhanced by the presence of acceptors. A case of this kind has been studied in detail by Reiss, Fuller, and Morin [5], the solubility of lithium in silicon being studied as a function of acceptor concentration. Lithium is a donor in silicon, and furnishes a convenient means of studying chemical equilibria because of its very high diffusivity at moderate temperatures. This enables one to reach

between subcortical neurophysiological systems important to the mental state, neuropharmacological agents, and the chemistry of the central amines. Postulates, however, have been made. Brodie and Shore [17], for example, suggested that serotonin may be the neurotransmitter for the trophotropic system of Hess, while norepinephrine may be the neurotransmitter substance for the ergotropic system. We have proposed that, at least as far as the extrapyramidal motor system is concerned, histamine and acetylcholine may be components of one modulating system while the catecholamines and possibly serotonin are components of another[18]. More recent experiments suggest to us that histamine may be of importance to pain or punishment centers in the brain. Unfortunately, none of these ideas can be well tested because sufficiently delicate chemical analytical methods are not yet available.

As more refined methods of analysis are forthcoming, further efforts will certainly be made to draw correlations between mental state and brain biochemistry. Particular attention is bound to be paid to the central amines but, at the same time, chemists will be alert to spot other chemical features which may distinguish the subcortical cells concerned with behavior from other cells in brain. We may be sure, at least, that the struggle to understand brain and mind will continue.

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equilibrium in a moderate time in samples of reasonable size. The silicon samples were brought to equilibrium with an external phase of molten tin containing a low concentration of lithium.

The equilibria which determine the solubility of lithiun in silicon containing boron as the acceptor element can be written as follows:

$$\begin{array}{l} \mathrm{Li_{(Sn\ phase)}} \rightleftarrows \mathrm{Li_{(Si\ phase)}} \rightleftarrows \mathrm{Li^+} + e^- \\ \qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad + \\ \mathrm{B_{(Si\ phase)}} \rightleftarrows \mathrm{B^-} + e^+ \\ \qquad\qquad\qquad\qquad\qquad\qquad\qquad\quad \downarrow\uparrow \\ \qquad\qquad\qquad\qquad\qquad\qquad\qquad\quad \text{Si lattice} \end{array} \qquad (7)$$

In pure silicon (no boron) the solubility of lithium for a given external phase is determined simply by temperature. The presence of boron will

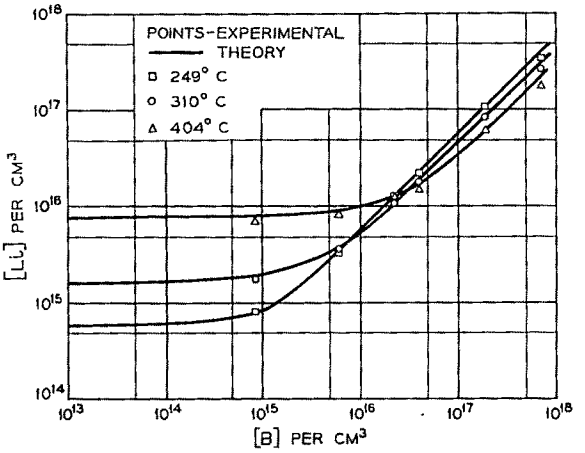


FIG. 2. Lithium solubility as a function of acceptor concentration.

increase the Li^+ solubility through the equilibria (7); the holes introduced by the boron operate through the hole-electron equilibrium to remove the electrons introduced by the lithium and thereby drive the top equilibrium in (7) to the right. A simple expression for the equilibrium solubility of lithium (which is essentially all ionized to Li^+ above room temperature), as a function of the boron concentration, can be derived from the mass action equations for the equilibria in (7). This solubility expression also contains the temperature, and the lithium solubility at that temperature in pure silicon. The calculated solubilities are shown in Figure 2 for three temperatures and compared with the experimentally determined values, with excellent agreement. At low acceptor concentrations, well below the lithium solubility for pure silicon, the acceptor has little effect; when the acceptor concentration exceeds this solubility, however, there is approximately a 1 to 1

BIOLOGICAL ASPECTS OF NUCLEAR WEAPONS*

By CYRIL L. COMAR

THERE has naturally been great public interest about the effects of radiation on the human population, past, present, and future, mainly because nuclear weapons now make it possible for practically every organism living on earth to be exposed to detectable radiation from a single event controlled by man. Lack of knowledge, misunderstandings, a new mysterious force abroad in the world, personal involvement, radioactive contamination of milk and babies—all have led to a degree of emotional controversy.

It is attempted here to present an up-to-date and balanced view of the important biological radiation problems of the day. For the purposes of clarity and not evasion it is necessary to oversimplify, restrict the discussion to the most important matters, and omit details. Hopefully, this discussion will further an understanding of the difficult problems of present-day affairs on a basis both national (should we ban nuclear tests?) and personal (should I change my dietary habits?).

Some Generalities

It must be crystal clear to all sane and thoughtful people that nuclear warfare is not the means for attainment of national or ideological goals. The category of the sane and thoughtful would include all people from extreme Left to Right, the general public, military leaders, and government officials. Also included are all peoples of other countries, especially those of Russia. From news impressions, one cannot be sure that the Red Chinese have reached this degree of understanding as yet, but it is to be hoped they would by the time they are capable of producing and conveying nuclear weapons. Our primary objective then must be to avoid war, but at the same time to preserve our political systems of individual rights and freedom. How best this can be accomplished is the crucial issue, and strategy is a matter to be much debated, although not here. Personally I feel that simply to display moral indignation about the seriousness and necessity of avoiding nuclear warfare provides our government leaders with little that they do not already fully appreciate.

There seems to be public consternation and confusion because reputable scientists have expressed differing views about the effects of nuclear weapons. Unfortunately, in a sense, the public seems to regard scientists as god-like in their pronouncements. Perhaps this is due to the advertising approach, where science is used to promote sales. Actually, we

* A lecture delivered January 11, 1962, as one of a series of Cornell University Lectures on *Nuclear Peril and Disarmament.*

increase in lithium solubility with added acceptor atoms. It can be seen that the solubility of the lithium can be increased by several orders of magnitude in this manner. Clearly, the presence of a donor in the semiconductor would have the inverse effect, and would depress the lithium solubility. This is analogous to the common ion effect in aqueous solutions. The concentrations in these experiments were determined simply from measurements of electrical conductivity, which give the electron (and hole) concentrations.

A second interesting example of the hole-electron effect relates to the distribution of an impurity between two possible lattice sites, in the semiconductor gallium arsenide, GaAs, in this case. In this material the normal donors are Group VI elements, which occupy arsenic sites and have an extra electron available to contribute to the crystal, and the normal acceptors are Group II elements which enter the lattice on gallium sites with a deficiency of an electron, thereby introducing a hole. Group IV elements could be expected to act as donors if they entered the lattice on gallium sites, whereas they would be acceptors on arsenic sites. The distribution of silicon between gallium and arsenic sites, with and without other added impurities, has been studied by Whelan [9]. Radioactive silicon was used to permit a determination of the total silicon present in solution, with electrical measurements providing the values for the hole and electron concentrations which permit the determination of the equilibrium distribution of the silicon between the two kinds of lattice sites. At low silicon concentrations, it was found that the silicon preferentially occupied gallium sites, only 1.8% of the silicon being on arsenic sites. At higher silicon concentrations however, the increasing electron concentration operates through the hole-electron equilibrium to reduce the silicon "solubility" on donor sites, while at the same time increasing it on acceptor sites. Thus we have the equilibria

$$\left.\begin{aligned} &Si_{Ga} \rightleftharpoons Si_{As} \\ &Si_{Ga} \rightleftharpoons Si_{Ga}^{+} + e^{-} \\ &Si_{As} \rightleftharpoons Si_{As}^{-} + e^{+} \end{aligned}\right\} \tag{8a}$$

$$\left.\begin{aligned} &[e^{-}]\,[Si_{Ga}^{+}] = K' \\ &[e^{+}]\,[Si_{As}^{-}] = K'' \end{aligned}\right\} \tag{8b}$$

where Si_{Ga} and Si_{As} denote silicon on gallium and arsenic sites respectively, and the K's are equilibrium constants. We may assume that at the measurement temperature (300°K.) essentially all the silicon in the solid is ionized, according to (8a). From (8b) we find that

$$\frac{[Si_{As}^{-}]}{[Si_{Ga}^{+}]} = \frac{K''}{K'} \cdot \frac{[e^{-}]}{[e^{+}]} \tag{9}$$

We may evaluate K''/K' by observing that in the limit of low silicon concentrations the GaAs is nearly intrinsic and $[e^{-}] \approx [e^{+}]$. We then rewrite (9) as

should be proud that anyone in this country can state his view and have his ideas represented widely, and we would not want it any other way. Also, disagreement among scientists is the rule rather than the exception, because such disagreement emphasizes the gaps in our knowledge, and very often stimulates needed research.

This places a burden on the public, the need to evaluate and judge. There is a keyword that can help, namely "credentials." From whence, and by what authority does a man obtain his information; what are his "credentials"? A reputable scientist, speaking in his own field, deserves careful attention—a scientist speaking out of his field should be given one vote, just as anyone else.

We must be especially careful about opinions and numbers presented only in part and selected to support specific points of view—for example, to prepare public opinion for nuclear war, one could in truth predict the continued existence of mankind regardless of any foreseeable dissemination of nuclear debris; in the opposite direction, one could predict, even from weapons testing carried out to date, what appears to be vast numbers of individual genetic deaths, and a vast amount of individual human suffering accumulated over thousands of years.

The credentials for viewpoints expressed here arise from a consensus gained by personal contact with various National and International Committees, reports of which are listed. Within recent years there has been developing general agreement as to the levels of radiation exposure resulting from past nuclear tests and the order of magnitude of effects expected. Disagreement arises primarily on moral issues and on the uncertainties in the necessary balancing of potential biological cost against potential benefit from a given course of action. All are keenly aware of the need for intensive research.

The Nuclear Weapon and Fallout

When a nuclear weapon is detonated, tremendous quantities of heat are produced within a small period of time—thousandths of a second—and within a relatively small quantity of matter. As a result, all materials within the immediate vicinity of the device are completely vaporized, and raised to temperatures approaching those of the sun. The high temperature gives rise to what is known as the "fireball," which expands rapidly, heating material within the environment as it expands, and which then starts to rise. As the fireball rises, violent winds are produced which can suck large quantities of soil or water into the hot fireball and molten particles can condense onto this material. These heavy dirt particles or droplets, with their attached radioactive contamination, may return rapidly to earth and are called "local fallout." The extent and nature of fallout may vary considerably. For example, an airburst in which the explosion occurs at an appreciable distance above the earth's

$$\frac{[Si_{As}^-]}{[Si_{Ga}^+]} = \left(\frac{[Si_{As}^-]}{[Si_{Ga}^+]}\right)_0 \cdot \frac{[e^-]}{[e^+]} \equiv R_0 \frac{[e^-]}{[e^+]} \tag{10}$$

where the subscript 0 refers to the value for the ratio, R_0, of acceptor to donor silicon in the limit of very low concentrations of silicon. As noted above, R_0 was found experimentally to be 0.018. The total silicon concentration can now be evaluated, by algebraic manipulation and by substitution of equation (10),

$$[Si_{Ga}^+] + [Si_{As}^-] \equiv \frac{1 + [Si_{As}^-]/[Si_{Ga}^+]}{1 - [Si_{As}^-]/[Si_{Ga}^+]} \cdot ([Si_{Ga}^+] - [Si_{As}^-]) \tag{11a}$$

$$\frac{[Si_{Ga}^+] + [Si_{As}]}{[Si_{Ga}^+] - [Si_{As}]} = \frac{1 + R_0[e^-]/[e^+]}{1 - R_0[e^-]/[e^+]} \tag{11b}$$

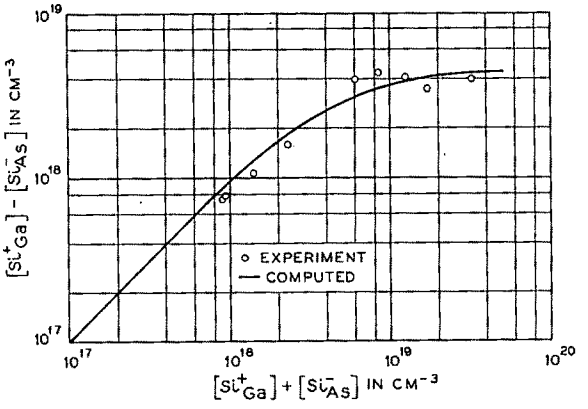


FIG. 3. Excess of donor over acceptor silicon as a function of total silicon concentration.

One sees that this expression correctly describes the behavior at low values for the total silicon concentration, since $[e^-]/[e^+]$ does not then differ greatly from unity and R_0 is small, so that the right hand side of (11b) is nearly unity. Thus we have $[Si_{Ga}^+] + [Si_{As}^-] \approx [Si_{Ga}^+] - [Si_{As}^-]$, and this can only be true if $[Si_{Ga}^+] \gg [Si_{As}^-]$, as was noted above. At higher silicon concentrations the material becomes more and more n-type however, and $[e^-]/[e^+]$ increases rapidly. The right hand side of (11b) therefore also increases above unity, so that $[Si_{Ga}^+] + [Si_{As}^-] \gg [Si_{Ga}^+] - [Si_{As}^-]$. For this to be true, the amount of acceptor silicon, $[Si_{As}^-]$, must be appreciable in comparison with $[Si_{Ga}^+]$, although it never exceeds it. The theoretical curve (11b) is plotted in Figure 3 and agrees well with the experimental values. It can be seen that at high silicon concentrations additional silicon is distributed approximately equally between the two sites as a consequence

surface, so that dirt or water are not sucked into the cloud, produces little or no local fallout; this may be an important consideration if the strategy of an attack is to use large weapons to produce damage by fire. A land-surface explosion, on the other hand, produces a cloud heavily loaded with debris, containing relatively great numbers of the large particles raised by the surface winds. Under such conditions, it is estimated that about 80 per cent of the total debris deposits as local fallout.

For purposes of discussion, then, fallout can be classified either as local or as world-wide. Local fallout, sometimes called "close-in" fallout, is defined as that which is deposited within 24 hours after detonation. Human beings have not been exposed to local fallout except for accidents such as involved the Marshall Islanders and the 23 Japanese fishermen. World-wide fallout can be classified into two categories—either as "tropospheric" or "intermediate" fallout, which is deposited within the first 30 to 60 days; or as "stratospheric" or "delayed" fallout, which may take many months or years for deposition. The radioactive iodine in our milk during October 1961, was an example of intermediate fallout from the Russian 1961 tests. Our present body burdens of radioactive strontium result mainly from stratospheric fallout from the tests prior to 1959.

Local fallout is distinguished by its settling speed and high radiation intensity, consisting of visible particles heavy enough to fall through the air. The particles are transported by winds as they settle; with light winds, they will settle close to the site of explosion and make an intense radioactive area; with heavy winds, they will be spread over larger areas, to give relatively lower radiation intensities. Local fallout is of critical importance in warfare, but of little interest from the standpoint of testing, because it is limited to the test site. The important thing about local fallout is that external radiation is the major hazard, and that the radiation intensity decreases rapidly with time, because the local fallout as produced contains considerable short-lived radioactive materials. As a rule of thumb, it is estimated that, for every seven-fold increase in time, the radiation intensity decreases by a factor of 10. For example, if the radiation intensity were 4000 units at one hour after the explosion, it would be 400 units after seven hours, and 40 units after 49 hours.

World-wide fallout is important both in warfare and in weapons testing. In contrast to warfare, about which we hope always to have to speculate, world-wide contamination is now with us. The earth and all living things on it are now contaminated to a degree with radioactive materials from past tests, and we must have an understanding of the levels of such contamination and the effects that such levels might produce in order to evaluate the biological cost of any further testing. It should also be noted that, as a result of warfare, those countries not involved will, however, be subject to world-wide fallout from the nuclear weapons used; also, areas in the warring countries that escape local fallout will be subject to world-wide fallout.

of the hole-electron equilibrium, in marked contrast to the situation at low concentrations where nearly all the silicon goes to donor sites.

This effect can be enhanced by the addition of a second electrically active impurity, and, to show this, samples were also examined which contained the donor selenium. The electrons contributed by the selenium serve to increase $[e^-]/[e^+]$ and therefore the right hand side of (11b), relative to the samples with no selenium. For

$$\frac{[Si_{Ga}^+] + [Si_{As}^-]}{[Si_{Ga}^+] - [Si_{As}^-]} \equiv \frac{1 + [Si_{As}^-]/[Si_{Ga}^+]}{1 - [Si_{As}^-]/[Si_{Ga}^+]}$$

to be greater in the selenium-doped samples than in the undoped samples- $[Si_{As}^-]/[Si_{Ga}^+]$ must also be greater, consequently the selenium-containing samples are expected to show a higher fraction of the silicon on acceptor sites than will samples containing the same total silicon but with no selenium. This was indeed found to be the case, as shown in Table I.

TABLE I

Specimen	[Se]	$[Si_{Ga}^+] + [Si_{As}^-]$	$[Si_{As}^-]/[Si_{Ga}^+]$
1	...	1.4×10^{19} cm^{-3}	0.55
2	1.1×10^{14} cm^{-3}	1.4×10^{19} cm^{-3}	2.4

Ion Pairing

Another class of chemical interactions seen in semiconductors, which is also familiar in aqueous solutions, is the formation of ion-pairs. A number of aspects of ion-pairing in the solid have been studied by Reiss, Fuller and Morin [5], again using lithium as a mobile donor. The interstitial ion, Li^+, can move rapidly through, say, the germanium lattice, and can form ion pairs with the immobile ions of substitutional acceptors such as Ga^-. Thus, we have the equilibrium

$$Li^+ + Ga^- \rightleftharpoons \text{Pair}$$

$$K = \frac{[P]}{([Li] - [P])([Ga] - [P])} \tag{12}$$

where $[P]$ is the pair concentration, K is the equilibrium constant, and [Li] and [Ga] are the *total* lithium and gallium concentrations, given by the sum of the amount in solution which is unpaired (but completely ionized at the temperatures of measurement) plus the amount paired. From (12) it follows that the fraction of Li^+ paired is

$$\frac{[P]}{[Li]} = \frac{1}{2}\left(1 + \frac{1}{K[Li]} + \frac{[Ga]}{[Li]}\right) - \left\{\frac{1}{4}\left(1 + \frac{1}{K[Li]} + \frac{[Ga]}{[Li]}\right)^2 - \frac{[Ga]}{[Li]}\right\}^{1/2} \tag{13}$$

The Bjerrum-Fuoss theory of electrolytic solutions gives an expression for the pairing equilibrium constant, and this is

$$K = 4\pi(q^2/\kappa kT)^3 Q(\alpha) \tag{14}$$

The Fission Products and Radioactive Contamination

To understand the implications of world-wide fallout which is important primarily after entrance into the body, we must consider the specific radioactive elements that are produced and their behavior. The terms radioactive elements, radioisotopes, and radionuclides are used loosely to designate forms of elements that are unstable and give off ionizing radiation. From an atomic explosion, there can result contamination with the fissile material itself, such as uranium or plutonium, and with the so-called fission products. The primary and secondary fission products consist of about 200 radioactive species of elements. In addition, there is the possibility that some radioactive materials may be produced by activation with the neutrons from the weapons, just as in the process used for production of radioisotopes in a nuclear reactor. From the biological standpoint, perhaps the most important activation product is the radionuclide carbon 14, arising from reaction of neutrons with nitrogen atoms of the atmosphere. Of the 200 or so radioactive species produced by the weapons, only a few are of biological importance. This is because a radioactive material must possess several characteristics to varying degrees, or otherwise it cannot be potentially harmful to the human population.

First, the radionuclide must be produced by the explosion in appreciable amounts.

The second characteristic that governs the biological hazard is the so-called physical half-life of the given radioactive material, this being the time required for one-half of any given amount to disappear by radioactive decay. For world-wide fallout, which is relatively slow in coming to earth, radionuclides with half-lives of less than days or weeks are of little significance.

Another important characteristic that governs the biological hazard is the efficiency with which the particular radioactive material is transferred from the atmosphere through the food chain to the human diet. This transfer occurs primarily by two routes: contamination can stick to surfaces of plants which are eaten directly by man or eaten by animals with the passage of the radioactive elements into milk and meat; another pathway is incorporation into the soil for uptake by plants through their roots and subsequent entry into the diet of man. Knowledge of these pathways is of utmost importance for future assessments because contamination by surface adsorption is governed by the *rate of fallout*, whereas contamination through the soil is governed by the *cumulative amounts* of radioactivity deposited. The rate tends to decrease relatively rapidly after cessation of nuclear explosions, whereas the cumulative total builds up from all previous tests and decreases only by radionuclides becoming physically unavailable and by physical decay.

where q is the ionic charge, κ the dielectric constant, k is Boltzmann's constant, and $Q(\alpha)$ is the pair-distribution function, a known function of the parameter $\alpha \equiv q^2/\kappa kTa$, with a the distance of closest approach of the paired ions. If a were known, the fraction paired in the semiconductor could be calculated from (13) and (14); since no independent determination of a is available, however, the inverse was done and a value of a was calculated from the measured value of the fraction paired, $[P]/[\mathrm{Li}]$. This latter was obtained by measuring conductivity and Hall effect, which gives a value for the carrier mobility. The mobility is a

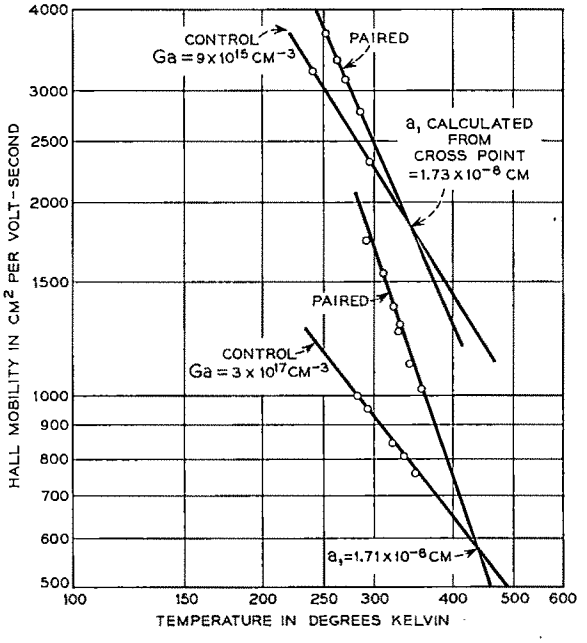


FIG. 4. Mobility vs. temperature for ion-pairing and control samples, for each of two concentrations of gallium.

function of the total number of ions present as these are the principal scatterers for the carriers.

The mobilities of two germanium samples were compared, as shown in Figure 4; one of these contained equal concentrations of lithium and gallium, the other contained only the gallium. The sample with both donor and acceptor was held at various temperatures so that the equilibrium number of pairs would form, and the mobility was measured at each temperature and compared with the control sample. At low temperatures, where a high fraction of the ions pair, the mobility of the sample containing both donor and acceptor was actually higher than that of the control despite the presence of twice as many impurities,

Metabolic barriers may prevent the radioactive material from reaching locations in the body where it may cause damage, depending on the behavior of the substance in the food chain or man's system. For example, although the rare earths are common fission products, their absorption from the intestinal tract is so small in man and animals that they are of little importance as a hazard to man. Similarly, any unfission uranium or plutonium would likewise be poorly absorbed.

A final important factor that governs hazard is the length of time that a material is retained in the body. Thus, for example, strontium 90, which deposits in bone like calcium and is removed relatively slowly, would be potentially more hazardous than cesium 137, which deposits in soft tissues, like potassium, and is removed fairly rapidly.

Thus, it appears that, of all the radioactive materials produced, only a few are limiting as a potential hazard; these are strontium 90, which is a bone-seeker and could eventually lead to leukemia and bone cancer, and cesium 137 and carbon 14, which would be of interest primarily from the standpoint of genetic effects. Under certain conditions, iodine 131 could be of potential hazard from tropospheric fallout, but because of its eight-day half-life, it does not present a long-term problem.

The human population is exposed to world-wide fallout mainly by radioactive contamination of food and water, the radioactivity then being eaten and built into the body where it serves as a source of internal irradiation. Thus, from world-wide fallout the radiation for the most part will be long-term, that is, delivered over the lifetime of the individual.

During the last 10–12 years, considerable research has been done on the metabolic behavior and movement of fission products through the food chain to end up in the body of man. Suffice it to say that our knowledge of this area is reasonably adequate, coming from extensive experimental studies, collections and analyses of foodstuffs, and collections and analyses of thousands of human bones. Thus, there is reliable information on amounts of radionuclides present in the human population from the weapons tests completed in 1958.

Biological Effects of Radiation

The term radiation refers to the transport of energy without a material carrier, such as radiation of light rays from sun to earth, from desk lamp to desk. The type of radiation we are concerned with is of a special kind, called ionizing radiation, which has relatively large amounts of energy and has characteristic effects on biological materials. For example, if one drinks a cup of hot tea, a certain amount of thermal energy is transferred to the body as the tea cools down and reaches body temperature; that same amount of energy, delivered in the form of ionizing radiation, would end a person's tea-drinking days forever.

because only the unpaired ions are effective as carrier scatterers, the neutral pairs being ineffective. As the temperature was increased the pairs began to dissociate, and, at the temperature where both samples had the same mobility, the 50% pairing point was reached, the (unpaired) ion concentrations then being necessarily equal for the two samples. Using (13) and (14) a value for a could then be calculated. For samples with two different gallium concentrations the values of a

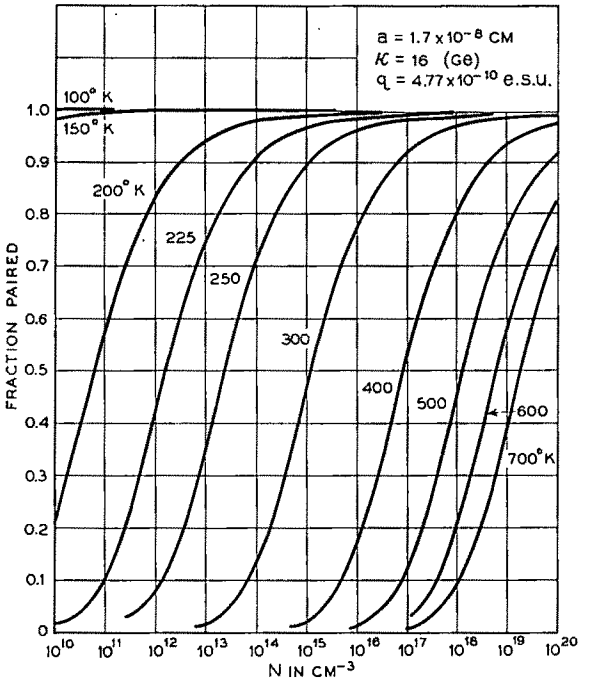


FIG. 5. Fraction paired vs. concentration for several temperatures. Donor and acceptor concentrations were assumed to be equal, and the values for the distance of closest approach and dielectric constant were chosen as shown.

determined in this way were in good agreement, as indicated; they also are close to the figure for the sum of the ionic radius of Li^+ and the tetrahedral covalent radius of gallium, 1.86Å.

The fraction of ions paired as a function of concentration for a number of temperatures and assuming a to be 1.7Å, is shown in Figure 5. The donor and acceptor concentrations are taken to be equal in this plot. It can be seen that a high degree of pairing is to be expected over a wide range of experimental conditions.

A number of other aspects of ion-pairing were also studied. For example, pair formation was shown to increase the solubility of Li in much the same manner as the hole-electron equilibrium discussed

To discuss radiation it is necessary to deal with amounts, and therefore some sort of unit is needed. To keep matters as uncomplicated as possible, only one unit will be used, the Roentgen, named after the discoverer of the X-ray. The roentgen is based on ionization produced and can be taken to represent radiation dose absorbed in tissue. This is convenient because biological effects depend primarily on the amount of energy that is dissipated in the tissue and are governed closely by the amount of ionization. As far as we are concerned, it is important only to know what the unit means in terms of the effects of interest. For example, few of us remember what a volt is, but although we don't worry about shock from an electric train (6–10 volts), we would not change a tube in a plugged-in radio while sitting in a bathtub; and we know full well what would happen if we tangled with a high-voltage line (over 10,000 volts). To illustrate the practical meaning of the roentgen unit, consider two examples: Whole body exposure of man to a dose of about one roentgen produces no observable biochemical or clinical effect; about 400 roentgens will kill about half the individuals exposed. Later, more specific effects will be mentioned.

The whole question of biological effects is extremely complicated and space does not permit full discussion. To understand the problems posed by radiation from nuclear weapons it is essential, however, to mention three general principles.

I. Radiation effects can be classified as either genetic or somatic. The genetic effects of radiation are those which affect offspring conceived after the exposure of individuals. The effects of radiation upon the exposed individuals themselves, such as the production of leukemia or bone tumors, or the shortening of life-span—these are called somatic effects.

II. A given radiation exposure, delivered over a long period of time, produces less biological effect than when delivered over a short period of time. For example, a man exposed to a single dose of 600 roentgens of radiation over a lifetime would show effects that were scarcely, if at all, observable. This behavior comes about because of the body's capacity to recover in part from the radiation, if given time. There is recent evidence that the genetic response may also be decreased when a given radiation dosage is spread over a long period of time. Thus, if a linear extrapolation to low dose rates is made, the predicted effects will always be in error on the side of safety. This principle is of especial importance in the evaluation of effects from world-wide contamination that is built into our bodies and delivers its radiation dose over a lifetime.

III. Is there a level of radiation exposure below which no harm is caused? This is called the "threshold" question. In many studies of the effects of drugs and chemical agents, in addition to radiation, one usually gives increasing doses of the agent and measures the effect. For many substances one finds that the effect as measured is zero until a certain

earlier, and the formation of pairs was found to lower the diffusion constant of Li^+ in germanium, as only the Li^+ ions which are unpaired at any given time are able to diffuse. The kinetics of the ion-pairing process were also examined in detail, by following the conductivity of samples which were initially far from equilibrium as a function of time. For this process the relaxation time, τ, which is the inverse of the rate constant, can be determined from the expression

$$\log (\sigma_\infty - \sigma_t) = \text{constant} + \frac{t}{\tau}$$

where σ_∞ and σ_t are the conductivities at infinite time (when the pairing reaches its equilibrium value) and at time t. Figure 6 shows one

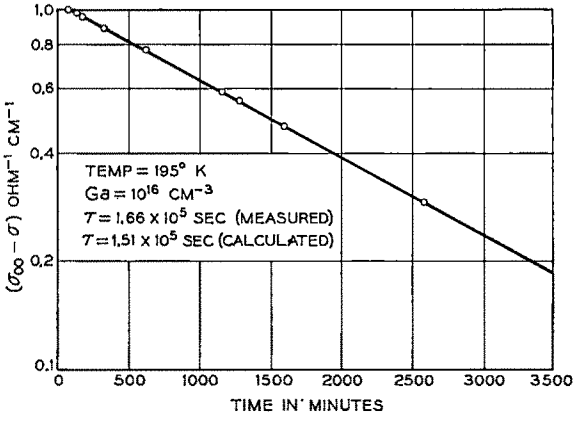


FIG. 6. Ion-pairing relaxation.

such measurement, and the value of τ determined in this way. Also, τ may be calculated from the theory of diffusion-controlled reactions, according to which, for this geometry

$$\tau = \frac{\kappa kT}{4\pi q^2 [\text{Li}]_0 D} \tag{16}$$

where $[Li]_0$ is the value of [Li] at t = 0 (when there was no pairing), and D is the diffusion coefficient for Li^+. The values for τ calculated in this way were in good agreement with the measured values, as shown for the example in Figure 6.

Nonstoichiometry

A very important but complex category of chemical equilibria is involved in determining the composition of binary semiconductors, especially the compounds formed from elements in Groups II and VI of the Periodic Table. These materials characteristically show nonstoichiometry, and the lattice defects which result are generally electrically

dose, which is called the threshold, is reached. Theoretically, any dose lower than threshold would be expected to cause no effect. It must be emphasized, however, that such behavior is entirely dependent upon the sensitivity of the methods for detecting the effect. For example, as more sensitive methods become available, the threshold dose might well be lowered, even approach zero. Where the population of the world is exposed by dissemination of radioactivity from nuclear weapons, then tremendous numbers of individuals are involved which certainly increases the sensitivity of observation. There is general agreement that genetic effects show a non-threshold behavior curve—that is, that there is no radiation dose below which genetic effects would not be observed. There is no agreement as to whether somatic effects are threshold or non-threshold in behavior. Until there is definite evidence, we must be prudent and err on the side of safety by assuming that any amount of radiation will produce some measure of harm.

Our next task is to see what are the best estimates of how much harm results from given exposures to radiation. It is convenient to classify radiation effects either as high-level, above 50 roentgens, or low-level with especial interest below about one roentgen. Evaluation of high-level radiation effects is not particularly difficult since experiments can be done with animals, and observations are available from human beings, either accidentally exposed or treated medically.

Evaluation of the effects of radiation at low levels, especially when the exposure is over long periods of time, is much more difficult. There is no direct evidence on the effects of radiation under conditions of low-level, long-term exposure. The reason is simply that, under these conditions, experimental studies are practically impossible, requiring vast numbers of experimental animals (millions), long periods of time (decades), and sensitive criteria of damage.

The most useful approach to evaluation of low-level, long-term exposures is to consider the background radiation to which mankind has always been exposed, and simply to assume that additional exposure to radiation at about the same level as background will produce about the same harm, and always to recognize that the more we increase our exposure over background, the more uncertain are we about the effects. Essentially, this means that if any amount of radiation produces harm, then the radiation from our natural environment, background radiation, must have been producing harm to mankind through the centuries, and perhaps can serve as our best yardstick.

Since the beginning of time man has been exposed to external radiation from cosmic rays and from naturally-occurring radioactive materials such as uranium, thorium and potassium 40 in the ground, air and structures in which he lives and works. Man has also been exposed to internal radiation from radionuclides that occur universally in the body, for ex-

active, serving as donors and acceptors in much the same manner as foreign impurity atoms do. For example, a cation vacancy can act as an acceptor, and an anion vacancy can act as a donor. The composition of the crystal is determined by the partial pressures, during the crystal growth, of not only foreign impurities but also the constituents of the compound itself; alternatively, this composition can be effected by

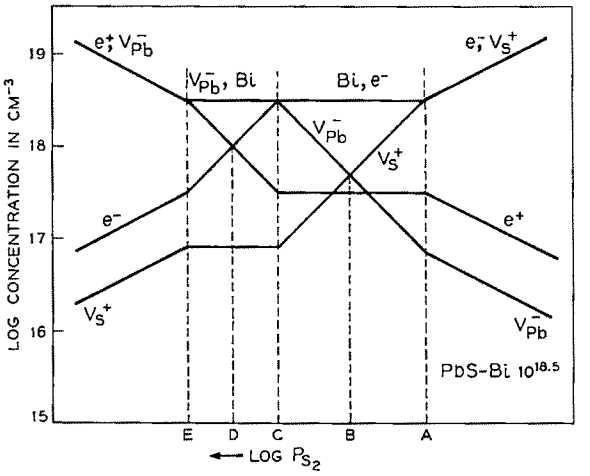


FIG. 7. Calculated compositional diagram for lead sulfide, containing bismuth, showing concentrations of the various species as a function of sulfur pressure.

bringing the crystal to equilibrium with an external phase after its growth.

Compositional studies have been carried out for several oxides and sulfides in an extensive series of investigations by the Philips workers in Holland, in particular. One such study, by Bloem [10], was of the lead sulfide, PbS, system. In this work, equilibrium between the solid and a gas phase containing hydrogen sulfide, H_2S, was established, with the following equilibria describing the system for the case of pure lead sulfide.

$$\left.\begin{array}{l} PbS \rightleftharpoons Pb_{(ext.)} \times \tfrac{1}{2}S_{2(ext.)} \\ \tfrac{1}{2}S_{2(ext.)} \rightleftharpoons S_S + V_{Pb}^- + e^+ \\ Pb_{(ext.)} \rightleftharpoons Pb_{Pb} + V_S^+ + e^- \\ PbS \rightleftharpoons V_{Pb}^- + V_S^+ \end{array}\right\} \qquad (17)$$

where the subscripts refer to the external phase and to lead and sulfur lattice sites. Thus S_S is a sulfur atom on a normal sulfur site, V_{Pb} is a vacancy on a lead site, etc. When foreign impurities are also present, such as bismuth or silver, then additional equilibria need also to be taken into account, e.g.

$$Bi_{(ext.)} \rightleftharpoons Bi_{Pb}^+ + e^- \qquad (18)$$

ample, potassium 40, carbon 14, and members of the radium and thorium families. It is estimated that the typical radiation dosage from such natural sources is about one-tenth of a roentgen per year, amounting then to about seven roentgens over a standard 70-year lifetime. It must be pointed out that, in some areas of the world, in parts of India and Brazil, for example, the natural radiation exposure may be up to ten times higher than this. Population studies are being done to see if any effects can be detected. Such studies are most difficult, however, because one needs to have controlled populations for comparison: populations that have the same standard and way of living, the same nutritional plane, the same medical care, the same degree of inbreeding. As yet, in these areas there is no evidence that the backgrounds ten times higher than average are deleterious, but this does not mean that there are no affected individuals. It does mean that the number of individuals affected is too small to be detected.

Let us now consider the relationship between radiation exposure and harm to the population. For reasons of clarity, the discussion is limited to the two situations most likely to be encountered—an exposure similar to that of background resulting from world-wide fallout, and a single exposure at high levels as from local fallout. The two most important radiation effects are the production of leukemia and genetic defects; others are of much less concern.

Biological Effects from World-wide Fallout

Nuclear testing was stopped in the fall of 1958 and, with the exception of the French tests which did not contribute significantly to environmental contamination, there was no testing until the Russian series of 1961. There are reliable data on the present levels of radioactivity in the human population that have resulted from the tests through 1958. If one adds up all of the dosages from the radioactive materials, including carbon 14, it appears that the population exposure from world-wide fallout is about five one-thousandths of a roentgen per year—in other words, about five per cent of background.

In the Western countries, exposure for medical purposes is about equal to that from background and therefore amounts to about 50% of total exposure. The medical profession is doing all it can to keep exposure to a minimum and it must be remembered that the individual who receives medical exposure expects to benefit from it. Fallout and miscellaneous peacetime uses such as from watches and luminous buttons, etc., power reactors and radioisotope usage, television, each contribute some two to three per cent of total exposure.

Consequences expressed in numbers have been conservatively estimated and presented only to provide some idea of the magnitudes involved. It is emphasized that uncertainties are large.

The mass action expressions for the various equilibria involved, plus the charge neutrality condition for the crystal as a whole, suffice to determine the concentrations of all the species present as a function of the partial pressure of the components in the external phase, at least in principle. A graphical solution obtained for one assumed case, with a constant bismuth concentration of $10^{18.5}$ is shown in Figure 7. Thus, at very high sulfur pressures, it can be seen from (17) that $[V_{Pb}^{-}] = [e^{+}]$, and both are much greater than $[V_S^{-}]$ and $[e^{-}]$, so that the material is p-type with $[e^{+}]$ varying as the fourth root of the sulfur pressure. Correspondingly, at very low sulfur pressures, the material would be expected to be n-type, with $[e^{-}]$ varying inversely with the fourth root of the sulfur pressure. At intermediate sulfur pressures the presence of the bismuth becomes important; in the lower part of this range $[e^{-}$

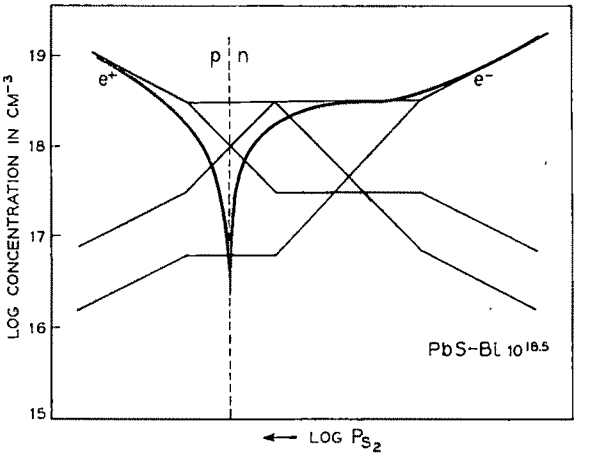


FIG. 8. Observed electron and hole concentrations in lead sulfide (heavy line) compared with calculated compositional diagram of Fig. 7 (light lines).

would be constant and equal to the bismuth concentration, reaching a region at somewhat higher pressures where the transition to p-type occurs. The changes in the other quantities may be explained in a similar way. From an experimental point of view it has not been possible in any real case to determine all the quantities in compositional diagrams like that in Figure 7. What can be done, generally, is to determine the carrier concentration as a function of the external pressure. This has been done for the PbS-Bi composition shown in Figure 7, and the values found are shown by the heavy line in Figure 8. The light lines refer to the calculations of Figure 7, and it can be seen that there is reasonable agreement. An important consequence of such calculations is the awareness it gives of the concentrations of the various species present in a complex system, in terms of the conditions for the preparation of the

The natural incidence of genetic defects in the world population is about four per cent, which means that each year there are about three million genetic disabilities of which perhaps one million are due to background radiation. Testing through 1958 may have added as few as 2500 or as many as 100,000 cases over subsequent years. Genetic defects may be of all kinds: The innocuous defect such as the early death of an embryo that passes unnoticed; varying degrees of impaired vigor and fertility; and the worst type of defect that causes suffering but does not interfere with reproduction.

Out of a world population of three billion the natural incidence of leukemia is about 150,000 cases per year, of which some 1500 cases per year are estimated to result from background radiation. Testing through 1958 has probably added a further 75; the number will decrease with time, since leukemia incidence falls off at four to seven years after radiation exposure. Bone tumors are less frequent in occurrence than leukemia, and less liable to production by radiation.

For those not accustomed to thinking in terms of population statistics two examples are cited for comparison. In the United States alone, about 40,000 people a year are killed on the highways—a biological cost we accept for transportation. In peacetime military pursuits there are about 1400 accidental deaths per year—a biological cost for preparedness. These figures are not quoted to justify any unnecessary additions to individual suffering, but merely to emphasize that normally we do not have personal anxiety about such risks.

From a moral standpoint, we as individuals or as a nation would not want to undertake any action that would unnecessarily cause even one instance of human suffering in the world population. Realistically, we know that man has always been faced with the protection of his well-being from natural and man-made changes in his environment and circumstances. Each step forward, for example, the use of fire, electricity, transportation, medical practice, has been a great boon to humanity, while at the same time causing individual hardship that in no sense of the word is preventable. Man developed and needs fire and heat, but individuals die from fires and boiler explosions. In medical practice, uncounted lives are saved by antibiotics, but individuals have allergies and die from antibiotics. This point certainly needs no further belaboring. Even the controlled peaceful uses of atomic energy will produce some radiation exposure, and therefore by our standards will have some biological cost. But everyone, I am sure, would agree that these biological costs are far outweighed by the benefits to mankind.

As far as nuclear testing is concerned, there is no question but that it has contributed in a small way to world-wide radiation exposure, and that we should therefore take the position that further production of uncontrolled radioactive contamination should not be undertaken unless this

crystal. It is clear that the complex electrical behavior of such a material can only be interpreted properly in terms of the detailed chemistry of the foreign impurities and lattice defects.

Nonequilibrium Processes

Up to this point we have been mainly concerned with chemical equilibria in semiconductors. The kinetics of chemical processes in the solid are also amenable to investigation; we saw an example of this in the discussion of the kinetics of ion-pairing. Another kind of diffusion-controlled reaction in solids is the precipitation process from a supersaturated solution in the solid, with the formation of a second phase.

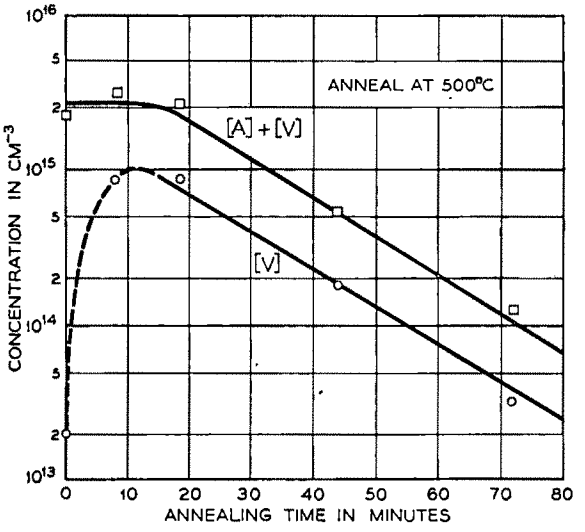


FIG. 9. Concentration of nickel acceptors and vacancy acceptors as a function of time, in germanium.

Studies of this kind can yield information about the behavior of both chemical and structural imperfections in solids; two examples will be mentioned.

The precipitation of nickel in germanium has been studied by Penning[111], who used Hall effect measurements to follow the concentration of the unprecipitated nickel as a function of time. Nickel is ordinarily an acceptor in germanium in substitutional positions on the germanium lattice sites, but it can also move to an interstitial position, where its diffusivity is much higher and it is electrically inactive. The precipitation results from this "dissociative" diffusion mechanism, the nickel first jumping from a substitutional to an interstitial position, then diffusing to a dislocation where the precipitation occurs. Initially, then, all the nickel is on substitutional sites; when the dissociation step occurs, an

be unequivocally justifiable. In point of fact, it would seem that the decision to undertake further atmospheric testing by this country should be based primarily upon the military, political, propaganda or disarmament benefits to be obtained. At the present stage of affairs, it appears in actuality that the biological effects of further testing would be of minor consideration.

There is interest, of course, in what one might expect from the 1961 Russian series of tests. Through 1958 there had been put into the atmosphere about 100 megaton equivalents of fission products, and this produced roughly a five per cent increase of radiation exposure over background. From what we know of the 1961 Russian tests, which put up about 25 megatons of fission, it is anticipated that the increase from them would be considerably less than another five per cent. One may well wonder, if we are not worried about the tests up through 1958, about the Russian tests in 1961, or about another series by the United States, whether there is a point at which we should worry. The answer is, of course, yes, but settling on a given level is difficult because each country will have its own concept of benefit *vs.* cost. It is certainly to be hoped that national self-interest, if nothing else, will dictate control on an international basis.

Biological Effects of High Radiation Exposure

What is expected to happen when populations are exposed to high levels of radiation delivered over short times? Practically all persons exposed to more than 800 to 1000 roentgens will die. About fifty per cent of persons exposed to 400 roentgens will die. If pregnant women were exposed to as little as 30 roentgens there would be expected a high percentage of malformations of the newborn, depending upon the age of the unborn child at exposure.

In regard to fertility, the radiation effects are similar for both men and women. About 150 roentgens would induce brief, temporary subfertility; 250 roentgens might induce temporary sterility for one or two years; and 500–600 roentgens might well produce permanent sterility.

In regard to leukemia, it is estimated that each 100 roentgens of radiation will cause about 100 cases of leukemia per million people per year; the normal incidence is 50 cases per million people per year.

In human beings, genetic effects are difficult to evaluate since no direct observations are easily available. Social conditions interact with biological phenomena; for instance, medical care maintains genetically handicapped individuals who may reproduce themselves and transmit deleterious genes, and public health has reduced infant mortality, one of the major selective agents. Can genetic radiation damage lead to the extinction of the human species? Based on present knowledge of radiation and population genetics this is most unlikely; the levels of radiation

acceptor disappears and a vacancy is formed. Vacancies in germanium, however, also act as acceptors, but the Hall measurement can distinguish between these and the nickel acceptors because the ionization energy of the vacancies is very much less. The experimental results are shown in Figure 9. The total acceptor concentration is the sum of the vacancy concentration $[V]$ and the Ni-acceptor concentration $[A]$, and this total remains constant through the first stages of the precipitation

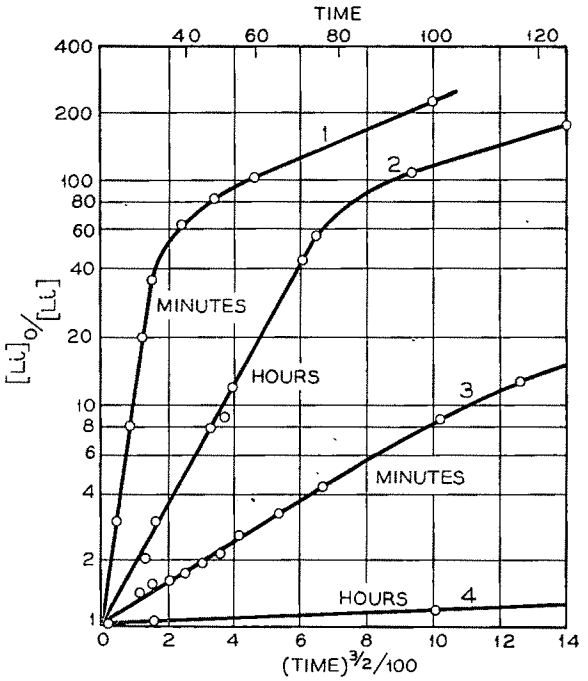


FIG. 10. Precipitation of lithium in germanium. Curves represent samples with different concentrations of nucleation centers.

because for every nickel acceptor lost by the dissociation process an acceptor vacancy is formed. The value of $[V]$ increases during this period. At longer times, the vacancies are lost as they diffuse to dislocations where they are annihilated, and a decline in $[V]$ sets in. Simple rate equations may be written to describe this model. The rate of change of acceptor nickel concentration is given by the difference in the rates of the dissociation step, proportional to $[A]$, and the recombination of interstitials and vacancies to form acceptor nickel,

$$\frac{d[A]}{dt} = -k_1[A] + k_2[I][V] \tag{19}$$

where $[I]$ is the interstitial nickel concentration. The rate of change of

needed to cause genetic extinction would produce obliteration of humanity by direct damage.

Some Present-day Problems

Since human exposure to world-wide fallout is primarily by food, a matter of primary interest is whether there is control of the food that we can buy and whether any upper limits have been set for contamination which, when exceeded, would be reason for concern and action. Levels of radioactivity are being watched very carefully by Federal agencies, especially the Public Health Service, and the categorical statement may be made that, if one can buy a food on the market, it may be eaten and enjoyed. The Government has established a body known as the Federal Radiation Council which is setting guide-lines in regard to levels of radioactive contamination in food. For example, this body has indicated that a daily intake, averaged annually, of 0 to 20 micromicrocuries of strontium 90 requires no consideration; if the intake should fall between 20 and 200 micromicrocuries, then quantitative surveillance and some routine control might be necessary; at intakes between 200 and 2000, action would be called for. (The micromicrocurie is a unit for amounts of radioactivity.)

In 1959 and 1960, when strontium 90 levels were at their highest from the tests carried out prior to and including 1958, the average dietary intake in the United States was about 15 micromicrocuries of strontium 90. It has now fallen to about nine. Next spring, we can expect to see the effects of the Russian tests and it is estimated that the new levels will be about the same as those reached following the cessation of testing in 1958.

There has been considerable publicity about radioactive iodine in milk during September and October of 1961, the main point being that the thyroids of children appear to be especially sensitive. So far the levels observed have warranted no action.

The question is often asked as to whether we should modify our diet or food technology to decrease somehow the intake of radioactive contamination, with suggestions being made to supplement diets with stable calcium which theoretically reduces strontium retention, or to give iodine drops to block thyroid uptake, or perhaps to reduce milk intake. The answer at present is categorically no. The chance of doing harm by individual and perhaps misguided modification of diets far outweighs the chance of doing any good. Paradoxically, for example, even though milk is the single largest source of strontium 90 in the diet, reducing the milk intake tends to increase the body burden of strontium to be developed. This is because milk calcium contains less Sr^{90} than does plant calcium, since the cow uses calcium more effectively than it does strontium. If the diet were to contain no milk, then the body would get all its calcium from

vacancy concentration includes these terms and also one proportional to $[V]$ which describes the annihilation of vacancies at dislocations. Letting G represent this proportionality factor,

$$\frac{d[V]}{dt} = k_1[A] - k_2[I][V] - G[V] \tag{20}$$

The solution of these two equations gives expressions for $[V]$ and $[A]+[V]$ as a function of time which have the form shown in Figure 9, thus describing the experimental results satisfactorily.

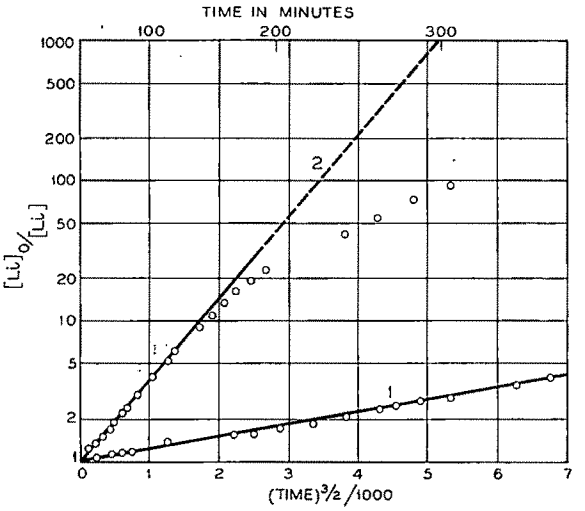


FIG. 11. Precipitation of lithium in germanium. Sample 2 was precipitated at 60°C., sample 1 at 25°C.

Another precipitation system which has been investigated in considerable detail is lithium in both germanium and silicon. Experiments in germanium were carried out by Morin and Reiss [12], who used samples with a supersaturation ratio of more than 10^3, using conductivity measurements to follow the precipitation with time. Figure 10 shows typical results, with the ratio of the lithium concentration at time zero to that at time t being plotted against $t^{3/2}$. The dependence on the three-halves power of time is typical of a diffusion-controlled reaction in which the diffusion is to nuclei with a spherical symmetry, with no new nuclei being formed during the precipitation. The solution of the differential equation describing the system can be written, for all but the final stage of the precipitation, as

$$[\mathrm{Li}]_0/[\mathrm{Li}] = \exp(\text{const. x } N\, D^{3/2}\, t^{3/2}) \tag{21}$$

where $[\mathrm{Li}]_0$ is the concentration at time $t = 0$, $[\mathrm{Li}]$ is that at time t, D is the diffusion coefficient and N is the concentration of nucleation cen-

plant sources, which are more highly contaminated. If it were not for milk, our bodies would contain three to five times more strontium 90 than at present.

Another question often raised is whether or not consideration is being given to the decontamination of land and of foodstuffs. Such decontamination measures, or as they are now called, remedial measures, are under intensive scientific investigation. Again it must be emphasized that remedial measures should be taken only when declared necessary by responsible authorities. They are mentioned here as a matter of information, since none is practical and in any event such measures would only be considered under conditions of serious accident or warfare. Remedial measures being investigated for decontamination of land are as follows:

1. Removal of recently-contaminated grass or crops;
2. Scraping off several inches of surface soil;
3. Leaching with large quantities of solvents that would move strontium down the soil profile;
4. Liming of acid soils to reduce the strontium content or the strontium-to-calcium ratio in the crop;
5. Deep ploughing to reduce the uptake of radioactive contamination by shallow-rooted grasses;
6. Using deep-rooted plants;
7. Using land for plants with low ratio of calcium-to-calories or for fiber.

Plant foods could be reduced in contamination by removing the bran from cereal products and by washing or skinning fruits and vegetables. Remedial measures for animals would consist of avoidance of surface-contaminated herbage and supplying uncontaminated calcium.

Considerable attention is being given to removal of strontium 90 and cesium 137 from milk by use of ion exchange resins. The procedure is technically feasible but as yet could not be used on a large scale without greatly increasing the cost and requiring considerable installation time and capital investment. It is emphasized that there would be greater probabilities of harm from nutritional and health problems created by attempts to decontaminate milk on a large scale, than from present levels of radioactivity. Any food problems with iodine 131 can best be handled by storage for appropriate times before use, as for example the use of canned milk.

Changes in man's diet, as already indicated, might include use of uncontaminated calcium and reservation of less contaminated food for segments of the population at greatest risk, such as children and pregnant women.

Nuclear Warfare

Since there is no way of knowing the scale and pattern of attack, only non-educated guesses are possible. No attempt will be made to draw a word picture of the chaos that would ensue; none of us has a vivid enough imagination. It must be emphasized that our frames of reference would

ters. The various curves in Figure 10 represent samples with different values of N. The effect of D is shown in Figure 11, which shows the results for two identical specimens, one being precipitated at 25°C., the other at 60°C. The ratio of the slopes is equal to the ratio of $D^{3/2}$ at the two temperatures within experimental error, as it should be according to Equation (21).

In the case of silicon, the precipitation process was found by Pell [13] to be similar in samples with very low oxygen concentration. Silicon often contains large amounts of oxygen, however, and he found a quite

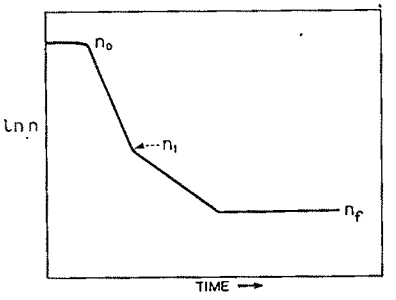


FIG. 12. Electron concentration (n) vs. time for lithium precipitation from silicon containing oxygen.

different behavior in this case. The electron concentration as a function of time is shown schematically in Figure 12. This was explained in terms of a model which assumes an interaction between the lithium and the dissolved oxygen to form a new species, LiO, which also acts as a donor

$$Li^+ + O \rightleftharpoons LiO^+ \qquad (22)$$

In the early stages, nearly all the oxygen has a lithium attached, equilibrium (22) being far to the right. When the lithium concentration drops to a value (n_1) equal to the oxygen content of the sample, however, a break in the rate of precipitation occurs, as the rate is now limited by the equilibrium (22) which is driven to the left to provide lithium for the continuation of the precipitation. The equilibrium constant for this reaction (22) is such that, at equilibrium, $[LiO]/[O] \approx 0.02$, so that a second break in the curve occurs at $n_f = 0.02\, n_1$, with no further percipitation occurring; essentially all the electrons at this stage come from the donor LiO, which has now reached its equilibrium value.

Conclusion

Semiconductors furnish an advantageous system for the study of chemical interactions in solids, a few examples of which have been described. Their unique electrical properties not only provide an easy, non-

be changed completely. Previously established acceptable levels of radiation would have no meaning whatsoever. The hungry man who has to decide between starvation as against increasing his chances of developing cancer in 10 or 20 years will make that decision during the split second between the time his hand touches the food and his mouth opens to receive it.

Within close range of a surface explosion, blast and heat would be the major destructive forces and any survivors would also be at risk from external radiation. Intense fallout would cover up to hundreds of square miles nearby and persons not sheltered would be exposed to most hazardous intensities of radiation. Outside this area, there would be another zone up to thousands of square miles where a proportion of those not sheltered would be at serious risk. In areas untouched by "local fallout" the risk from radiation would be slight.

Large weapons, if used, would probably be exploded at high altitudes and this would increase the areas of fire damage (perhaps to a 50-mile radius for 100 megatons), but would decrease the areas subject to local fallout. Under these conditions, future food production would not be complicated by radioactive contamination although other ecological factors might become important. For example, there could be unchecked fires over large areas of forests and grasslands followed by spring thaws that would erode denuded slopes and flood the valleys; intensification of plant and animal disease might be expected.

If weapons were exploded so as to maximize local fallout, then future food production would depend on how much arable land was left reasonably uncontaminated. The fact that we have as much as a million or so square miles of agricultural land makes it difficult to conceive of any attack that would leave us without sufficient usable land. The major short-term problems would most likely be those of logistics and transport. Theoretical studies on the basis of attacks that were credible in past months and years indicate that life and food production would go on. For example, the latest study with which I am familiar indicated that an attack of about 1400 megatons would leave 70 per cent of our cropland usable and 90 per cent of our livestock alive. But the capability for destruction increases with time and predictions are meaningless.

Could then the North American man survive a nuclear attack? Certainly individuals would. Whether the survival would be 5 or 95 per cent would depend on what the enemy can do and wants to do, and to a lesser degree on defensive preparedness. Whether the North American man could be well-adjusted and happy, I leave to the psychologists; whether he would again assume world leadership, I leave to the geopoliticians.

Let us now consider the situation in the areas of the world that receive world-wide fallout only. If we assume, as a point of departure, that a total of 20,000 megatons would be used in the war, and that this would

destructive method for examining the kinetics and equilibria of these reactions, but also lead to a number of subtle chemical effects which result from the effect of the hole-electron equilibrium on the mass action law governing the chemical equilibria.

Acknowledgment: The author is indebted to C. S. Fuller for comments on the manuscript.

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involve 10,000 megatons of fission, we can estimate what the mean population exposure might be. It is recalled that previous weapon tests of 100 megatons of fission gave a radiation exposure equivalent to about five per cent of natural background. This means that 10,000 megatons would increase the radiation exposure to about five times that of background, assuming the same pattern of detonations as in past tests. The exposure in the Southern hemisphere would be much less. Thus, life in the rest of the world would, to all intents and purposes, go on as usual as far as radiation damage is concerned. The greater the number of megatons, however, the greater the load of distress and suffering that individuals and all human societies would have to support.

Another Generality

It has been argued that nations disappear from the world or from world leadership for one of two reasons: (*a*) the fight to achieve supremacy, or (*b*) the desire for an easy life. I have not noted any great urge to fight for supremacy. There does seem to be a desire for the easy life. . . . schooling with frills, easy programs and long vacations; spectator sports; bright youngsters who don't get to college, dull ones who do; the drive to shorten the work week, etc. These examples are not cited in criticism of any group or to belittle our great national achievements. Rather, the point is made that we have a tremendous and literally untapped reserve of creative manpower which, when coupled with productive efficiency, should permit us to accomplish whatever is needed. If we want to badly enough, we can make the sacrifices and put forth the effort to mount simultaneously a deterrent, a peace offensive, a coexistence offensive, a world leadership offensive, a space offensive, a health and education offensive, a civil defensive—whatever it's going to take to keep us out of nuclear war and to allow us to fulfill the responsibilities of world leadership.

Summary

1. Any use of nuclear energy or radiation involves a biological cost which must always be weighed against the benefits expected.

2. Controlled peacetime use of nuclear energy yields benefits far in excess of biological costs.

3. The biological cost of past weapons tests of about 100–150 megatons of fission is small enough so that individuals need have no anxiety about personal or family well-being.

4. The factor of biological cost is minor in reaching decisions about resumption of atmospheric weapons tests; other factors—military, strategic, political, propaganda—are much more important.

5. Full scale nuclear warfare means mutual destruction, certainly of highly organized social systems, although all life would most probably

RADIO ASTRONOMY AND COSMOLOGY*

By MARTIN RYLE

ASTRONOMY differs from most sciences in that we cannot do experiments; the astronomer must build up from his existing observations a picture or "model" of the Universe, and then look for further effects which should be observable if his model were correct. Although in 700 B.C. the Babylonians were making excellent observations without attempting to interpret them, and although the Greeks proposed models without always looking too carefully into how well they fitted the facts, this approach has always been present and in the 17th century led, through Kepler and Newton, to a good understanding of the solar system.

But what were the stars? Kepler had no conception of their great distance, and imagined them to be on the surface of a fixed sphere with a radius some 2000 times that of the earth's orbit. But it seems certain that Newton had, when he wrote:—

> "But if the matter were evenly disposed throughout an infinite space, some of it would convene into one mass, and some into another, so as to make an infinite number of great masses scattered great distances from one another throughout all that infinite space. And thus might the Sun and fixed stars be formed, supposing the matter were of a lucid nature."

We can see here the first idea of an *infinite* Universe, in which the Sun has no more significance than any of the stars.

There now began the exploration of space on an ever-increasing scale. William Herschel, by constructing larger telescopes than had ever been built before, was able to show that the diffuse band of light of the Milky Way was in fact due to countless stars, and he thus confirmed Newton's suggestion of the insignificance of the sun. Today we know that our own Galaxy, the Milky Way system, is a flattened disc containing some 100,000 million stars each comparable in size and luminosity with the Sun.

But the observations of William Herschel and later his son John showed, besides the millions of star images, some thousands of diffuse sources of light or "nebulae." Some of these are now known to be clouds of luminous gas in our Milky Way system, but even at that time the Herschels considered the possibility that these sources were themselves distant "island Universes" too far away for any of the stars to be resolved individually. It was not until methods of distance measurement had been established in the present century that these suggestions could be confirmed, showing an apparently limitless Universe containing, at great separations, compact islands of stars, or galaxies, of which the

* This article by Professor Ryle is reprinted with his permission and that of the Royal Institution of Great Britain. It first appeared in the Proceedings of the Royal Institution, *38*, No. 173, 439 (1961).

not be obliterated in the countries attacked, and the rest of the world might go essentially unscathed. Recognition of these facts by rational men is the hope for prevention of nuclear war.

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Milky Way is one.

Besides showing the insignificance of the earth, these observations raised new problems. One of these, first propounded by the German astronomer Olbers, is based on an observation which any of us can make—that it gets dark at night.

If we look at the sky we see that between the stars it is dark; a larger telescope will see fainter stars here, but again it is dark between them. Now if we must suppose that the Universe is infinite in extent, then in whatever direction we look we will eventually see the surface of a star. Although the intensity from each star individually will be very weak, since they must occur in every direction, the sky should have the same brightness as the surface of the sun: but if this were the case the earth would be heated to 6000° and would immediately be vaporized.

We cannot avoid the difficulty by supposing that the light from distant stars is absorbed; by the very fact of absorbing the light the obscuring matter will be heated until it has the same temperature as the stars—when it will radiate as much light as it absorbs.

It seems that we can only explain this paradox by supposing that the Universe is not infinite—but if we ignore the local stars in our own Milky Way system, we find that the sky is uniformly dark in all directions, and we should have to return to a pre-Copernican idea of a Universe constructed with us at its centre, having in the meantime demonstrated our complete insignificance.

The discovery which eventually led to the solution of the problem was made by the American astronomer Slipher, in 1912. Slipher, who was studying the spectra of the stars in distant galaxies, found that, for a particular galaxy, the spectral lines were displaced slightly, and all occurred on a slightly longer wavelength than expected. This displacement of the lines toward the red end of the spectrum, the so-called "red shift," is just what would be expected if the light were emitted from a moving source, just as the pitch of a passing train whistle appears to decrease after it has passed us. His observations suggested that not only this galaxy but also a number of others were all moving away from us, and later work led, in 1928, to the conclusion that no matter in what direction one looked, the galaxies were traveling away from us; it was also found that the speed of recession was greatest for the most distant galaxies.

We can now see an explanation for the darkness of the night sky; the light we receive from a very distant galaxy will have suffered such a large red-shift, that most of its energy now falls in the infrared; any visible light received must be emitted in the ultra-violet, where the energy emitted is very small. As a result the light from galaxies beyond a certain distance will be very small, and we have in effect produced a limit to the size of the Universe we can see.

PERSPECTIVES

THE NEW WORLD, 1939/1946

By HUGH TAYLOR

THE United States Atomic Energy Commission has undertaken to provide a perspective on The New World which was ushered in during the first days of 1939 when Niels Bohr, arriving in New York on the liner *Drottningholm*, was met at the pier by Enrico and Laura Fermi. Bohr was disturbed by the threat of war in Europe, by his knowledge of a recent scientific discovery of revolutionary implications by Otto Hahn and Fritz Strassmann, and by Lise Meitner's and Otto R. Frisch's conclusion that a new type of nuclear reaction had taken place—fission. Laboratory results communicated by Frisch to Bohr by telegram confirmed the conclusion. An authoritative account by Bohr was carried in the *Physical Review* of February 15, 1939, with corroborating experiments from research centers in this country. Bohr and John A. Wheeler worked out "The Mechanism of Nuclear Fission" in the realm of theory by September 1939 but already the distinguished group of experimental physicists at Morningside Heights in Columbia University were at work on the practical studies—Dunning, Fermi, Leo Szilard, a Hungarian scientist, Walter H. Zinn, Herbert L. Anderson, a gifted graduate student, members of a team, fortunately "under a sympathetic if somewhat conservative administrator, George B. Pegram," a physicist himself and Dean of the Graduate Faculties. That was how The New World began. The whole world knew about what had been achieved since 1939 when the U-235 projectile exploded at Hiroshima on August 6, 1945 and the plutonium bomb on Nagasaki a few days later. Einstein's equation $E = mc^2$ had been confirmed in formidable fashion.

Volume 1, *A History of the United States Atomic Energy Commission* has been written jointly by Dr. Richard G. Hewlett, AEC chief historian and Dr. Oscar E. Anderson, Jr., a professional historian assigned to the history project. In their four years of preparation and writing they worked closely with the AEC Historical Advisory Committee, headed by Dr. James P. Baxter, III, President-Emeritus of Williams College, a former Pulitzer Prize winner in history. Volume I was published by The Pennsylvania State University Press, at University Park, Pa., on March 30, 1962. It contains 766 pages with 57 photographs and is very modestly priced at $5.50.

Although we have found an explanation for the observations, we have had to introduce the idea of a Universe changing with time, and if we relate the rates of recession of galaxies at different distances, we must conclude that all those which we now see must have been crowded close together about 10,000 million years ago. This "age of the Universe" is little more than twice the age of the earth as indicated by the radioactivity in the oldest rocks. Although the observations cannot indicate whether this period of high density should be regarded as the "Creation" or simply a phase in a Universe whose density varies periodically, they do imply that the appearance of the surrounding Universe is changing with time.

In 1948 however, Bondi, Gold and Hoyle proposed an entirely different model, based on the supposition that the Universe as a whole is completely unchanging with time. In order to account for the expansion of the Universe and the consequent loss to view of distant galaxies, they introduced the idea that new matter is continuously created everywhere throughout space; this matter will eventually condense to provide new galaxies to replace those lost to view through the expansion. Although many types of observation and experiment have suggested that matter and energy cannot be created or destroyed, it has never been verified with an accuracy sufficiently high to rule out the theory of "continuous creation."

If we accept this Steady-State theory we have a model which has the philosophical attraction that the Universe presents the same appearance no matter from where, or at what time it is observed. It is a Universe unlimited in space or time, and the only unfamiliar feature which we have to accept is that the law of conservation of matter and energy is not *quite* true.

What we must now ask is whether any of the models proposed bears any resemblance to the *actual* Universe? Here we need to look for subsidiary consequences of the different models and see whether any form the basis for observational test. While it is likely to be very difficult to distinguish between forms of evolving cosmology, there is a good chance that there are detectable differences between *any* evolving model and the Steady-State model. In the evolving case we shall expect to find a progressive aging of the galaxies, and the average distance between galaxies will steadily increase; on the Steady-State model new galaxies are being born to replace those lost by the expansion and we shall always find a mixture of old and young galaxies in any sample we choose.

If therefore we could compare a photograph of the surrounding galaxies taken a thousand million years ago with one taken today, we should be able to make this distinction. Although this particular experiment is impossible, we can do something very like it by observing galaxies at a distance of a thousand million light-years. What we see is a picture of

The informed reader can second the testimony of Dr. Baxter and the members of the Advisory Committee "to the scholarly world and to the general public that this book is a major, impartial and objective contribution to American history." It was also an international achievement to which great contributions were made by European as well as American scientists and engineers.

As the authors emphasize, "only a few . . . were in a position to see the wartime technical work as a whole: Vannevar Bush, James B. Conant, Leslie R. Groves, Eger V. Murphree, Kenneth D. Nichols, Henry D. Smyth, and Charles A. Thomas. Compartmentalization prevented most men from knowing even one process through all its stages from research through design and construction to operation." Security consideration and the secrecy of the whole undertaking dictated a cell-like structure of the Manhattan Project. Those of us who occupied one or other of those cells can testify to the excellence of the authors' history in the area of our own experience and get satisfying knowledge of what went on in other realms which we knew to exist but nothing of the details. Let one example suffice. Two men in different cells met one morning on the station platform at Princeton, N. J. One cell member said: "It can't be as terrible as you calculate." The reply came: "If one falls on Princeton there will be no Princeton." And Hiroshima proved that he was right. Pages 376 to 380 on the Trinity Project at Almagordo on July 16, 1945 are a dramatic record of the culmination of so many efforts, and the reactions of the participants at the successful operation.

But this is history of interest to a much wider public than those engaged in the scientific effort. It is a document not only for the scientist and engineer but also for the historian, the economist, the political scientist, and for university and governmental administrators, since it inaugurated a new pattern of university-government relations. It has international overtones, negotiations with Great Britain and Canada during the war on the interchange of attained knowledge, on the hopes and problems of international control, on U. S.-Russian relations. It deserves a wide audience, for it does much to bridge the gap between the scientist and those students of international affairs and public administrators who have need of such knowledge.

these galaxies as they were when the light left them, and since the light has been traveling for a thousand million years, we can see what changes have occurred in this time by comparing them with nearby galaxies from which the light has taken a much shorter time to reach us. By extending our observations to greater and greater distances we can in this way explore back to earlier and earlier periods in the history of the Universe. This is the reason why astronomers want to study the faintest galaxies, and why the great 200-inch telescope on Mount Palomar was built.

Unfortunately even with this telescope it has so far proved impossible to reach any definite conclusion, owing to the difficulties of making sufficiently accurate observations of sufficiently distant galaxies.

There seemed little hope of overcoming a barrier apparently so serious, but in 1952 a most remarkable discovery was made—that the second most intense source of radio waves which had been detected in the new science of radio astronomy, was a very distant galaxy, at a distance of 500 million light years; its intensity showed that its radio emission was about a million times more powerful than that of our own or other nearby galaxies.

The fact that a source of such power could exist was surprising enough, but almost as remarkable was the evident rarity of such sources; there are about 100 million galaxies within a distance of 500 million light years and yet only this one produces radio emission so intense.

More detailed observations with the 200-inch telescope at Mount Palomar suggested that this source was a collision between two complete galaxies. This explained both the rarity of the sources (for the chance of such an encounter is extremely small) whilst the collision supplied the additional energy needed to maintain the powerful emission of radio waves.

The existence of such a source suggested at once the possibility of exploring to greater distances, and thus to earlier stages in the history of the Universe than would ever be possible in optical astronomy. But before we can use radio observations to test any particular cosmological theory, we must find some quantity which we can predict according to the theory, and which we can also measure. One such quantity, as we have seen, is the density of galaxies at different distances—or equally well, the density of radio galaxies. But in this case we have no way of measuring distance directly. If we knew the average power of the sources we could use their intensities as a measure of distance.

The first problem is therefore to find the power of the sources, so that we can predict, according to a given cosmological theory, the relative number which we should expect to find in a given range of intensity. We can then compare these results with the actual numbers we find from our observations. We must endeavour to make this comparison for the

HERE AND THERE

By the Board of Editors and the Membership of The Sigma Xi-RESA Societies

Natural Philosophy passed from the language during the Industrial Revolution and science, which succeeded it, diverged evermore from philosophy. This we learn from *The Dreams of Reason* by René Dubos; 167 pages; $5.00; Columbia University Press, 1961, where the quotation given starts on page 157, is verbatim, and reprinted with permission.

> Nothing could illustrate better the change that occurred in the focus of the scientific community during the Industrial Revolution than the sudden and complete disappearance of the term "natural philosophy." The schism between science and philosophy was the result of two forces which operated almost simultaneously. One was the recognition that knowledge could be used for creating wealth and power; the other was the rapid accumulation of new and unexpected facts which engendered a sense of humility before the complexity of nature and rendered scientists shy of extrapolating from factual knowledge into speculative thoughts. Then humility evolved into scorn for speculation and today the statement "This is not science, this is philosophy" rules out of scientific discussion any statement that goes a step beyond established fact.
>
> Yet it is apparent that today, as in the past, many scientists—among them some of the most brilliant and most effective—are eager to escape from the austere discipline of factual knowledge and to experience again the intoxication of philosophical thought. They may distrust Plato, but, like him, they seem to regard philosophy as the "dear delight." Witness the flurry of speculative books published by scientists as soon as some discovery enlarges the scope of their knowledge. The theory of evolution has been used by biologists as a platform to erect or justify religious, political, and economic philosophies. Familiarity with modern theoretical physics seems to warrant opinions not only on the structure of matter and its relation to energy, but also on the nature of life, the existence of free will, or the symbolism of language.
>
> This return to scientific philosophy negates, it seems to me, the fears so commonly expressed that scientists are becoming a class apart from the rest of society by developing a culture without contact with the rest of human life. It is true, of

weakest possible sources—for the further we can penetrate in distance—or the further back in the history of the Universe we can reach, the greater will be the differences between the predictions of the different cosmological models.

It seemed impracticable to build an instrument of sufficient size by conventional means, but at Cambridge we have developed a new method of obtaining a very large resolving power, by moving a smaller aerial to different positions within a much larger effective aerial. Our new instrument consists of two elements, one of which is 1450 × 65-ft. and aligned East-West; the other is a smaller aerial 190 × 65-ft. mounted on railway

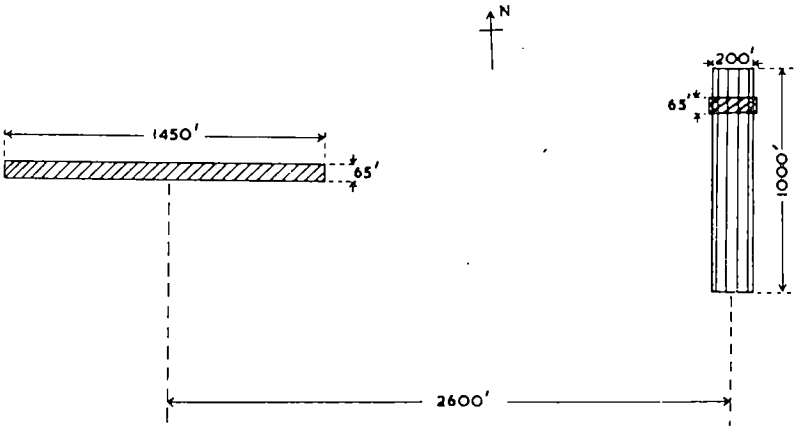


FIG. 1. Diagram of the Cambridge Interferometer.

lines 1000-ft. long running North-South (Fig. 1). The earth's rotation scans the beam round a 4° strip of sky each 24 hours, the strip being selected by the elevation of the two aerials.

In each set of observations, the moving aerial is driven to the North end of the rails and the observations are recorded (as holes in punched tape) for 24 hours; we then move the aerial by about its own width and another set of observations is made. After 25 days we have all the information we need to produce a map of the whole 4° strip.

In order to do this we must make about a quarter of a million calculations, and these are made on EDSAC, the computer of the Cambridge University Mathematical Laboratory. The output of the computer is printed on sheets which enable us to find the positions and intensities of the sources, and the detail obtained is the same as if we had built two aerials each 800-ft by 500-ft.

We have been using this telescope continuously since it was completed in the summer of 1958, most of the time on problems relating to the cosmological question.

course, that within the area of his particular work each scientist becomes so specialized that he finds it difficult to communicate on scientific subjects except with other workers in the same specialized field. But this situation is not peculiar to science. It exists just as much in other forms of learning—in philology or Moslem culture as much as in mathematics or genetics. Moreover, science should not be regarded as one single discipline concerning which sweeping statements can be made, any more than this can be done for the so-called humanities. With regard to the knowledge and operations defined by their techniques, the biologist and the mathematician are as far apart as they are from the student of Sanskrit or from the art critic, and as these are from each other. We must accept as a fact that the modern world is made up of an immense number of specialized groups, intellectually separated by experience, words, and the meaning of symbols. But although the members of one intellectual guild can rarely understand the professional jargon of another guild, men can and do communicate nevertheless at a higher level of discourse. Experiences, words, and symbols can usually be reformulated in the context of broader human meanings. In my opinion, there are not "two cultures," even though C. P. Snow has made the expression famous. There are a multiplicity of intellectual occupations, each of which fortunately has several points of contact with human life. Whatever his field of specialization, the scholar can be understood beyond the confines of his guild—but only if he is willing to raise his language above the jargon of his trade. The scholar must learn to speak to man.

How Gauss (1777–1855) made an early reputation in the theory of numbers and enunciated a *Prime Number Theorem*, proof of which has only been given in the last generation, is recorded in *The Great Mathematicians* by Herbert Westren Turnbull; 141 pages; $3.50; New York University Press, 1961. The quotation starts on page 124, and is published verbatim with permission.

> Gauss made an early reputation by his work in the theory of numbers. This was but one of his many mathematical activities, and, apart from all that followed, it would have placed him in the front rank. Like Fermat, he manifested that baffling genius which leaps—one knows not how—to the true conclusion, leaving the long-drawn-out deductive proof for others to formulate. A typical example is provided by the *Prime Number Theorem* which has taken a century to prove. Prime numbers

Our first problem was to establish the power of the sources—so that we could predict the relative number of sources we should expect to find at different intensities. At the same time we must make sure that the observations are not being diluted by large numbers of relatively weak sources at small distances—for we know that some of the sources are the remains of supernova explosions inside our own Galaxy—while others are nearby galaxies with relatively weak radio emission.

We have used three methods of finding the power of the sources, the first of which is based on radio methods alone, and involves no assumptions about the nature of the sources. This method showed that only a few per cent of the observed sources were within the Galaxy, and that

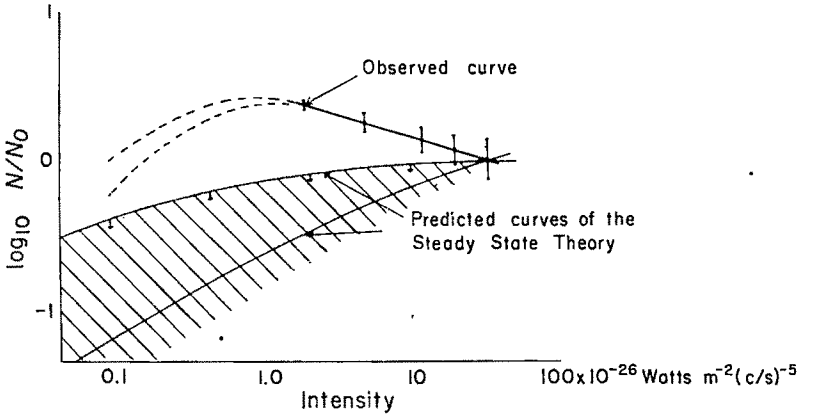


FIG. 2. Plot of relative number of sources observed (N/N_0) above given intensities I. The dotted lines represent the extension of the observations to weaker sources than can be observed individually; they are based on a statistical method which gives no information about the positions or intensities of individual sources, but allows limits to be set to the number of sources of different intensities. The full lines are the curves predicted by the Steady-State model for two values for the effective power of the sources.

most of them were external galaxies having a radio emission at least 1000 times greater than that of our own Galaxy. If we suppose that most of the sources emit as much *optical* radiation as our own Galaxy, then the limiting optical brightness of the sources allows us to set an even greater limit to the radio emission. The third method was based on measurements of the angular diameters of the sources, and again indicated an average radio emission 10^4—10^5 times that of the Galaxy. From these results we can now compute the number of sources we should expect to find at different intensities, but before we can compare the results with what we observe, we must explore a number of possible difficulties which might affect the interpretation of the observations. One difficulty of this sort is the possibility that radio galaxies might occur in clusters—

were studied by Euclid, and continue to be an eternal source of interest to mathematicians. They are numbers, such as 2, 3, 5, 7, 11, that cannot be broken up into factors. They are infinitely numerous, as Euclid himself was aware, and they occur, scattered through the orderly scale of numbers, with an irregularity that at once teases and captivates the mathematician. The question is naturally suggested: How often, or how rarely, do prime numbers occur on the average? Or, put in another way, what is the chance that a specified number is prime? In some form or other this problem was known to Gauss; and here is his innocent-looking answer:

"Primzahlen unter a $(= \infty)$ $\frac{a}{la}$."

It means that when a is a very large number, the result of dividing a by its logarithm gives a good approximation to the total number of primes less than a: and the larger a is, the more precise is the result. Whether Gauss proved his statement is not known: The quotation is taken from the back page of a copy of Schulze's Table of Logarithms which came into his possession when he was fourteen. Probably he recorded his note a few years later. Even if we recall the history of the logarithm and its diverse relations with so many remote parts of mathematics, this latest example is not a little strange. The contents of a book of logarithms, with its thickly crowded tables of decimal fractions, appears to be foreign indeed to the delicate distributions of primes among the whole numbers.

An actual proof of this theorem was given only as recently as thirty years ago by Hadamard and de la Vallée Poussin. It is an example of a new and very abstract part of the subject, now called the analytical theory of numbers, one of the striking developments in the present century.

if they did so, then although we would be able to recognize the individual sources in a nearby cluster, a very distant cluster might be mistaken for a single nearby source. We therefore made a number of subsidiary investigations of this sort to see what corrections would be necessary.

The final, and most extensive problem was to determine the actual number of sources above given intensities, down to the smallest value we could reach. The results, plotted as the number of sources N compared with the number N_0 which would have been observed in the absence of effects of the red-shift are shown in Figure 2. The corresponding curves predicted by the Steady-State model on the basis of (*a*) the lowest permissible value of radio power and (*b*) the value suggested by the optical data are also shown in Figure 2. The discrepancy seems too great to be explained by errors in the observations or their interpretation, and suggests that the Steady-State theory in its original form does not correspond to the actual Universe.

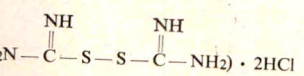
This is only the beginning of the problem, and the simplest of the questions which one can ask. There are many versions of evolving cosmologies, and there may be special cases of the Steady-State model which fit the present results. More detailed observations may make it possible to eliminate some of these too.

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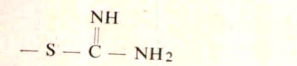
Pompously we would point out how the pattern goes: how some obscure organic compound is first synthesized in 1937 or 1927 or 1887; how investigators of a later generation pull down the dusty old volume from a high shelf for directions on the synthesis.

We would point out how these alert, up-to-date investigators base a technique on the compound; what a distressing fraction of their time and manpower is diverted from their alert, up-to-date investigation to synthesizing the old compound, an enterprise for which scant credit can be taken, however grimy the bottoms of any pitfalls left by the parties who originally described the synthesis.

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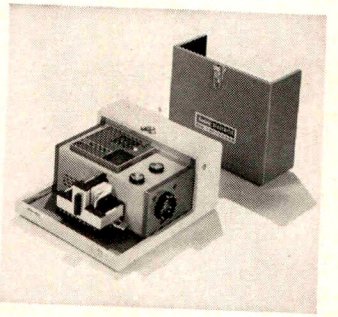
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that seems to seek out active sulfhydryl sites on transamidinases with which it forms disulfide links, with the result that the transamidinase cannot transfer metabolic amidine until something like cysteine comes along that is more attractive to the thioformamidine and breaks the blockage (*Archives of Biochemistry and Biophysics, 86*, 80).

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EXPLORATION OF THE MOON'S SURFACE*

By EUGENE M. SHOEMAKER

MANKIND today stands on the threshold of an unprecedented chapter in the history of the human race—the exploration of the solar system. Only a few short years ago serious discussion of this prospect was restricted mainly to a small group of visionary enthusiasts. Yet now both the Soviet Union and the United States have embarked with determination on major programs to place men on the Moon.

I shall attempt here to review briefly the motivations for exploring the surface of the Moon, to examine the scientific objectives of this immense undertaking, to outline some problems of strategy in exploring the Moon, and finally to forecast the probable sequence of events in the American program in the near future. The opinions expressed are personal views and do not necessarily reflect policy of the Federal Government.

Motivation for Lunar Exploration

It is important to recognize at the outset that the motivations which have led us into space exploration include much more than scientific objectives. For this reason many factors besides scientific objectives have governed the direction, timing, and distribution of effort, particularly in the exploration of the Moon. Scientific knowledge will be one of the products of our efforts in space, and science is an essential handmaiden to the enterprise. But exploration of space cannot be evaluated or justified solely on the basis of scientific merit.

The motivations that have led the United States into its present program of lunar exploration are highly complex (Rechtin, 1959 and 1960). The most valid justification for man to explore space has little to do with Cold War or science. The exploration of space is a broader exercise of human intellect and capabilities. We should explore space because it is an outstanding quest of our time (see National Academy of Sciences Space Science Board, 1961). The expectations are for new perspectives of man's role in the universe, new domains to conquer, and a wealth of new technological capabilities as well as new scientific knowledge. Many individuals, from different walks of life, have been drawn to the space program by an excitement engendered by the sense of participation in history. It is a supreme human adventure, and it is the unique privilege of persons now living to be a witness to its opening phases. A full development of this question lies beyond the scope of this paper.

* Publication authorized by the Director of the U. S. Geological Survey. This report concerns work done on behalf of the National Aeronautics and Space Administration.

THE SCIENTISTS' BOOKSHELF

By Hugh Taylor, the Associate Editors, and Guest Reviewers

SEE INDEX AT END OF THIS SECTION

Ishi in Two Worlds by THEODORA KROEBER; 255 pages; $5.95; University of California Press, Berkeley and Los Angeles, 1961.

A book which was read by the reviewer in a howling snowstorm in the winter of 1961–62 may well be the ideal book for a scientist to pack into his kit-bag as he departs for summer vacation, for use on a rainy day over the holidays. It is the story of a man who wandered out of the Stone Age into Oroville, California (shades of the Gold Rush) on the ninth day of August 1911 and into the 20th century. Fortunately, the story of his incarceration in the Oroville jail came, through newspaper accounts, to the attention of Professors Kroeber and Waterman, anthropologists of the University of California "alerted to the human drama behind the event and to its possible importance."

Professor Kroeber's wife has recorded the subsequent career of "Mister Ishi" within the Museum of the University in San Francisco, and his "brightest year," in 1914, when he went with his friends back to the Yahi country from which he came, and took them to the areas where his fellow Yahis had lived, were decimated, and finally reduced to five, of which he was the last survivor. Ishi's "capacity for gaiety was such that it bubbled up given the smallest opportunity. Ishi was quite literally the life of this party, and it was his own and his only party."

Mrs. Kroeber's luminous story is in two parts, a prologue recording a living Indian people, a dying people, episodes in extermination, and the disappearance of the Yahi tribe. The second section is Ishi's, his new world, living in a museum, his craftsmanship revealing the Stone Age to the sophisticated 20th century, his brightest year and his death, after less than five years, of tuberculosis, in the museum in which he wished to die. Even Pope, his doctor friend, could not dissuade him from this choice. As with his ancestors, he was cremated and his ashes buried in the earth over which a rock cairn was erected.

We are only fifty years from this emergence of the last man of the Stone Age in North America. If we wish to know something of the inhabitants of this continent before Western Man arrived, what is surely known of them, what they believed, what our part was in their ultimate fate, this book can give many answers. And the answers are truly moving, elements of drama and high tragedy. It is fortunate for us that Professor Kroeber, Ishi's *Chiep* (his version of Chief) was able to transmit to us, through his widow, his own personal reminiscences, his profound knowledge of the psychology of the American Indian; who could do this, as Lewis Gannett in his preface tells us of her effort, with "scrupulous integrity, poetic insight and the sense of historic drama." It is also, as Gannett concludes, "one of the great American stories, a contribution not only to our history but to our literature."

Biographical Memoirs of Fellows of the Royal Society, Vol. VII; 363 pages; 30s; London: The Royal Society, 1961.

Each issue of the *Biographical Memoirs* has a special interest for American readers. This year the list includes Ross Granville Harrison, Charles Edward Kenneth Mees, and John Henry Constantine Whitehead. The Duc de Broglie, Erwin Schrödinger, Harold Spencer Jones and H. T. Tizard are among those

The economic value of the technological advances will likely repay the cost of the space exploration program (Dryden, 1961). The successful prosecution of a difficult space mission needs much more than military might or scientific ingenuity. A broad-based and highly sophisticated technology and a great variety of advanced skills and knowledge are required to carry it through. The development of these skills and new materials, machines, and processes are, in fact, among the principal fruits of space exploration. Many of these new materials and processes will have their effect on the market place in the form of new products and familiar products produced in a better way.

Scientific knowledge is the other principal fruit of space exploration. Scientists are the most favored beneficiaries of the space program and it behoves us to make the most of the scientific opportunities. The challenge is demanding and the potential rewards are correspondingly great.

Lunar Science and the Scientific Objectives of Lunar Exploration

A wide variety of scientific purposes may be served by placing instruments on the Moon or establishing a manned lunar base. They fall in two classes, those utilizing the Moon's surface as a convenient site for experimentation or astronomical observation and those concerned with the Moon itself. As a stable platform with a rather precisely known orbit, the Moon may afford a prime site for astronomical and astrophysical observation outside the Earth's atmosphere. Our knowledge of the conditions near the lunar surface is too fragmentary as yet to know what advantages the Moon affords over an artificial satellite observatory. A space charge at the lunar surface and a large increase in the number of flying solid particles derived by impact of meteoroids could be severe nuisance factors. On the other hand, certain types of cosmic ray observations could be made, for example, utilizing the Moon as a shield. The hard vacuum at the lunar surface may be a suitable environment for certain classes of physical experiments that cannot be performed on Earth or with artificial satellites. The primary scientific objective in going to the Moon, however, and one that far transcends all others, is the study of the Moon itself.

The Moon has been an object of scientific investigation since Galileo first trained his telescope upon it in 1610. The present state of knowledge furnishes a broad base for evaluating what further rewards may be expected from exploring the Moon's surface.

The motion of the Moon in space has been studied since long before the invention of the telescope and still presents a challenging problem in celestial mechanics. Observations of the Moon and planets provided the empirical foundation for Newton's formulation of gravitational theory. The most complete theory of the Moon's motion, taking account of the various effects of the non-spherical shape of the Earth and the influence of other major bodies in the solar system, has been developed by E. W.

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also whom American readers will be interested in reading the record of their lives and achievements. Ross Harrison, the inventor of tissue culture, receives a glowing tribute from M. Abercrombie. Dr. Mees, who played so large a part in the scientific development of photography, is sympathetically recreated by Walter Clark. Henry Whitehead, who rose to the heights of his mathematical powers in Princeton and in Oxford, was stricken all too soon in May 1960.

These memoirs are an indispensable record of the scientists of our times who have made of science that which she holds of esteem and reverence.

Sexuality and the Genetics of Bacteria by FRANCOIS JACOB and ELIE L. WOLLMAN; 374 pages; $10.00; Academic Press, 1961.

One of the most spectacular advances in science during the last decade has been in the development of the genetics of bacteria. Not only has understanding of the structure of the bacterial genetic material and its mode of recombination been reached, but the results are fundamental to areas such as virology, biochemistry, and classical genetics.

In particular, the study of bacterial genetics by bacterial recombination has yielded great insight into genetic structure and the expression of genetic material. The authors of this book have provided many of the major contributions to bacterial recombination, and while their work as well as that of others is widely appreciated, it has not previously been brought together into a form that is readily available. The material is collected and organized in this book.

In the first section (50 pages), the background of bacterial genetics is summarized in a most readable way. In the second section (150 pages), the intricate process of bacterial conjugation is dissected into its stages. The connection between lysogenic phage and genetic material is shown to be useful in elucidating both bacterial recombination and also lysogeny. Roles of the several bacterial mating types are discussed. In the final section (100 pages), the results of these studies are applied to questions of the organization of bacterial genetic material—particularly gene mapping and chromosome structure, expression of genes, and the nature of lysogenic prophage and episomes—genetic entities which need not be attached to the chromosomes.

The book is written in a masterly style, with great clarity and elegance. It is up-to-date—most references are dated in the last five years. The result is a volume of primary importance to everyone interested in genetics or microbiology. The simpler parts, at least, should be most stimulating to advanced students. This book can be recommended highly in every way. —*Arthur B. Pardee*

Vertebrate Biology by ROBERT T. ORR; 400 pages; $7.50; W. B. Saunders Co., 1961.

The primary object of the study of animal biology is to give us some idea of the nature of the varied and marvellous living creatures we see about us—how they function, what makes them tick. In the earliest days of zoological science, little could be done except to name the animal and classify it, describe its external features and tell what could be discovered from field studies of its habits and mode of life. This was, to be sure, superficial—necessarily so—but, at least, the main objective, study of the whole organism, was kept in sight.

Over the last century and a half we have, with new techniques and new methods, probed much more deeply into the nature of animals. A variety of specialties have arisen, in which various fractions of the animal have been studied intensively—its anatomy, histology, physiology, biochemistry, embryology, paleontology, and what have you. But, in the process of laboring in our own little compartments, we have tended to lose sight of our real objective. We have taken the animal apart. We have considerable knowledge of various fragments of it, but the concept of the totality of the organism has tended to disappear. It is well for us

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occasionally to crawl out of our separate little holes and realize that the series of discrete structures and functions with which we are dealing have no real meaning except in so far as they are integrated into a living whole. We need to put the animal together again in our mental processes. For the vertebrates, Orr's book does this admirably. Here are animals—whole animals, going about their business of being animals. Let us pause a bit and admire the synthesis nature has made of all the special bits and pieces which we study seriatim. The first two-fifths of the book gives a "who's who" of the vertebrates by classes, from fish to mammals, noting for each major group its general and special characters, together with a brief classification. The remainder of the volume treats, chapter by chapter, a series of topics over the range of all groups—methods in systematics, distribution, territory and home range, population movements, dormancy, reproduction, growth and development, population dynamics. A short, well-chosen list of references is appended to each chapter.—*Alfred S. Romer*

Introduction to Entomology by R. JEANNEL, translated by HAROLD OLDROYD; 344 pages; $8.85; London: Hutchinson & Co., 1960.

This is a translation of the original edition of *Introduction à l'Entomologie* (Ed. N. Boubée et Cie), by Professor Jeannel of the Muséum National d'Histoire Naturelle in Paris. Mr. Oldroyd is himself an entomologist and the author of numerous technical papers.

This *Introduction* has apparently been prepared for readers with some zoological training, but it is not sufficiently documented or detailed to serve as a college text. It also reflects Jeannel's strong belief in the Lamarckian principles of the inheritance of acquired characters. The book is divided into three main sections. The first covers general anatomy and classification; the second, physiology and behavior; and the third, palaeontology and geographical distribution. The first two require no special comment; the account of morphology is conventional and the illustrations of the section are taken largely from works by Imms and Weber. The classification used is basically that of Dr. A. V. Martynov, with some specific modifications.

The section on paleontology and geological distribution, comprising about a third of the work, is the unusual part, these being subjects that are ordinarily briefly treated at best in general works on insects. Unfortunately, this account is most disappointing. Even the illustrations are pathetically antiquated; of the 96 figures of fossil insects included in the book, 80 are taken from Handlirsch's publications of more than 50 years ago. What is worse, these are, in the main, Handlirsch's highly imaginative and erroneous reconstructions which have been rejected by serious students of fossil insects for several decades. The text is equally out-of-date and contains a great many statements which are definitely incorrect. For example, Jeannel's account (p. 243) of the immature stages of the Palaeodictyoptera reads as follows: "Fossil imprints of larvae are known (Pl. 3, II, Fig. A). They are said to have compound eyes, and rudiments of wings sticking out at right angles to the sides of the body." However, a critical study of these particular specimens published in 1948 (*Psyche*, 55: 41–50) showed that they were neither larvae (nymphs) nor Palaeodictyoptera. Frequent assertions, made without explanation or documentation, place Jeannel in a vulnerable position and are often contradicted by similarly definite assertions elsewhere in the text. In his account of the extinct order Protephemeroptera, he writes (p. 256), "These insects were dominant during the Carboniferous." Later, on the same page, he reveals that this "dominant" order is known from a single species, although he does not indicate the further fact that this species is represented by a unique specimen! Another example (p. 295): "The Psocoptera, which are not known as fossils, are close to the Planipennia (Neuroptera) and seem clearly to have

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Sir MacFarlane Burnet is a recipient of the 1960 Noble Prize in Physiology and Medicine "for the discovery of acquired immunological tolerance," and author of *Virus as Organism* (Harvard University Press, 1945). $4.75

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arisen from a primitive group of Hemerobiidae . . ." But on page 305 we read, regarding the Archepsyllidae [*sic*]: "This is a group that Handlirsch first placed with the Homoptere but which is really a group of Psocoptera that flourished in the Jurassic . . . Psocoptera of a variety of families are known from the Baltic Amber."

The publication of this English translation of Jeannel's book is regrettable; it certainly adds nothing to Jeannel's reputation and contributes little to our knowledge or the dissemination of knowledge of the insects. —*F. M. Carpenter*

Cell Heredity by RUTH SAGER and FRANCIS J. RYAN; 411 pages; $7.50; John Wiley & Sons, 1961.

This book should perhaps be called "What every geneticist should know." It is a successful blend of newer findings in genetics at the cellular level with the classical literature in this field. The resulting mixture is both extremely interesting as well as scholarly.

In the present era it isn't surprising that much of the content is microbial genetics. What is pleasantly surprising is that the authors have kept the outlines of general genetics clearly in view. Although cells and molecules such as DNA are very much in the foreground, the content would be interesting to any biologist.

Some of the topics covered under chapter headings are: The Chemical Basis of Heredity, What Is a Gene, and Mutation as a Chemical Process. Mutation is comprehensively dealt with in several chapters and the over-all effect is quite satisfactory in terms of balance and integration with the other topics.

The style of writing is uncomplicated. It tends to lead the reader on, and for this reason the book might even be used as a beginning text in genetics. It should also be an exeellent graduate text because of its penetrating and straightforward analysis of complex problems such as recombination.

One is left with the general impression that the authors have made more than just a compilation of current facts. There is a good deal of meaningful synthesis of new ideas and some projection of unsolved problems in genetics.—*Bruce M. Eberhart*

Microbial Cell Walls by MILTON R. J. SALTON; 94 pages; $3.50; John Wiley & Sons, 1961.

The 1960 CIBA Lectures in Microbial Biochemistry, delivered at the Institute of Microbiology, Rutgers University, constitute the three chapters of this book, viz., Isolation and General Properties of Microbial Cell Walls, Chemistry of Cell Walls, and Enzymic Degradation and Biosynthesis of Microbial Walls.

By selecting for discussion those research contributions which are particularly pertinent and by including about each just the necessary amount of information to enable the reader to grasp its nature and significance, the author has managed to achieve, in a thoroughly readable book, an astonishingly complete and yet obviously quite concise coverage of this fascinating subject. The author tells us what is known about the cell wall, how it was found out and, of particular value, indicates the principal directions of present and future research.

The detailed factual information presented is of great interest. Beyond this, one finds several important general comments. For example, it is stated that knowledge in this field has resulted from the combined application of techniques of biophysics, chemistry, and biochemistry and that the method of choice (for wall isolation) will depend on the particular organism. It is well to remind both beginner and experienced worker that investigative success frequently depends on the use of an array of techniques and on a profound knowledge of the biological system under study. The latter, in particular, is too frequently lacking.

The references following each chapter are particularly worth while.

The few typographical errors detected, e.g., hydrazinolysis on page 38, attest to the care taken with this book.

THE USE OF VITAL AND HEALTH STATISTICS FOR GENETIC AND RADIATION STUDIES

PROCEEDINGS OF THE SEMINAR SPONSORED BY THE UNITED NATIONS AND THE WORLD HEALTH ORGANIZATION, 1960



The records of a seminar of some 70 international experts who met in 1960 to review how best to use and adapt national civil registration practices and vital and health statistics for genetic and other studies relevant to the radiation problem. The seminar considered practical suggestions for overcoming present limitations, so as to meet, in particular the critical needs of human geneticists and radiation epidemiologists. The seminar was sponsored jointly by the United Nations and the World Health Organization.

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The sole disappointing feature of this work was the fact that the electron micrographs of ultrathin sections of bacteria, figures 7, 17a, and 17b, are not up to the high quality of those of the shadowed bacteria. Thus, in figure 7, one cannot see the multilayered wall mentioned in text and legend. This is, however, a minor fault and detracts little from an extremely useful book.—*George B. Chapman*

Metabolic Pathways in Microorganisms by VERNON H. CHELDELIN; 91 pages; $3.50; John Wiley & Sons, 1961.

The text of this monograph is derived from the third series of the E. R. Squibb Lectures on Chemistry of Microbial Products at the Institute of Microbiology, Rutgers University. Lectures for the series were presented by the author and consequently much of the data covered are those from his laboratories.

Discussion in Chapter 1 is primarily concerned with the unusual metabolism of *Acetobacter suboxydans*, which is quite abnormal indeed. Several metabolic conversions in this quaint organism are reported catalyzed by more than one enzyme, while seemingly more important reactions are unable to proceed because of no enzyme at all (both glycolysis and the citric acid cycle are thought to be non-functional). Thus, the total flow of carbohydrate metabolism is reported to proceed via the hexose monophosphate pathway. Certainly a unique classification must be reserved for an organism with such unusual characteristics.

Evaluation of pathways of glucose metabolism is discussed in Chapter 2. A conveniently included table lists the relative importance of each pathway for a number of microorganisms. Both the data and comments are thought-provoking.

Chapter 3 is concerned with the general considerations of carbohydrate metabolism and, with only slight revision, could serve well as an introductory chapter. As it stands, Chapter 3 is both last and least, only supplementing the interesting reading provided in the previous portions of the volume.

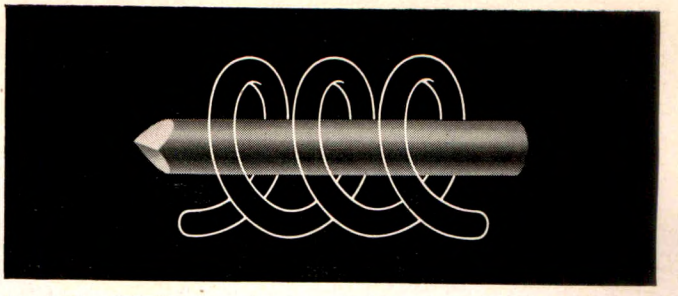
Although precious gems are sometimes found in small containers, great treasures seldom are. Hence, this three-chapter monograph presents some stimulating suggestions but, is unable to fulfill completely the expectations created by the ambitious title.—*John Imsande*

The Biology of Stentor by V. TARTAR; 413 pages; $12.00; Pergamon Press Inc., 1961.

In a refreshing revival of the best of the monographical spirit of the 18th and 19th centuries, Dr. Tartar has produced an admirable volume on a ciliated protozoon of growing importance in modern biological research. From the exquisitely colored frontispiece and hand-labelled text-figures to the carefully selected bibliography, one finds evidences of originality, independence, and broad comprehension rare in this age of scientific specialization. Trained at Yale University under the stimulating influence of the late Ross Granville Harrison, Tartar has found in *Stentor* a unicellular organism beautifully endowed with morphogenetic characteristics, the patient and skillful study of which may provide answers to innumerable questions which have persistently puzzled the experimental embryologist and cellular physiologist working with metazoan material.

Tartar's thorough exposé of the proved and potential uses and value of species of *Stentor* in biological research—the most important of which is their ability to undergo grafting and transplantation operations successfully—will be of interest to protozoologists, experimental morphogeneticists, cellular physiologists, and even teachers and students of general biology. Nearly three-quarters of the book is, quite properly, devoted to topics such as growth and division, reorganization, regeneration, primordial development, form, and polarity, and especially to experiments which have been designed (most often by Tartar himself) to solve problems within these areas. The lucid "do-it-

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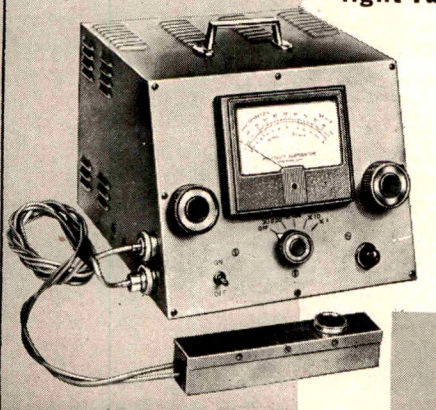
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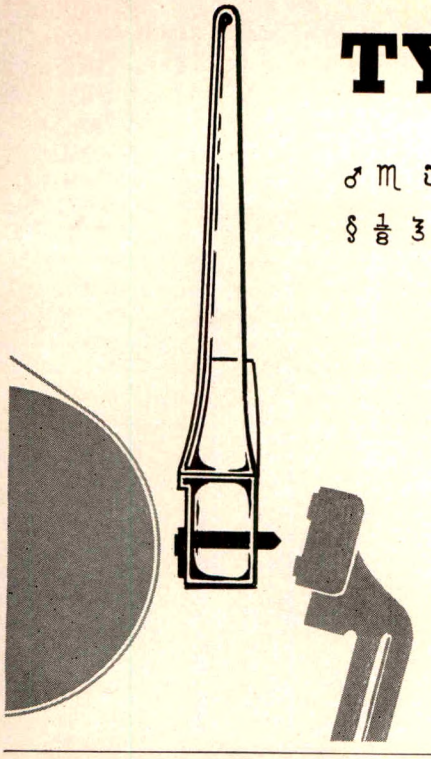
yourself" chapter on techniques should be instrumental in leading new students into the fruitful, yet to date practically virgin, field of *Stentor* morphogenesis. Rather briefly treated, understandably (because of lack of information in the literature), are the subjects of fine structure and general cytology, biochemistry, systematics, and genetics. There is certainly an urgent need to learn more of the sexual life of stentors, particularly in view of the great potential value of these ciliates in testing hypotheses concerning nucleocytoplasmic interrelationships via the transplantation approach. But even the cytology of conjugation has never been adequately described to date; unfortunately *Stentor* has cooperated little in this respect with the investigator, judging from the rare occurrence of sexual processes in laboratory cultures.

In a final chapter, Dr. Tartar presents heuristic discussions of the problems of morphogenesis in ciliates in general, of the cell theory and protozoa, and of theoretical implications of a "pattern factor" present in all organisms and endowed with genetic continuity.

Specialists, particularly protozoologists, will find inadequacies in this book in their own areas; embryologists may desire more discussion of their problems with simplification of the information so dear to the general student of the protozoa; literary purists will note scattered typographical errors; everyone will complain of the price (set by the publisher, not the author, of course). But there is little doubt that this unique volume represents, over-all, a classical contribution to mid-20th century literature in the combined fields of protozoology and experimental cell biology.—*John O. Corliss*

The Structure of the Eye, edited by G. K. SMELSER; 570 pages; $15.00; Academic Press, 1961.

This book represents the proceedings of a symposium, held in New York, devoted to the anatomy of the eye. Our interest in the minute anatomy of the eye stems, of course, from our desire to ascertain the functions of its parts; and on this basis we may divide the present studies into several groups. First and foremost from the point of view of physiological interest, are the studies on the structure of the photoreceptors, the rods and cones of the vertebrate retina. Here, of course, the electron-microscopical investigations take pride of place, and our present knowledge is summarized by such authorities as Sjöstrand, De Robertis and Lasansky, Fernandez-Moran and Wolken. The retina, of course, is not only a collection of photoreceptors but contains neurones, a glial system and a nutrient vascular system, while it is in contact with a layer of pigment epithelium which separates it from—or connects it with—the underlying choroid circulation. Some of these aspects of retinal structure have been studied in papers by Yamada, Bernstein and Wolter, but in respect to the vascular circulation the book is regrettably deficient. More fundamental aspects of the retina are covered by Wald, Tansley, Eranko, Niemi and Merenmies, and Dowling and Gibbons. A second group of papers is devoted to the crystalline lens; purely anatomical studies are represented by a single paper of Wanko and Gavin, who have described the electron-microscopical appearance of lens fibers in the region where they come close to each other in the sutures. The remaining papers are not anatomical at all, being concerned with such biochemical problems as antigens (Langman), radiation damage (Pirie), DNA synthesis (Harding, Feldherr, and Srinivasan). The vitreous body is a very delicate jelly, the intactness of which is essential for its transparency and also the firm attachment of the retina to its base. Schwartz describes the electron-microscopy of dried sections, confirming that the basis of the structure is a series of interlacing fibrils. A more detailed description of the molecular morphology is given by Balazs, based on biochemical as well as electron-microscopical studies. Studies on the cornea constitute another group of papers; here we have a complex tissue that maintains a high degree of transparency only under certain conditions,



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and the cause of this transparency, and of the loss of transparency that occurs sometimes are matters of considerable interest. In the present book, the purely morphological studies are those of Jakus, Feeney and Garron, Coulombre and Coulombre, and Schwartz, while the more functional aspects are dealt with by Maurice and Mills and Donn.

Finally, we are interested in those structures concerned with the elaboration and drainage of the intraocular fluid, namely the ciliary epithelium, where the fluid is primarily formed, and the trabecular meshwork adjacent to Schlemm's canal where it is filtered away, and the surface of the iris across which dynamic exchanges of material take place. The paper by Pappius and Smelser on the electron-microscopy of the ciliary epithelium adds to what has already been related by Pease on this subject, while Tousimis and Fine describe the pigment epithelium of the iris and Rohen the trabecular meshwork.

So much for most of the contents; as the proceedings of a symposium the book is of some value in so far as it contains summaries of recent research on a number of aspects of ocular anatomy. When asked to review this type of book I always ask myself the question: Is the publication necessary? Symposia are gatherings of workers in a given field that enable them to describe their progress and discuss their problems in comfort; and in fulfilling this role they justify their expense both in money and time. That there should be a permanent record of the lucubrations of the participants is by no means certain, however, since most of what is said has been said before or will be said later in publications of the learned societies. So far as the present proceedings are concerned, it would be churlish to answer the question with a flat negative; taken for what it is, the book is a useful addition to the library of the research worker on ocular physiology and anatomy; it is extremely well got up and the standard of reproduction of the electron-micrographs is high.—*Hugh Davson*

International Review of Cytology, Vol. XI, edited by G. H. Bourne and J. F. Danielli; 356 pages; $11.00; Academic Press, 1961.

The contributors to this volume and the titles of their reviews are: Kurosumi, electron microscopic analysis of the secretion mechanism; Slifer, the fine structure of insect sense organs; Coulombre, cytology of the developing eye; Wolken, the photoreceptor surface; Metz, use of inhibiting agents in studies on fertilization mechanisms; Prescott, the growth-duplication cycle of the cell; Cabrini, histochemistry of ossification; Pomerat, cinematography, indispensable tool for cytology. Also included is an alphabetized list of authors from the previous volumes.

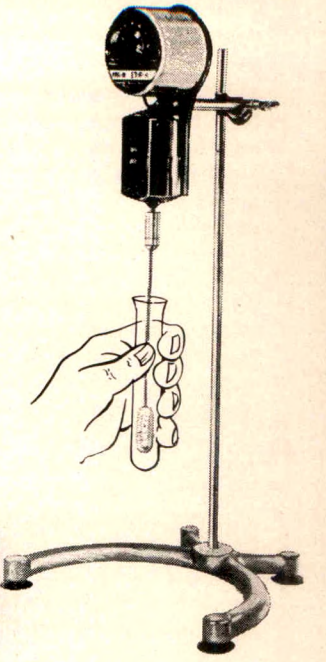
Kurosumi's comprehensive review is a successful attempt to collate the scattered results of electron microscopic researches that bear on the morphological basis of the different types of secretion. Slifer's polished review summarizes extant information and frames unanswered questions. Coulombre emphasizes recent advances in knowledge of the histology of the vertebrate eye. Wolken logically brings together the morphological evidence of plant and animal photoreceptors that indicates the structures may all act as semiconductors. Metz classifies in impressive detail the types of agents that inhibit fertilization; he demonstrates how the inhibitors have been used to clarify the mechanisms of fertilization. Prescott has presented a balanced and trenchant survey of recent literature pertaining to the cell cycle. Cabrini's article is a description of the locations of several enzymes in developing bones; he attempts to relate the location of the enzymes to the processes of ossification. Pomerat's article is mostly a description of his own work and that of his collaborators.—*R. C. von Borstel*

Symbols, Signals and Noise (The Nature and Process of Communication) by J. R. Pierce; 307 pages; $6.50; Harper and Brothers, 1961.

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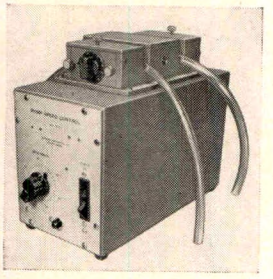
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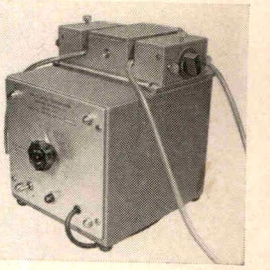
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Brown (1896). Ignoring all but the simplest terms, we may describe the Moon's orbit about the Earth as an ellipse of 0.055 eccentricity inclined 5 degrees to the ecliptic. Mean distance of the center of gravity of the Moon from the center of gravity of the Earth is about 239,000 miles. The plane of the ellipse precesses with a period of 18.6 years and the long axis of the ellipse rotates in the plane with a period of about 9 years. The Moon rotates with a period equal to the period of revolution about the Earth. Its equator is inclined to the ecliptic 1½ degrees in the direction opposite to the inclination of its orbit. Its rate of rotation is nearly constant in contrast to its rate of revolution, which varies according to Kepler's second law. As a consequence of these motions we see the Moon from a continuously varying angle, an effect known as libration, and thus about 59 per cent of the Moon's surface is visible over the course of a year.

The mean radius of the Moon is 1080 miles, about one-fourth the radius of the Earth and only slightly less than the radius of the planet Mercury. In dimensions the Moon is thus comparable to the terrestrial planets, and the Earth-Moon system may be thought of as essentially a double planet.

The mass of the Moon, which may be determined either from the precession and nutation of the axis of rotation of the Earth or from the revolution of the Earth about the center of gravity of the Earth-Moon system, is 7.3×10^{25} grams, or about $\frac{1}{81}$ the mass of the Earth. The mean density of the Moon is thus 3.34 gms/cc. This figure is the basis for nearly all speculations on the bulk composition of the Moon. After slight corrections have been made for compression of the rocks in the lunar interior, the density of the Moon is found to be close to the probable uncompressed density of the Earth's mantle. In fact, an estimate of the density of the Moon was used by Bullen (1940) for the top of the mantle in computing density profiles for the Earth. Its density is slightly less than the average grain density of the most common types of meteorites. These facts have led to the prevailing and reasonable view that the Moon is composed dominantly of ferromagnesian silicates as are the meteorites, and is probably close in average composition to the intermediate region of the Earth's unknown and hitherto inaccessible interior.

From mathematical relations related to the stability of the Moon's motion (Cassini's laws) and from very slight accelerations in its rate of rotation, the ratios of the three principal moments of inertia of the Moon may be computed within certain broad limits of error. The moment about an axis measured in the mean direction from the center of mass of the Moon to the center of mass of the Earth is significantly less than the moment about an axis perpendicular to the plane of its orbit and the moment about an axis perpendicular to the plane that contains the first two. Owing to libration, the rotation of the Moon undergoes continuous

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As is typical of other books written by Dr. Pierce and intended for the general reader, there are no end of entertaining references. There is a sample paragraph from an entire novel written without using the letter E. There is an interesting discussion relative to Maxwell's demon. There are pertinent comments on those who claim to work with cybernetics. There are examples of writing and music produced by essentially stochastic processes. Finally, there is a closing chapter pointing out that information theory is really a rather narrow field and that its limitations should be clearly recognized.

Even someone planning to go much deeper into the subject would find this book a good place to begin his studies.—*W. J. Cunningham*

Reactivity of Solids, J. H. DE BOER, Editor-in-Chief: 762 pages; $28.75; D. Van Nostrand (Elsevier) Co., 1961.

On May 30, 1960, 550 scientists from twenty-seven countries assembled in Amsterdam for the Fourth Quadrennial International Symposium on the Reactivity of Solids. The themes of the symposium were the kinetics and mechanisms of reactions of solids. Almost all the 81 papers are now available to posterity in this sumptuous volume. All serious libraries must buy it; few individuals will be able to afford it. The main topics discussed were diffusion and sintering, reactions of solids with gases, liquids, other solids; transitions; thermal decompositions; factors that influence reactivity (defects, radiation, pressure, crystal structure); technical implications; new experimental methods. In addition to research reports, there were broad summarizing lectures in the various fields.

This Conference traditionally has had a somewhat technological orientation. Industrial chemists, metallurgists, nuclear technologists, and ceramists outnumber solid state physicists and theoretical chemists. Perhaps as a consequence empirical observations outnumber solutions to basic problems. New experimental methods and theories in the field of diffusion are mentioned briefly but not applied to actual problems. There is much discussion of defect mechanisms, but only one convincing proof of a mechanism (Haul appears to have proved that oxygen diffuses in cadmium oxide through vacancies at oxide sites). But these experiments do deal with the stubborn substances of the real world—not just superpure NaCl, Si and Ge. Yet as Bénard pointed out in his beautifully illustrated report on nucleation phenomena, we shall have to get down to the task of making oxides, sulfides, halides, and other compounds as pure as zone-refined silicon if we expect to make comparable progress in our understanding of these materials.

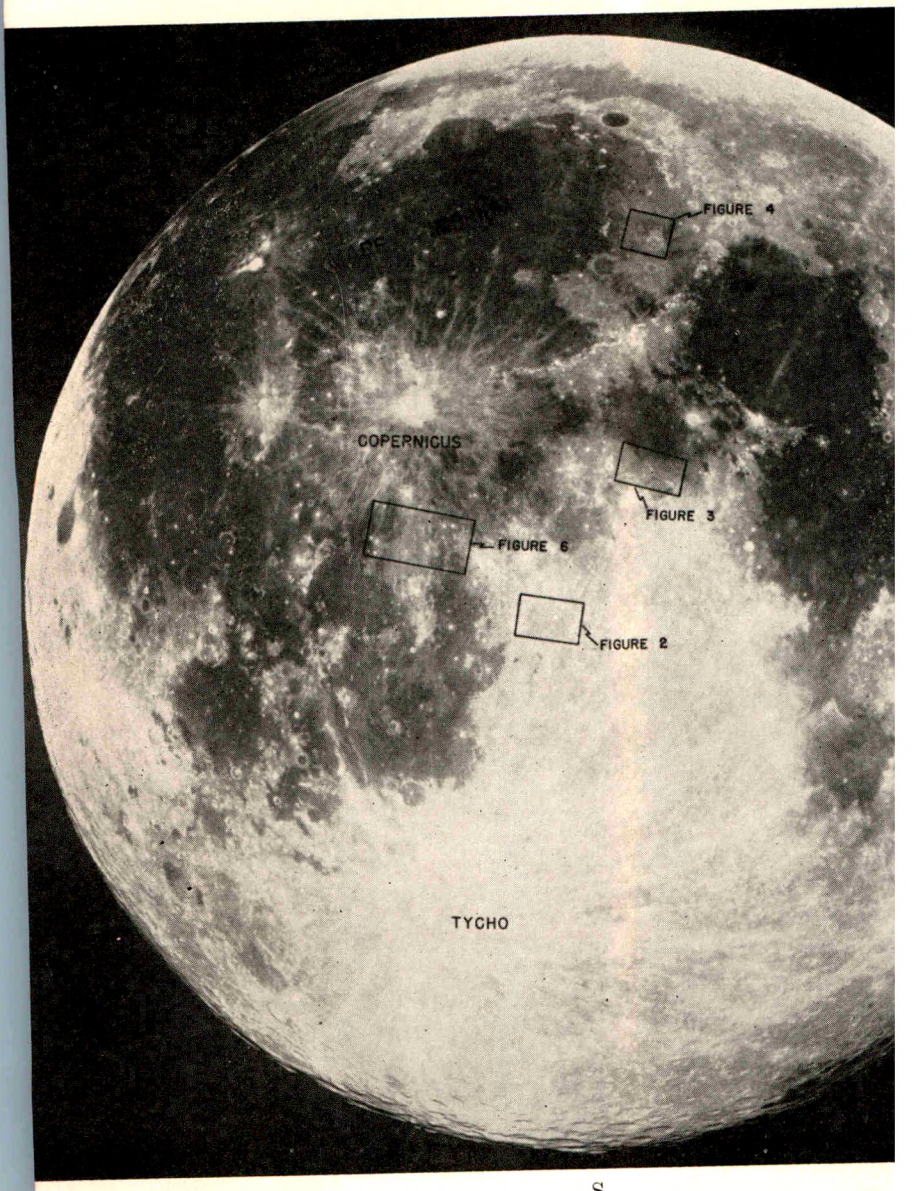
The next symposium will be in Munich in 1964. We can predict (1) an attendance of 1000 (2) a much more precise understanding of defect mechanisms (3) a revolution in the mathematical treatment as high-speed numerical computations replace some rather lame analytic approximations.—*Walter J. Moore*

The Radiation Chemistry of Water and Aqueous Solutions by AUGUSTINE O. ALLEN; 204 pages; $6.00; D. Van Nostrand Co., 1961.

The author of this book, a Senior Scientist at the Brookhaven National Laboratory, has been active in radiation chemistry for twenty years or so. He is particularly well-qualified to write on the radiation chemistry of water and aqueous solutions because he has contributed greatly to the understanding of the field. His book will be widely appreciated and used.

It is clear on general grounds that the chemical effects of high energy radiation must originate in the matter which

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FIG. 1. The full Moon, showing location of regions described. Photograp
Observatory.

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small but measurable harmonic accelerations (physical libration) from which the deficiency in the moment about the axis toward the Earth has been calculated. This deficiency may be due to asymmetrical distribution of density within the Moon, or asymmetrical shape, or both. It is equivalent to a bulge toward the Earth of about 1 km above the fluid equilibrium figure for a Moon of uniform density. Whatever the origin, the deficiency must be sustained by stress differences in the lunar interior which are similar in magnitude to the stress differences that must exist in the Earth in correspondence to the observed second and third harmonics of the Earth's gravitational field (compare Jeffreys, 1959, p. 202, and O'Keefe, 1960).

The geometric shape of the Moon has been investigated by methods related to those employed in modern photogrammetry. The three-dimensional relation of points on the lunar surface may be determined from the parallax between the points on different photographs introduced by libration. Most of the published determinations are more than half a century old (Franz, 1899; Saunder, 1905) and, except for the local relief, the departure of the Moon's surface from a sphere cannot definitely be said to be greater than the possible error of the calculated positions of the points. Renewed attack is currently being made on this question by several investigators.

As the motion of the center of mass of the Moon is fairly accurately known, the position of the center of mass with respect to the limb region or profile of the Moon can be determined from the occultation of stars. Such observations show that the center of mass lies about 1 km north of the center of the lunar disc. This is the most suggestive evidence that the distribution of density within the Moon is both non-uniform and asymmetrical. It is of special interest because of its possible correlation with differences in distribution of surface features between the visible parts of the northern and southern hemispheres of the Moon (Fig. 1).

The most detailed information accumulated about the Moon has to do with its topography. On the face of the Moon we find a landscape that is strikingly different from that of the Earth. Craters are the dominant topographic form; they range in diameter from huge circular basins hundreds of miles across down to the limit of resolution for telescopic observation, about one-eighth mile. The number of craters increases as an inverse exponential function of their diameter, and the entire surface of the Moon is peppered with uncounted tens of thousands of small craters barely large enough to be observed or photographed (see Figs. 2 and 3).

Most of the craters are scattered irregularly over the Moon's surface but some of the smaller craters are aligned in chains (Fig. 2) and some along narrow trenches known as rills or clefts (Fig. 3). In their alignment, size, and form the lunar chain craters most closely resemble terrestrial

absorbs the energy and thus track effects are always expected. Only for water and aqueous solutions has a framework evolved for explanation of radiation effects with explicit acknowledgment of track effects: it is called the "radical diffusion model." In this model, chemical effects result from the action of H and OH radicals in diffusion-controlled reactions. There is some theoretical foundation for the model, but it is by no means complete. Dr. Allen's book starts with a discussion of the physico-chemical basis of radiation effects and a description of the model. This treatment is of necessity brief but adequate for a book which is written primarily to present experimental work.

The author summarizes experimental results for all types of aqueous systems which have been studied. He covers solutions of inorganic compounds, organic compounds, and materials of biological interest. The literature of radiation chemistry has grown so much that in a book as short as this one selection of material has to be made with care. The author has chosen material effectively. He has also been careful to point out difficulties in interpretations and areas in which further work is needed.

Dr. Allen's book comes out at a time when radiation chemists are engaged in a critical re-examination of the radical diffusion model. The author mentions that evidence exists for "two kinds of H atoms." The number of intermediates has now increased again and it may be some time before a unified description of the field is possible. In spite of these difficulties, Dr. Allen's book will be useful for many types of readers, particularly because of the author's critical attitude.—*John L. Magee*

Science in Space by L. V. BERKNER and H. ODISHAW; 458 pages; $7.00; McGraw-Hill Book Co., 1961.

For anyone interested in geophysics, astronomy, cosmic rays, and extra-terrestiual life, this book is a gold mine. Like most compendia assembled from chapters written by many authors, it has rich veins and thin. In attempting to cover the various branches of science which promise to be the main lines of space research, Berkner and Odishaw have called on eminent men like Rossi, Urey and Van Allen. Rossi, in cooperation with Robert Jastrow, presents an exciting account of space experiments to date, which is intelligently organized and skillfully interpreted so that any scientist can profit from it. Urey's articles on the moon and planets are much more speculative in character. He makes no attempt to be comprehensive in his treatment of the problems susceptible to space techniques, but, in his inimitable way, launches dozens of intellectual Sputniks concerning the origin and evolution of those bodies. Thus, we learn that the earth tide on a liquid moon would vary 16 meters a month, and that if these tides could be observed, it might be possible to prove that the earth captured the moon. Van Allen contents himself with an account of the radiation belts. Prepared in 1959, this report is disappointingly sketchy, but, to compensate, he has added a valuable bibliography of twelve pages concerned with geomagnetically trapped particles alone.

In spite of its unevenness and lack of comprehensiveness, "Science in Space" is rather enjoyable reading for those looking ahead to research in this field. One suspects that one of the motivations for publishing it at this time was to interest scientists in space research. If that is true, Berkner and Odishaw have succeeded in at least one case.—*George B. Field*

Numerical Weather Analysis and Prediction by P. D. THOMPSON; 170 pages; $6.50; The Macmillan Co., 1961.

This is the first book-length treatment of the new techniques of weather forecasting by use of high speed digital computers. It stresses the fundamental physics and mathematical aspects of the subject.

After a brief introduction and historical survey of the field, the basic governing equations in various forms are introduced. The mathematical tech-

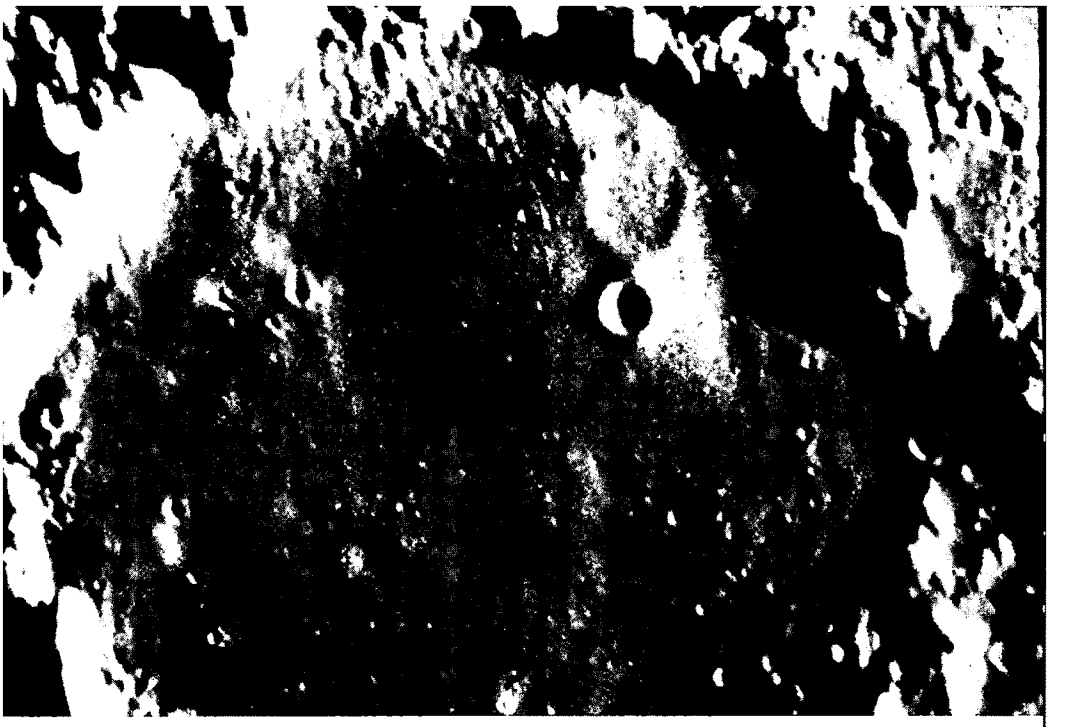
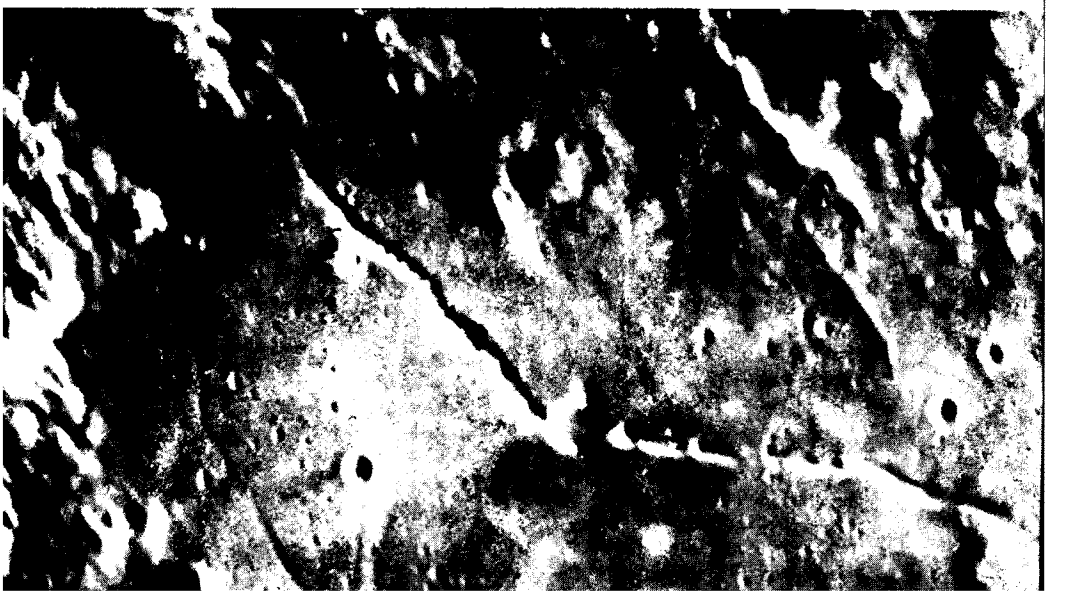


FIG. 2. Moon, region of Ptolemaeus. Photograph from Observatoire de Meudon. Ptolemaeus, a crater 90 miles across, is of pre-Imbrian age. Linear ridges and valleys that may be seen to intersect the crater walls are part of a system of ridges and valleys that radiate from Mare Imbrium. The floor of Ptolemaeus is blanketed with regional material of the Imbrian system (see Fig. 5), on which are superimposed a great number of small craters. Barely discernible small ridges that may be seen extending on to the floor in the upper part of the photograph are part of the rim material of a crater of Archimedian or Eratosthenian age. A series of chain craters may be seen in upper right hand corner of the photograph.

FIG. 3. Moon, region of Mare Vaporum. Photograph from Observatoire de Meudon. Near the center of the photograph is the crater Hyginus, 6 miles in diameter, lying astride the great rill. Over ten smaller craters large enough to be seen in this photograph occur along the rill. Smaller rills may be seen on the left hand side of the photograph. Elongate ridges observable in the upper and left hand side of the photograph are part of the system of ridges radiating from Mare Imbrium. A large number of small craters are scattered over the entire region.



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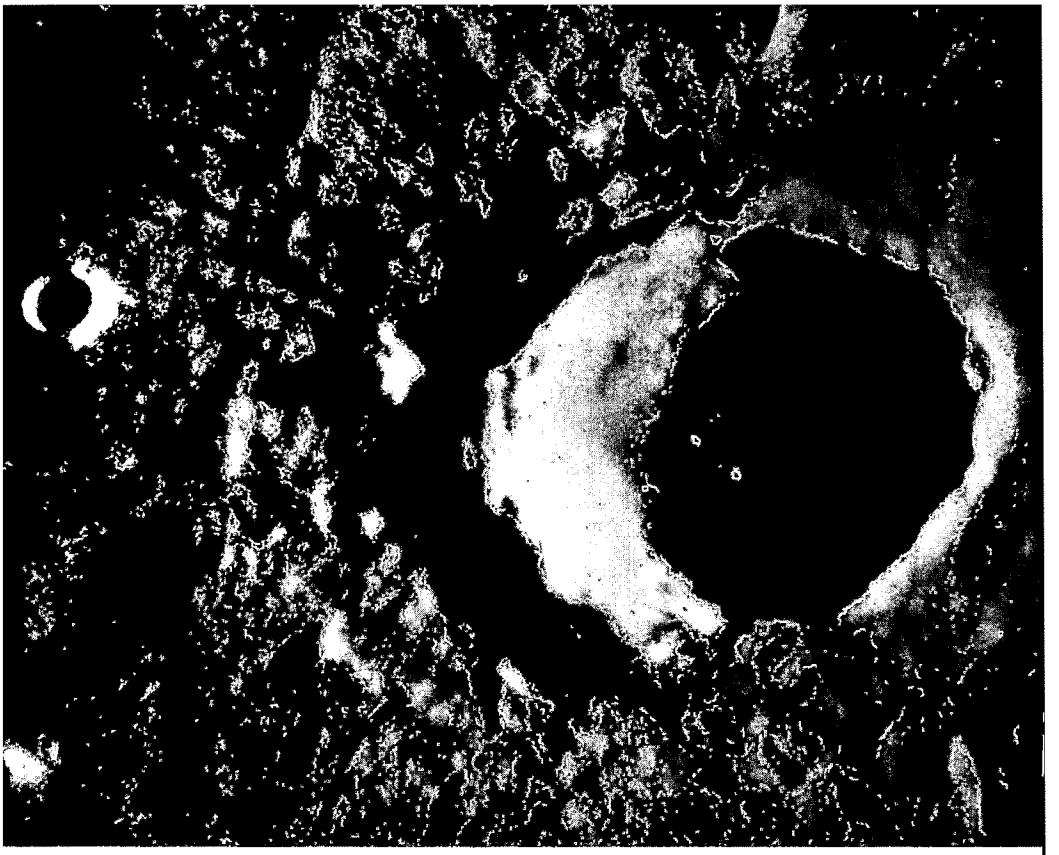


FIG. 4. Moon, region of Aristillus. Photograph from Observatoire de Meudon. The crater Aristillus, 35 miles in diameter, is surrounded by a hummocky rim that grades outward into subradial low ridges on Mare Imbrium. A larger ridge on the Mare, on which the small ridges of the Aristillus rim material are superimposed, may be seen in the upper left hand part of the photograph. The rim of a pre-mare crater partially covered with the rim material of Aristillus may be seen just above Aristillus.

volcanic craters of the type called maars. No terrestrial features are known that are closely similar both in size and form to the lunar rills.

Craters large enough that their form may be observed in some detail are unlike most crater-form features on the Earth. Certain types of lunar craters are characterized by a raised rim with a distinctively hummocky surface that grades outward into a system of low subradial ridges (Fig. 4). The floors of these craters invariably lie well below the general level of the terrain surrounding the rim and are also hummocky; a few irregular hills or peaks in all cases are found near the central parts of the floors. Of all known terrestrial craters, the only ones that resemble this type of

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niques presently in vogue, the finite-difference methods are then discussed. After surveying methods of filtering out unwanted solutions, the simple useful equivalent-barotropic model is detailed.

In lesser detail more realistic baroclinic models are described along with aspects of cyclogenesis. Following a description of procedures presently in use at the numerical forecasting center in Washington, D.C., shortcomings of the present state of the art, and unsolved problems, the book ends with a brief optimistic look into the future.

This book is most timely. Written by one of the active workers in the field, it comes when the first surge of progress has spent itself. It should not become out-of-date immediately and perhaps will help provide the impetus for the next surge forward.

While it is especially suitable as a text for graduate students in dynamical meteorology, it should also be required reading for all weather forecasters. In addition, it is so clearly written as to be readily intelligible to non-specialists, and it is to be hoped that mathematically inclined readers will be tempted by the opportunities for very useful work in the field of numerical weather forecasting. Assuredly there is room for every talent in this immense area of such usefulness to man.—*E. R. Walker*

From Miasmas to Molecules by W. Barry Wood, Jr.; 100 pages; $3.00; Columbia University Press, 1961.

In these four essays, Dr. Wood has succeeded in spanning the gap between the "two cultures" described by C. P. Snow and has demonstrated that he is both an able scientist and an eloquent humanist. By tracing the progress of medical science in the conquest of diphtheria, the author has dramatized the extraordinary power to control disease which modern biological science has placed in the hands of the physician. This story is brilliantly told in the first three essays. The opening essay depicts the spread of the great epidemic of "the Throat-Distemper" in the New England colonies from 1735 to 1740, and the helplessness, born of

lunar crater have been produced by meteorite impact or by shallow subsurface chemical or nuclear explosion (Shoemaker, 1961). In no case, however, is the resemblance complete.

The larger lunar craters exhibit a diversity of form and some are very complex, with numerous superimposed smaller craters and other topographic forms (Fig. 2). Except for the superimposed features, the great majority of the larger craters have a regular, nearly circular or slightly polygonal outline. The largest topographic features of definite form on the Moon are the great circular basins, which may be considered simply the largest class of craters. Topographically high areas encircling these basins have been named mountains, but they may be more appropriately looked upon as crater rims; they do not resemble terrestrial mountain chains. Around the basin of Mare Imbrium, the largest of the great circular basins, is a system of linear ridges and troughs (Figs. 2 and 3) oriented roughly along great circles that intersect near the center of Mare Imbrium.

Two broad classes of lunar terrain were recognized in the earliest observations of the Moon and were termed respectively terrae and maria. Though a twofold classification is somewhat misleading and incomplete, in a general way maria may be distinguished from terrae from the areal density of craters, which is about one-tenth as great on the maria as on the terrae, by a generally lower albedo (reflectivity), and by relatively smooth surfaces between the craters. The maria are everywhere lower topographically than the adjacent parts of the terrae. A variety of features of low relief are found on the maria including braided systems of low ridges, low rounded scarps, and domes. Small craters occur near the summits of some domes, which has led some investigators to liken them to small terrestrial shield volcanoes. (For an excellent description of the features on the maria, see Kuiper, 1959.)

Some of the physical properties and the texture of the lunar surface have been investigated by a variety of techniques. Photometric studies show that the limb regions of the Moon, except for local variation, are as bright as the central part of the lunar disc at full moon. This means that the lunar surface is very rough at some scale coarser than the wave length of visible light. Measurement of the variation of brightness of different small areas of the lunar surface as a function of the angle of illumination by the sun (the photometric function), has shown that all parts of the Moon's surface investigated reach maximum brightness near full moon, regardless of their position on the lunar surface. The brightness, furthermore, drops very sharply from the maximum at phases other than full moon. The forms of the photometric function for most parts of the lunar surface are satisfactorily explained by the effect of innumerable unresolved small shadows (Minnaert, 1961). Surfaces completely covered with nearly hemispherical pits, such as the surface of a

ignorance, of physicians of the period to halt its ravages. The second essay tells the exciting story of those great bacteriological discoveries between the late 1870's and the present, which have made it possible to eradicate diphtheria as an epidemic disease. In his third essay, Dr. Wood gives a beautifully lucid account of how recent biochemical investigations have illuminated the biological observations described in the preceding essay and have made it possible to understand them at a deeper level in terms of molecular interactions.

The final essay is suitably entitled "Consequences." Dr. Wood points out how the progress of medical science, as exemplified by the story of diphtheria, "has revolutionized the treatment and prevention of disease" and in so doing has presented "new problems to the medical profession—problems which are primarily social rather than scientific." He indicates the choices which scientific progress has posed, to the public which must pay for the kind of medical care it wants, to medical education which must provide the physicians to meet the rising public demand, and to the profession, in which the widening gap between scientist and practitioner must be narrowed.

This little book should appeal equally to the thoughtful scientist, physician, or layman. It will provide one or two hours of exciting reading and leave the reader with much to ponder, as he faces the future which the power of modern science has forced upon us all.—*Charles A. Janeway*

Dinosaurs: Their Discovery and Their World by E. H. COLBERT; 300 pages; $7.50; E. P. Dutton and Co., 1961.

In a recent AAAS Bulletin, which carried the surprising report that U.S. grade schoolers regard paleontology as the most interesting subject for science reading, Dr. Hilary Deason was quoted as saying "Dinosaurs appear to be giants and ogres for modern kids." Paleontology was rated third in interest for the great group of grade and high school readers combined but was only eighteenth in rank in the permanent

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piece of scoria or a rubber sponge, have photometric functions similar to those of most parts of the lunar surface.

The photometric properties of the lunar surface vary abruptly from place to place. Not only does the maximum brightness (normal albedo) vary (Fig. 1), but also, to a lesser extent, the form of the photometric function.

No striking color differences are observed visually on the Moon's surface, which appears gray to the eye, though a few faintly colored areas have been detected. The low color saturation is in keeping with polarimetric and other data which indicate the material at the surface must be finely pulverized. Photometric observations show that light reflected from the Moon is redder than sunlight, and, in general, the color index increases with albedo. The terrae are redder than the maria.

Polarization of moonlight varies with the angle between the incident sunlight and reflected moonlight (angle of vision) and generally with the albedo of lunar surface. The darker the reflecting area the greater is the polarized component of the reflected light. At angles of vision near maximum polarization, the amount of polarization changes irregularly from point to point on the Moon. The shape of the curves of polarization as a function of angle of vision for all parts of the Moon studied are such as would be produced by a powdery opaque substance (Dollfus, 1961).

In contrast to visible light, radiowaves of 10 cm wave length and longer are reflected from the lunar surface as from a relatively smooth surface. Most of the power in radar signals reflected from the Moon is returned from a relatively small region in the central part of the lunar disc by specular reflection (Senior and Siegel, 1960). Some of the signal, however, is also returned from the remainder of the lunar surface out to the limb. From the fading of the reflected radar signals, the frequency distribution of slopes may be found for lunar surface features somewhat larger than the radar wave length and smaller than the visible features of the topography. The root mean square slope of these small-scale topographic features has been computed to be between 8 degrees and 12 degrees (Daniels, 1961). The average distribution of slopes for the small unresolvable features appears to be similar to that of the larger features of the topography, which may be estimated from studies of the variation of visible shadows. By combined analysis of the range and of the spread in frequency of a reflected radar signal due to libration, Green and Pettengill (1960) have been able to construct a map which shows that the radar reflectivity of the Moon varies considerably from place to place.

Temperature and the variations in temperature of materials at and near the lunar surface have been calculated from measurements of the lunar infrared, microwave and radio emission. As determined from the infrared radiation (Pettit and Nicholson, 1930) the temperature at the lunar surface varies from about 375°K. near the subsolar point at full

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by E. J. CASEY
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book collections of the libraries discussed in the Bulletin. This extreme disproportion (largely created through book-choices by humanities-trained school librarians) can be alleviated by wide distribution of books like this one by a dinosaur expert. In it there's no threat that the dinosaurs will get you if you don't watch out. Colbert esteems the "terrible lizards" as rather familiar animals whose long-gone daily lives merit study. Through his personal researches on their bones and other records and his preparations of museum exhibits he has achieved an unusual position of authority on dinosaurs.

His book is written in a chatty, good-natured, straightforward and plain discursive style which comes close to the informality of a personal record of ideas dictated to a tape recorder. This freedom from academic stiffness (exemplified even in the bifocal use of the possessive pronoun in the subtitle) results in some repetitions, some inaccuracies ("sand is silica," "leg bones in *Allosaurus* have lost their hollow structure and have become solid"), some splendid understatements ("paleontology . . . is a science that leaves something to the imagination"), some unintended humor ("small mammals frequented the undergrowth beneath the feet of the gigantic dinosaurs"), and some illogical practices such as the use of position adjectives for time ("upper" rather than "late" Cretaceous). Many of the numerous and excellent pictures are new. Some lack scale. The maps are poor and so small as to be inaccurate and misleading, and the scale of one (opposite page 48) is badly off.

All the usual information about dinosaurs is in this volume, their ancestors, origins, kinds, relatives, tracks, eggs, distributions on earth and in time, and extinctions. There are yarns about dinosaur collectors, lists of exhibits and publications, and comments about desirable future researches. It will be the source for floods of essays in schools that are starved for up-to-date works on paleontology and it will help correct the ogre myths about dinosaurs.—*Glenn L. Jepsen*

moon to a value corresponding to a level of radiation close to the limit of instrumental sensitivity (about 125°K.) on the dark side opposite the sun. The rate of variation of temperature throughout the lunar day and rate of drop of temperature during an eclipse indicates the thermal inertia of most of the surface material is abnormally low, consistent with the properties of powdered silicates in a vacuum. Longer-wave-length emission is derived mainly from shallow depth beneath the lunar surface and reveals much less variation in temperature, as would be expected from damping of the monthly thermal wave with depth. At depths greater than 5 or 10 cm the temperature beneath most parts of the lunar surface is continuously below the freezing point of water. Values for the dielectric constant, which have been computed for the lunar surface materials on the basis of the radio emission observations, suggest an unusually high porosity. Both rate of cooling of the lunar surface during an eclipse, determined from infrared measurements (Shorthill and others, 1960), and the rate of cooling and heating during changing phase, determined from microwave emission (Coates, 1959), differ from one part of the Moon to another.

Sensitive tests for the presence of a lunar atmosphere have been made by searching for extended cusps of polarized light on the illuminated crescent moon and by observation of occultation of a radio star. A density of 10^3 electrons/cm^3 for the lunar ionosphere, derived from the observed occultation of the Crab Nebula (Elsmore, 1957), is consistent with an atmospheric pressure at the Moon's surface of the order of 10^{-13} terrestrial atmospheres. Such a rarefied atmosphere has a particle density not much greater than the particle density of interplanetary space. The extremely low atmospheric density is probably a consequence not only of the weak gravitational field of the Moon but also of a very weak or negligible magnetic field, which permits gases emitted from the Moon to be swept away by the solar proton wind (Herring and Licht, 1959). Free water could not remain long, if exposed on most parts of the lunar surface, but may be stable as ice in very cold, permanently shaded areas in craters near the poles (Watson and others, 1961) or elsewhere below the lunar surface.

Nearly all physical characteristics of the material exposed on the surface of the Moon, determined with the use of optical and radio telescopes, vary from one part of the Moon to another. Many of the variations in physical properties are closely related to differences in topography. Discontinuities are present in these areal variations, and the materials on the lunar surface are thus divisible into discrete units, each exhibiting a limited range of physical and topographic characteristics.

Different discrete units on the surface of the Moon are further characterized by distinctive patterns of distribution. The patterns of some units are in places superimposed on the patterns of other units and from

Plant Hunters in the Andes by THOMAS HARPER GOODSPEED; 378 pages, with 32 pages of illustrations; $7.50; University of California Press, 1961.

For nearly fifty years the author devoted his principal energies to the study of the plant genus *Nicotiana*, to which cultivated and wild tobaccos belong; in the field, chiefly in the Andes of South America, where source materials were obtained, and the geography and ecology of many species and races were observed; in the experimental garden at Berkeley where the genetic characters of his subjects were investigated and tested; in the laboratory where extensive cytological and morphological studies were made; and in the herbarium where complexed problems in taxonomy, phylogeny and nomenclature were worked out. This extraordinarily disciplined and integrated research program was brought to an end with the publication (1954) of his classic volume, "The Genus Nicotiana."

Thus, by mid-century, Professor Goodspeed (his predecessor in the work being Professor Setchell also of the University of California) and his colleagues had completed one of the most thorough and enlightening biosystematic studies that has been made on any large complex and geographically widespread flowering-plant genus of the world. It is upon such a profound and abundant biological basis and broad experience that the present book was written. His readers have a right to expect much from this background.

The first edition of "Plant Hunters" was published in 1941, at which time Professor Goodspeed and his collaborators had completed two of the series of six expeditions to the Andes. In these, however, his work had covered the essential regions of the natural range of *Nicotiana* in South America, the Andes from northernmost Peru to Paraguay, Argentina, Chile, to the very tip of the South American continent. The succeeding four expeditions extended detail of field work to the established framework and added a short excursion into Colombia on other interests.

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the relations of superposition it is possible to determine the relative ages of the units—the sequence in which they were formed. Bright streaks called rays, which surround certain craters such as Copernicus and Tycho (Fig. 1), are superimposed on most other mappable units on the Moon and were formed later. The material which makes up the moderately bright hummocky rim of Aristillus (Fig. 4) is superimposed on material which forms the smooth dark surface of Mare Imbrium (Figs. 1 and 4).

In most cases it may be inferred that the distinguishable units correspond to strata or to lenticular or irregular bodies of rock or rock debris. The lunar surface is locally built up of an imbricate and complexly overlapping set of layers composed mainly of material forming the rims of craters, material underlying the crater walls and floors, and material that occupies the maria. The thickness of these layers is not known though in some cases reasonable estimates of the thicknesses may be made (Marshall, 1961). The existence of the layers, however, can be recognized, and they form the principal record of the sequence of events in the history of the Moon.

Among the measurable physical properties, high resolution information on areal variation is most readily available for the normal albedo or relative reflectivity, which may be estimated from measurements of the brightness at full moon. Relative reflectivity and topographic characteristics, therefore, have been used by the U.S. Geological Survey as the primary criteria for distinguishing and mapping stratigraphic units on the Moon (Fig. 5). Some units thus recognized can also be distinguished by color, photometric function, polarizing properties, thermal inertia, or other physical characteristics. Most measurable properties are strictly surface characteristics of the stratigraphic units.

The succession of strata in the vicinity of Copernicus forms the basis for the stratigraphic classification adopted for mapping (Shoemaker and Hackman, in press). Four major stratigraphic subdivisions, termed systems, were recognized in the Copernicus region (Fig. 5), and further mapping has shown that the units in each of these subdivisions can probably be correlated over most of the visible hemisphere of the Moon. The stratigraphic units and systems are arranged in Figure 5 in order of relative age, the youngest at the top and the oldest at the bottom.

The distribution of stratigraphic units and other features south of Copernicus, near Fra Mauro, is illustrated in Figure 6. This kind of information, when plotted on a suitable base, constitutes a geological map. The geology of the Moon is being compiled by the Geological Survey on topographic maps with a scale of 1:1,000,000 that are prepared by the U.S. Air Force Aeronautical Chart and Information Center. Both the geologic and topographic mapping are being carried out to the limit of precision obtainable with existing telescopic technique. As more detailed information is acquired through lunar exploration, greater

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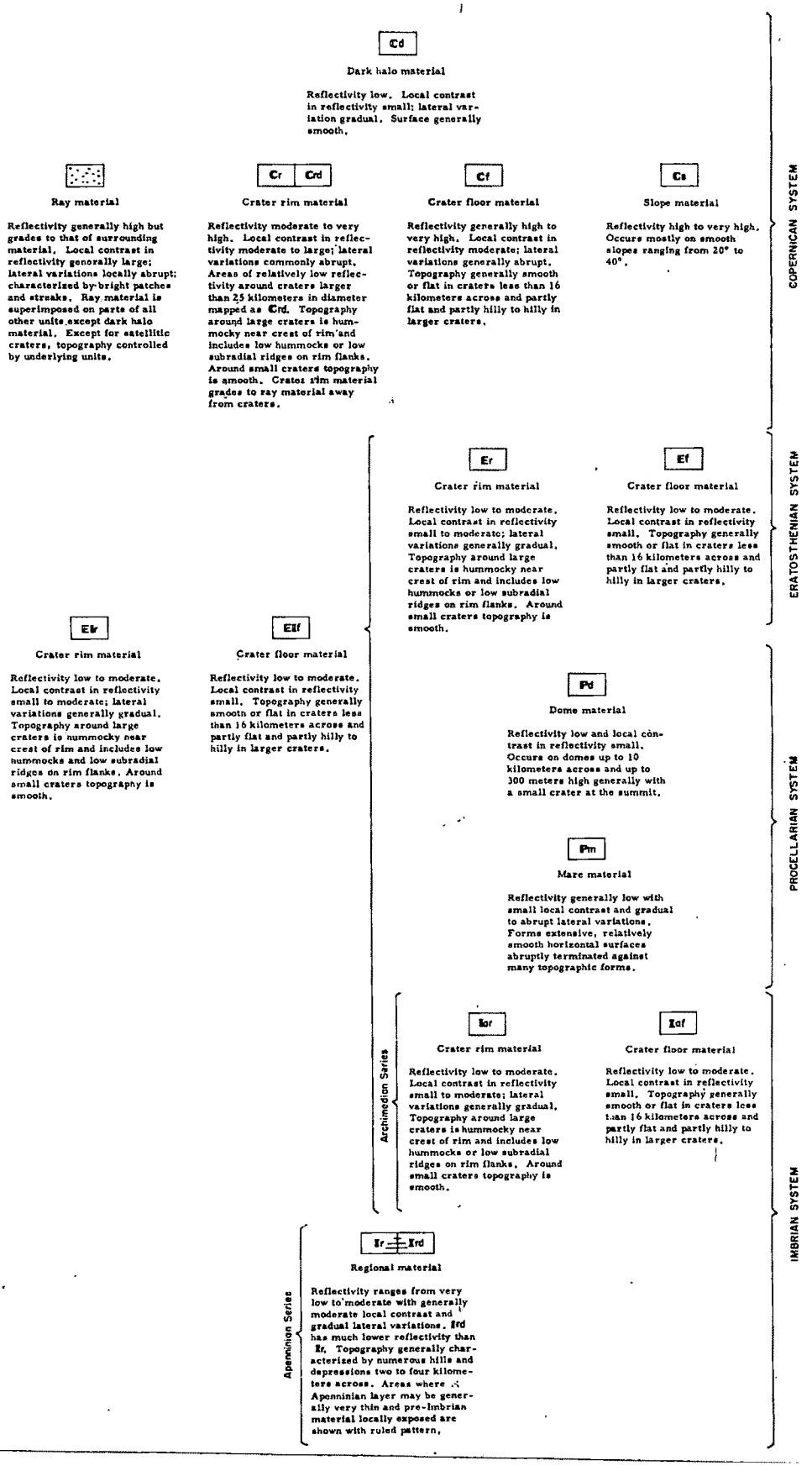
review, is a somewhat shorter rearrangement of the original; considerable portions have been rewritten to make the work contemporary, and new material has been added, chiefly in the chapter on Colombia (Taxi to Cali) and the last (Before Perón—and After), which, in part, is somewhat extraneous to the purpose and interest of the book. Presentation is largely in narrative form.

Professor Goodspeed sees much of the region through the eyes of his assistants and generously quotes from their letters and notes. Primary physiographic regions are graphically described; such as the coastal deserts (along which the Pan American Highway runs), the xeric Pacific slopes, the upland meadows of the fog belt, the high mountain plains and plateaus (punas), the great peaks, the cloud forests of the Atlantic rim and the lush slopes descending to the Amazon forest. All of these canvases are fascinating, but the chief warmth and affection of the writer seem to be directed to the spectacular high mountain and lake country of Chile and Argentina.

Considerable botanical, ecologic and geographic detail is given. In balance, the author comments on people and custom, on history and architecture. The book will be of interest alike to the botanist and the non-botanically informed. An attentive reading of the book would bring better enjoyment and understanding to any traveler to the Andes, be he naturalist or tourist.—*Bassett Maguire*

Introduction to the Study of Animal Populations by H. G. ANDREWARTHA; 281 pages; $5.00; University of Chicago Press, 1961.

Textbooks of ecology have been multiplying in recent years. This could mean that the intellectual environment is becoming more favorable for the subject; or, contrariwise, that the subject itself has changed, becoming more suitably adapted to the academic climate. Probably both things are involved: we are more and more aware of the need for understanding of the biological environment; and ecologists


Cd
Dark halo material
Reflectivity low. Local contrast in reflectivity small; lateral variation gradual. Surface generally smooth.
Ray material
Reflectivity generally high but grades to that of surrounding material. Local contrast in reflectivity generally large; lateral variations locally abrupt; characterized by bright patches and streaks. Ray material is superimposed on parts of all other units except dark halo material. Except for satellitic craters, topography controlled by underlying units.
Cr
Crd
Crater rim material
Reflectivity moderate to very high. Local contrast in reflectivity moderate to large; lateral variations commonly abrupt. Areas of relatively low reflectivity around craters larger than 2.5 kilometers in diameter mapped as Crd. Topography around large craters is hummocky near crest of rim and includes low hummocks or low subradial ridges on rim flanks. Around small craters topography is smooth. Crater rim material grades to ray material away from craters.
Cf
Crater floor material
Reflectivity generally high to very high. Local contrast in reflectivity moderate; lateral variations generally abrupt. Topography generally smooth or flat in craters less than 16 kilometers across and partly flat and partly hilly to hilly in larger craters.
Cs
Slope material
Reflectivity high to very high. Occurs mostly on smooth slopes ranging from 20° to 40°.
COPERNICAN SYSTEM
Er
Crater rim material
Reflectivity low to moderate. Local contrast in reflectivity small to moderate; lateral variations generally gradual. Topography around large craters is hummocky near crest of rim and includes low hummocks or low subradial ridges on rim flanks. Around small craters topography is smooth.
Ef
Crater floor material
Reflectivity low to moderate. Local contrast in reflectivity small. Topography generally smooth or flat in craters less than 16 kilometers across and partly flat and partly hilly to hilly in larger craters.
ERATOSTHENIAN SYSTEM
Elr
Crater rim material
Reflectivity low to moderate. Local contrast in reflectivity small to moderate; lateral variations generally gradual. Topography around large craters is hummocky near crest of rim and includes low hummocks and low subradial ridges on rim flanks. Around small craters topography is smooth.
Elf
Crater floor material
Reflectivity low to moderate. Local contrast in reflectivity small. Topography generally smooth or flat in craters less than 16 kilometers across and partly flat and partly hilly to hilly in larger craters.
Pd
Dome material
Reflectivity low and local contrast in reflectivity small. Occurs on domes up to 10 kilometers across and up to 300 meters high generally with a small crater at the summit.
Pm
Mare material
Reflectivity generally low with small local contrast and gradual to abrupt lateral variations. Forms extensive, relatively smooth horizontal surfaces abruptly terminated against many topographic forms.
PROCELLARIAN SYSTEM
Archimedian Series
Ilr
Crater rim material
Reflectivity low to moderate. Local contrast in reflectivity small to moderate; lateral variations generally gradual. Topography around large craters is hummocky near crest of rim and includes low hummocks or low subradial ridges on rim flanks. Around small craters topography is smooth.
Ilf
Crater floor material
Reflectivity low to moderate. Local contrast in reflectivity small. Topography generally smooth or flat in craters less than 16 kilometers across and partly flat and partly hilly to hilly in larger craters.
Apenninian Series
Ir
Ird
Regional material
Reflectivity ranges from very low to moderate with generally moderate local contrast and gradual lateral variations. Ird has much lower reflectivity than Ir. Topography generally characterized by numerous hills and depressions two to four kilometers across. Areas where Apenninian layer may be generally very thin and pre-Imbrian material locally exposed are shown with ruled pattern.
IMBRIAN SYSTEM

are striving to become orthodoxly scientific. They have become much addicted to quantification, to attempts at expressing relationships with mathematical formulations, and to the construction of elegant conceptual models. H. G. Andrewartha, Reader in Animal Ecology at the University of Adelaide, is a fine example of this ecological trend. In fact, he includes a rather long discussion of the "scientific method" in the middle of his book, *Introduction to the Study of Animal Populations.* This, like most such discussions, simply leaves me puzzled.

Andrewartha believes that attempts to analyze complex biological communities have failed, and that the most fruitful approach to general ecology is through the study of the behavior of particular populations. He discusses populations under such headings as density, dispersal, and relations with specific components of the environment, tying this all together with a chapter on "Theory: The Numbers of Animals in Natural Populations." Since he is writing a textbook, he concludes with a section on laboratory exercises for population study.

Andrewartha's point of view is different from that of many of his ecological colleagues, which means that they would do well to give careful consideration to his ideas. I doubt, however, whether the general reader would gain a very well-rounded impression of population ecology from such a special sort of approach.—*Marston Bates*

The Fire of Life (An Introduction to Animal Energetics) by MAX KLEIBER; 454 pages; $11.50; John Wiley & Sons, 1961.

Kleiber's book fills an urgent need. In the continuous flood of reports of research on problems that happen to be in vogue, "classical" problems of physiology are apt to be bypassed - which does not mean that they are solved or have become uninteresting. Among these, the problems of global animal metabolism are not the least. Here, then, is a textbook which reviews the

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Rill and chain-crater material

Material of generally low to moderate reflectivity lying within narrow linear depressions with associated small craters or within linear series of craters.

Copernican satellitic craters

Small, relatively shallow craters occurring in rim material and rays around large Copernican craters. Satellitic craters are commonly composite or elongate with very low rims or no observable rims.

Eratosthenian satellitic craters

Small, relatively shallow craters occurring in rim material and in region surrounding rims of large Eratosthenian craters. Satellitic craters are commonly composite or elongate, with very low rims or no observable rims.

Contact

Dashed where approximately located.

Indefinite contact

(Pm)
(Ir)

Concealed contact

Symbols in parentheses indicate the concealed units:

(CEr) Copernican and Eratosthenian crater rim material, undifferentiated
(CEf) Copernican and Eratosthenian crater floor material, undifferentiated
(CIr) Copernican to Archimedian crater rim material, undifferentiated
(CIf) Copernican to Archimedian crater floor material, undifferentiated
(pIr) Pre-Imbrian crater rim material
(pIf) Pre-Imbrian crater floor material

U
D

Offset of surface

Showing foot of scarp separating regions of similar topography. U, upthrown side; D, downthrown side.

Lineament

Partially or completely covered. Inferred from linear topographic features.

Mare ridge

Showing crest line. Tapered end indicated by arrowhead. Dotted where concealed.

Mare trough

Showing trough line. Tapered end indicated by arrowhead. Dotted where concealed.

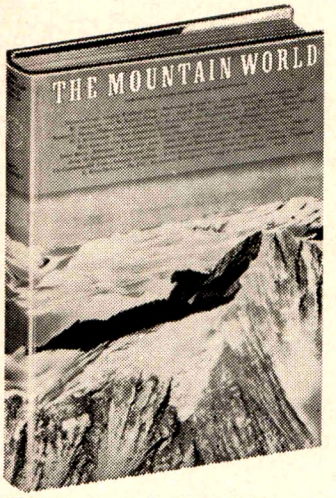
Rounded mare scarp

Showing trace of foot of scarp. Barbs point in direction of slope. Dotted where concealed.

Outer limit of telescopically observable low hummocks or low subradial ridges on Copernican crater rim material.

FIG. 5. Explanation of stratigraphic units and other features on the Moon mapped by the U. S. Geologic Survey at a scale of 1:1,000,000.

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field in modern garb; the importance of the enterprise is apparent from the fact that no comparable presentation in English has appeared since Brody's "Bioenergetics and Growth" of 1945.

Kleiber's forte is a remarkable pedagogic aptitude. Individual aspects of metabolism are used as introduction to wider fields. The chapter on starvation gives opportunity for a brief outline of basic concepts of statistics and explanation of the fundamentals of calculus; that on respiration interestingly reviews the various machines to determine metabolic rate, ranging from Lavoisier's simple glass bell in 1784 to Cartesian divers, Warburg apparatus and other modern devices. The energetics of food consumption opens a view toward the problem of population explosion. One will not often find in textbooks such happy introduction of wider viewpoints and history of science.

However, there are shortcomings. The "classic" character of the problem leads to neglecting viewpoints apparent from recent research. For example, in the treatment of bioenergetics one reads: "The law of entropy has been developed for closed systems and may not directly apply to open systems, such as animals. This has been emphasized particularly by Bertalanffy (sic!)." This is a rather objectionable formulation of the elaborate theory of Irreversible Thermodynamics. The insufficient inclusion of newer research is particularly disappointing in the field where the author made his own original contributions. As is well-known, the classic surface rule of metabolism was altered by Kleiber and Brody into the 3/4-power rule of dependence of metabolic rate on body weight. However, the chapter treating this important problem essentially remains Kleiber's presentation of 1947 (itself a classic in the field) with few additions. As this reviewer has incessantly emphasized, one simply cannot discuss the surface rule and related problems by considering mammals alone. Reviewing the various interpretations of the surface rule, the author concludes that the old theory of Bergmann and Rubner in terms of maintenance of constant body tem-

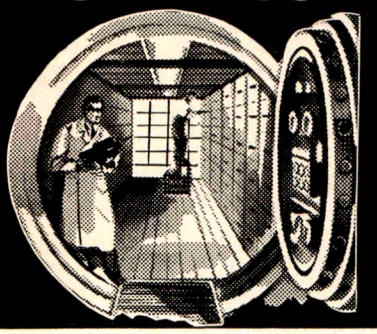
refinement in recognition of the chronologic sequence of strata will be possible and greater precision in the discrimination and location of geologic units and structures is to be expected.

A rough estimate of the maximum age of stratigraphic units on the lunar surface may be made from the rate of formation of craters by impact of astronomical objects. From data on meteorite falls, the mass distribution of the asteroids, and the terrestrial record of impact, the rate of formation of impact craters on the Earth over the past half billion years has been estimated to be on the order of 100 to 300 craters 3 km in diameter and larger per million square kilometers per billion years (Shoemaker and others, 1961). The estimated rate of impact crater formation on the Earth is an inverse exponential function of the crater diameter resembling the size frequency distribution function of lunar craters. If the estimated rate of impact is extended as constant back over geologic time, and appropriate corrections are made for the different effects of the gravitational fields of the Earth and the Moon, all the craters on the maria of the Moon larger than 3 km in diameter may be accounted for if the maria are about as old as the Earth (about 4.5 billion years). This result is consistent simultaneously with the hypotheses that most of the craters on the maria are of impact origin and that the maria are indeed very ancient features on the Moon (Baldwin, 1949; Urey, 1952; Kuiper, 1954). The craters on the terrae, however, are too abundant to be accounted for by the present rate of impact and must be explained in some other way. Adopting an age of 4.5 billion years for the maria, the age of younger stratigraphic units may be estimated from the distribution of superimposed craters. The age of the rim material around the crater Copernicus, on this basis, is found to be about half a billion years (Shoemaker, 1961). A large probable error must be attached to this estimate owing to the combined uncertainties in the estimated rate of cratering by impact and to statistical uncertainties of counting for the small number of resolvable superimposed craters. If the age of a few stratigraphic units on the Moon were established by independent methods and much higher resolution photography were secured from lunar spacecraft, great strides could be made in determining both the relative and absolute ages of the strata on the lunar surface from the distribution of small craters.

Another consequence of the impact of objects on the lunar surface is that some material from the Moon almost certainly arrives on the Earth. Experimental studies of hypervelocity impact of particles on rock targets shows that meteoroids striking the Moon, at speeds similar to the observed geocentric speeds of meteors, probably eject many times their own mass from the lunar surface at speeds greater than the escape velocity. As the Moon is exposed to about the same flux of high velocity particles as the Earth a nearly continuous spray of moon dust is ejected into

FIG. 6. Annotated photograph of an area on the Moon near Fra Mauro. Geology by R. E. Eggleton, photograph from Mount Wilson Observatory. See FIG. 5 for explanation of symbols.

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upper atmosphere. "Who is sufficient for these things?" The bibliography includes over 1600 items covering 78 pages; it will be most useful. The author avows that his main incentive in writing the book was the wish to have some familiarity with all phases of its subjects. His immense efforts and good judgment and interesting treatment will enable many others to attain the same objective, and thereby will certainly much advance the subject. To be helpful when the book is used as a basis for graduate courses, problems practical and theoretical are appended to each chapter. Its thirteen chapter titles will give some slight indication of the value and wide range of this excellent book. They are: Radiation in spectral lines; Scattering of radiation in finite atmospheres; Magnetic fields, charged particles, and the upper atmosphere; Occurrence of aurorae in space and time; Auroral spectroscopy and photometry; The radio-aurora; Physical processes in the auroral atmosphere; Auroral particles in space; The airglow spectrum; Analysis of twilight observations for emission height; Theory of the twilight and day airglow; Spectral photometry of the nightglow; Excitation of the nightglow.—*Sydney Chapman*

Quantum Theory. Vol. I., Elements, edited by D. R. BATES; 447 pages; 30s; Academic Press (London), 1961.

Texts on quantum theory are so diverse and numerous that the title of this series of volumes cannot convey its rather unique character. Volume I, Elements, is the first of the projected three volume sequence. (Tables of contents of Vol. II, Aggregates of Particles, and for Vol. III, Radiation and High Energy Physics, are given in the present volume.) The audience to which this treatise will be useful cannot be more clearly stated than to quote Professor Bates' preface: "*Quantum Theory* . . . is intended as an advanced text and reference on the fundamentals and applications of quantum theory. It is primarily designed to meet the needs of (graduate and) post-graduate students. The hope is that it will enable

space, some of which will be directed at the Earth. Moon dust, therefore, must be a constituent of the Earth's atmosphere and of every sedimentary rock on the Earth, though it has never been identified.

It is clear from the results of the impact experiments why the stratigraphic units of the lunar surface are not covered up by a uniform blanket of cosmic dust. Most, if not all of the mass of the high speed particles that hit the Moon, as well as the moon dust, is returned to space. The effect of bombardment by small high speed meteoroids is gradually to grind down the lunar surface. At least a thin layer of pulverized lunar material that has not been accelerated to escape velocity, however, will always be present.

The impact of large meteorites must occasionally launch larger fragments of the Moon on ballistic trajectories to the Earth. The question is whether such free samples from the Moon have ever been recognized. Urey (1959) has suggested that the stony meteorites may be such objects, and Nininger (1943), Varsavsky (1958), Chapman (1960), and O'Keefe (1960), following earlier workers, have advocated that certain siliceous glassy objects known as tektites are derived from the Moon. It is possible that both tektites and some stony meteorites are of lunar origin.

The physical exploration of the Moon that is about to begin opens up new vistas of scientific opportunities. Shortly we will be able to bring to bear the whole spectrum of powerful tools and techniques that have been developed in the study of the Earth. From the orbit of a close satellite about the Moon it will be possible to obtain a very precise determination of the lower harmonics of the Moon's figure and thus a far more accurate estimate of the internal stresses and limits on the possible distribution of density. Seismometers placed on the lunar surface would provide direct information on the internal structure of the Moon and the mechanical and thermal processes now occurring there. Chemical and mineralogical analysis of samples from different stratigraphic units on the Moon's surface would provide the basic information for determining whether and by what processes the Moon may have been chemically differentiated.

Beginning with the physical exploration of the Moon the sciences of geology and geophysics will become broadened to the comparative study of planets. Some of the principal scientific rewards will be new insights into major geological processes, the internal constitution, and history of the Earth. In particular, data from the Moon may be expected to provide critical evidence on fundamental geological questions such as the origin of the Earth's crust and the causes of mountain building.

If rocks of granitic composition are widely distributed over the surface of the Moon, as implied by the hypothesis that tektites are of lunar origin, the demonstration of this will have profound influence on our

them to refresh and deepen their understanding of the elementary parts of the subject, that it will provide them with surveys of the more important areas of interest, and that it will guide them to the main frontiers on which advances are being made. In addition, teachers at universities and institutes of technology may find the compilation, which is unusually wide in scope, useful when preparing lectures." Volume I would be suitable as textual and reference material for the second and third term quantum mechanics courses of most physics graduate schools in American universities.

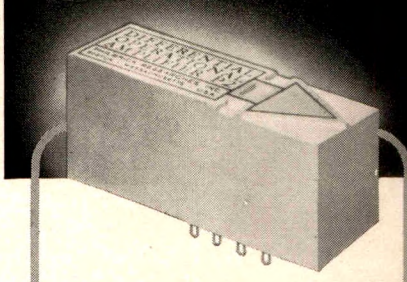
The material set forth in Volume I is that which finds wide application in electronic structure problems and classical nuclear physics. Although written by different authors (each a competent specialist in the field on which he or she writes), the chapters are not a collection of individual research papers and in general they are clearly written and not particularly difficult.

A large part of the material in this volume may be found in other texts or tutorial survey articles, but this material has nowhere been brought together and presented in so satisfactory a manner.—*Leland C. Allen*

Mathematical Handbook for Scientists and Engineers by G. A. KORN and T. M. KORN; 943 pages; $20.00; McGraw-Hill Book Co., 1961.

Even a cursory examination of this volume shows it to be a considerable work, if only for the typography, which involves a greater variety of symbols than most handbooks or mathematics texts. The symbolism is necessary because of the scope, as indicated by a sampling from the 21 chapters: vector analysis, curvilinear coordinate systems, Laplace transformation, partial differential equations, abstract algebra, matrices, eigenvalue problems, tensors, probability, and finite differences. In addition, there are included the many other topics that would be expected from this sampling, in basic and advanced algebra, geometry, and calculus; no significant omission is noted by this reviewer.

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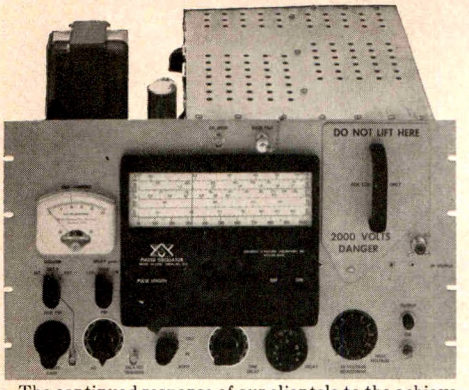
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FIG. 6. Annotated photograph of an area on the Moon near Fra Mauro. Geology by R. E. Eggleton, photograph from Mount Wilson Observatory. See FIG. 5 for explanation of symbols.

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The authors intended the handbook not only as a reference but as a means for rapid reviews of entire mathematical subjects. However, the discussions are not as complete as might be desirable for this, although they are very thoroughly cross-referenced, indicating the interrelations of many superficially different mathematical topics. The emphasis is upon rigorous mathematical statements without proofs, and frequent but brief mentions of applications. Therefore, the handbook is a valuable reference, but not a substitute for the excellent recent applied mathematics texts. It should inspire further reading from the bibliography provided with each chapter.

It is well arranged, with boxes, large print, and small print to indicate successively less important material. The chapters are amply subsectioned, and the index can serve as a mathematical dictionary. The appendix has 150 pages of numerical tables. There is a long but incomplete notation list, and the notation throughout the volume seems as consistent as possible considering the diversity of subjects.

A few random samples may illustrate the work. Eight symbols for various types of equality are given in the notation list, but $\doteq$ and $\rightarrow$ are omitted ($\rightarrow$ is used in the text). "Complex potential," and "streamlines" are in the index; "velocity potential" is not, although it is in the text. Green's function, the weighting function, transfer functions, superposition, and the convolution integral are completely related by cross-referencing, but it would be difficult for the beginner to understand the relationships by reading the handbook alone. Liapounoff's first stability theorem is briefly treated; the important second theorem is not.

The omissions do not significantly detract from the excellence of the book, which is the more remarkable for being entirely the work of a relatively young electrical engineering professor and his wife; the only indication of assistance is the acknowledgment to three mathematics professors who read parts of the manuscript. The authors are to be admired for their accomplishment.—*Peter L. Balise*

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FIG. 6. Annotated photograph of an area on the Moon near Fra Mauro. Geology by R. E. Eggleton, photograph from Mount Wilson Observatory. See FIG. 5 for explanation of symbols.



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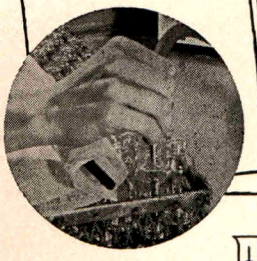
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an aid in the elucidation of puzzling field observations. Construction engineers might look on the information as a guide to the origin of dimensionally oriented structures and inhomogeneities in various kinds of rocks.

The appendix contains such added data as a check list of equipment required for various kinds of survey work, standard abbreviations, visual percentage estimate charts, geologic symbols, isogonic chart of the U.S., and tables of natural trigonometric functions. The volume is well illustrated with clear sketches and diagrams.

The author has kept the size of his volume down by pointing out references for many marginal phases of geologic endeavor as well as including many for modern mapping techniques and detailed field studies.—*J. Kalliokoski*

Plastic Flow and Fracture in Solids (Vol. II of Mathematics in Science and Engineering) by TRACY Y. THOMAS; 267 pages; $8.50; Academic Press, 1961.

This book is based to a large extent on articles published by the author within the last five years and is devoted to the study of the propagation, growth, and decay of discontinuities in solids, including the theory of characteristic surfaces and the spread of plastic into elastic regions. The treatment of waves in elastic-plastic solids will also be of interest to those people who are concerned with applications other than fracture of solids.

The author begins with the development of the equations of continuity, motion, and compatibility, then discusses waves in elastic media and the constitutive equations for perfectly plastic solids. The equilibrium theory of Luder's bands is discussed as an appendix to the first part of the book. The last part of the book is devoted to characteristic surfaces, wave propagation, instability and fracture. Of particular interest is the prediction of fracture surfaces for closed circular pipes under internal pressure and axial tension, for flat plates and round bars in tension, and for round bars in tension.

space, some of which will be directed at the Earth. Moon dust, therefore, must be a constituent of the Earth's atmosphere and of every sedimentary rock on the Earth, though it has never been identified.

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If rocks of granitic composition are widely distributed over the surface of the Moon, as implied by the hypothesis that tektites are of lunar origin, the demonstration of this will have profound influence on our

These predictions are in excellent agreement with measured results.

Tensors are used in the development of the book (Vol. 1 of the series is "Concepts from Tensor Analysis and Differential Geometry" by the same author). The book is suitable for a graduate course and should be required reading for all persons interested in solid mechanics.—*Robert E. Sherrer*

Chemistry of Carbon Compounds, Vol. IVC of Heterocyclic Compounds, edited by E. H. Rodd; pages 1465-2201; no price given; Elsevier (D. Van Nostrand Co.), 1960.

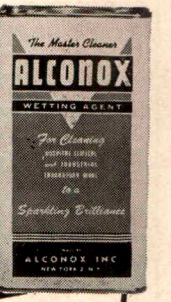
With this volume of "Chemistry of Carbon Compounds," Rodd concludes his survey of heterocyclic chemistry. The topics covered are the most diversified yet reviewed in this series. As is the case with books of this type, each field is expanding with such rapidity, that it requires an expert in that particular field to do it justice. To help him in the current survey Rodd has summoned to his aid a selection of England's top organic chemists, and together they have produced a volume commensurate with others of this series.

Aside from the early chapters which complete the survey of those six-membered heterocycles with two or three hetero atoms, this volume contains chapters on a wide range of natural products. The purines, alloxazines, pteridines, nucleosides, nucleotides, nucleic acids together with the nucleotide coenzymes, vitamins B_1 and B_2, and other cellular components are all covered. The final seven chapters are devoted to the alkaloids, the bulk of which have been competently reviewed by Dr. A. R. Pinder (Cardiff). A number of the articles have been reviewed elsewhere, in particular the alkaloid section which is very similar to that presented in the latest volume of Manske. The best chapter in the book is the article on the seven-membered or larger heterocycles, by Dr. D. M. Maitlis (London) whose presentation is both timely and well written. In this chapter the literature is comprehensively covered and well referenced and is surprisingly free from annoying errors.

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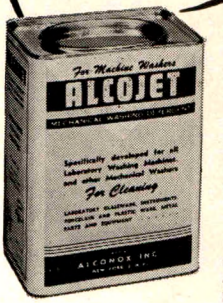


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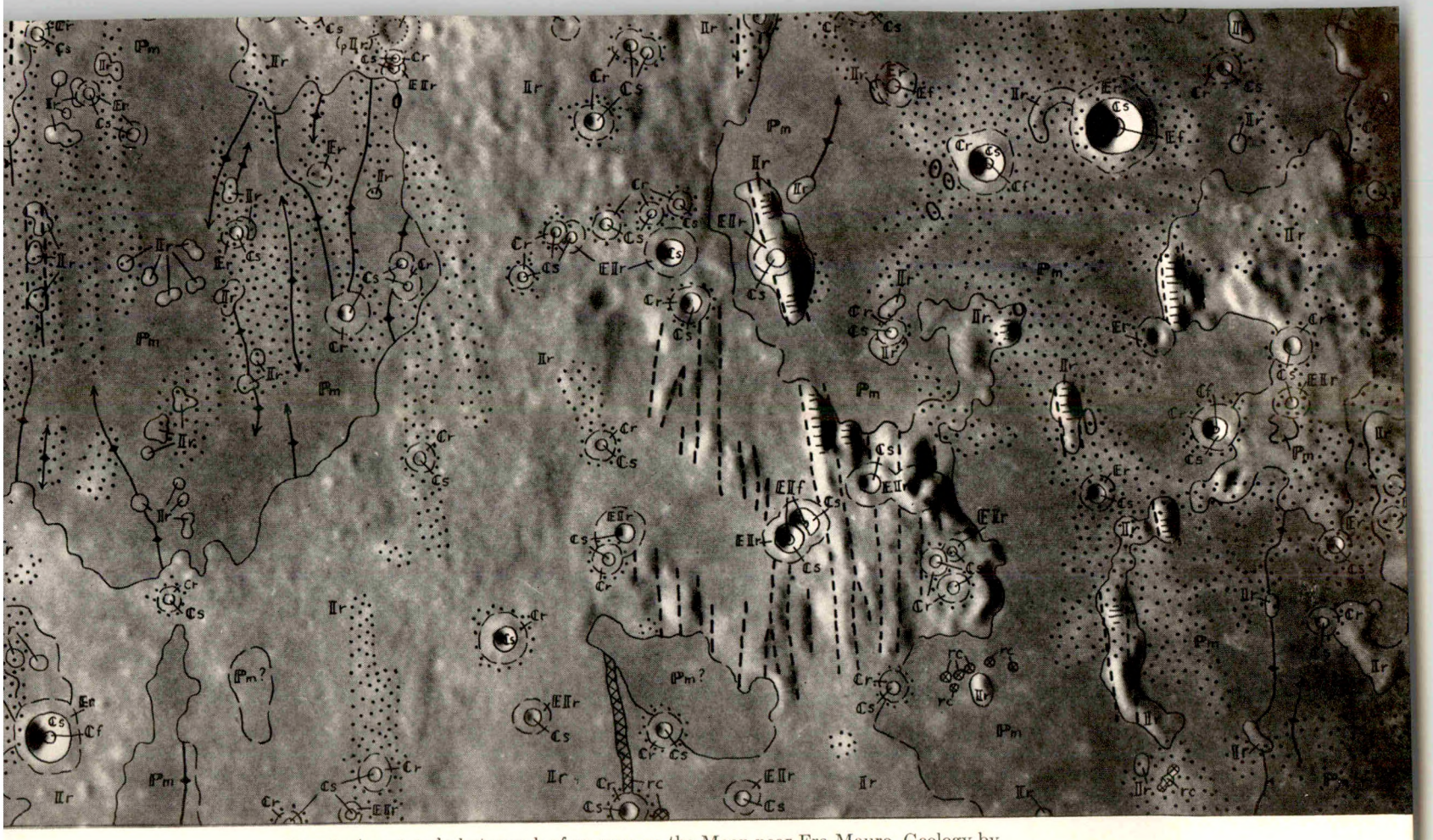


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There are a number of comments on this particular volume which seem to be characteristic of this series. The worst feature is the large number of printing errors in the presentation of formula and in referencing. This is typified by the chapter on purines, by Dr. G. A. Howard (Warrington) which appears to have escaped proof reading. A check on the accuracy of the references was taken by randomly selecting two pages from each chapter. Four of those selected from the first eight chapters had at least one mistake! As this type of book is primarily a reference book it is essential that it should be reliable and accurate.

The presentation of work in the articles is certainly in accord with Rodd's introduction to the first volume "a work concerned much more with fact than with theories of organic chemistry." It is subtitled, however, "Modern Comprehensive Treatise." It is modern in that the literature is covered in the better chapters until 1959 (although the book did not appear until late in 1960), it is comprehensive, but it is not strictly a treatise. It is too documentary and fails to stress the principles involved and to draw conclusions from the facts presented. The reviews are not critical, but merely compilations of data from the literature. The few attempts in the book to present any mechanisms are not clear, and are poorly presented arrow-wise (*c.f.* p 1755 and p. 1609, the latter being confused because the central arrow should be absent).

The inconsistence of formulation, using both the conventional = and also the : to represent the double bond (frequently in the same formula) is unsatisfactory.

The method of referencing in this volume, where the author's name and the relevant literature reference occur in the body of the text, has now been discarded by all the better known chemical journals, as it is too difficult to trace individual references. The use of this system of referencing without an author index detracts largely from the usefulness of the book.

The need for reference books of this type is a real one, but this book leaves much to be desired.—*Ronald N. Warrener*

Elements of Maser Theory by ARTHUR A. VUYLSTEKE; 362 pages; $9.50; D. Van Nostrand Company, 1960.

Maser, or microwave amplification by stimulated emission of radiation, owes its theory mainly to quantum mechanics and its practice to quantum electronics, a relatively new subject. This book bridges the gap between academic learning and the applied technology of masers. It is written on an elementary level. The author's detailed attention to derivations of equations and to algebraic manipulations may be boring to an advanced reader, but must be welcomed by those who want to learn theory and also want to see how it comes about in all steps.

In spite of the book's preponderance in equations, the author excels in explaining in lucid style many fundamental concepts of physics necessary

space, some of which will be directed at the Earth. Moon dust, therefore, must be a constituent of the Earth's atmosphere and of every sedimentary rock on the Earth, though it has never been identified.

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interpretation of the processes in the Earth. A modern concept of the differentiation of the Earth's crust holds that granitic rocks are produced mainly through intermediate processes of erosion and sedimentation and are thus a consequence of the presence of an atmosphere and hydrosphere. On the Moon the formation of granitic rock would most likely have taken place by processes of igneous differentiation—fractional melting and fractional crystallization—somewhat along the lines envisioned for the Earth's crust by N. L. Bowen (1928). If, on the other hand, the surface of the Moon proves to be made up chiefly of rocks with high density ferromagnesian minerals, for example rocks like the meteorites as suggested by Urey, then the initial assumptions in the concept of gradual evolution of the Earth's crust under the effects of sedimentary differentiation will have received important support.

Comparison of the structural features of the surface of the Moon with those of the Earth, in conjunction with detailed information on the seismic activity and internal structure of the Moon and on the present heat flow through the lunar surface, will almost certainly enhance our understanding of the causes and processes of mountain building disturbances in the Earth. The Moon is of special interest for comparison because folded mountain chains, which are major structural features of the crust of the Earth, appear to be absent on the Moon. Lack of major lateral offsets in the walls of craters and their prevailing subcircular form seem to indicate that large lateral displacements, such as are observed on the Earth, have not taken place on the Moon, at least not during its decipherable history. The differences between the Moon and the Earth may therefore be expected to reveal diagnostic clues to the causes of mountain building.

The history of the Moon will be the special scientific objective in exploring the Moon's surface, and it is its history that makes the Moon an object of unusual interest among those that we may explore in the solar system. Because of the lack of an atmosphere, the surface of the Moon has been continuously exposed to the flux of radiation and solid particles in the vicinity of the Earth, and the record of events etched upon the lunar surface has not been washed away or buried by the unremitting action of wind and water. The strong possibility that the accessible parts of this record extend back to a very early stage in the history of the solar system gives the study of the Moon unique scientific importance. The record, the key to the dimension of time, lies in the stratigraphy, which will be solved by geologic mapping of the surface. The most important scientific objective for sending a man to the surface of the Moon will be to crack the unsolved parts of the stratigraphic code.

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for the understanding of their technological applications. In the introductory chapter, old quantum theory is surveyed; statistical mechanics is introduced from consideration of the relationship between macroscopic entropy and microscopic *a priori* probability of quantum states. The principles of maser theory are touched upon from the simple radiation theory view point. This is followed by a chapter on quantum theory and a chapter on statistical mechanics which provide the theory and language for the later chapters and serve to make the book self-contained. The following chapter on interaction of radiation and matter treats time-dependent perturbation in detail, considered signals, line shapes and includes an instructive section on statistical matrix formulations.

With the foundations thus well laid, the author then proceeds to two-level excitation in which the pulse inversion method, inversion by adiabatic rapid passage, as well as the molecular beam ammonia maser are presented. As a preliminary to later treatment of three-level masers, the maser levels of magnetic substances and the effect of crystal field are discussed. This is supplemented by appendices on exchange energy and spin-orbit splitting of a cubic field ground state. The chapter on three-level excitation presents solid state masers and discusses three-level gas masers. The subjects on masers are limited in this book but are all thoroughly treated. It is therefore a good introduction to the applied technology in this field and good reading for those who want to understand masers, the theory of which they may have learned in academic texts of quantum and statistical mechanics but have not realized the potential of its lively applications and usage as brought out in this book.—*Y. N. Chiu*

Mankind Evolving by THEODOSIUS DOBZHANSKY; 369 pages; $7.50; Yale University Press, 1962.

Dobzhansky's personal genes and environment qualify him almost uniquely to write this book: he is a clear and lively writer whose background in, and own contributions to, genetics give him mastery of this essential approach to mankind's origin and variety. He deals with past evolution but his emphasis is on racial and individual differences of the present, and on selection, fitness, population dynamics. He never loses sight of the mundane meanings of these things, which is why the book should have special interest for ordinary readers: it is actually a modern view of eugenics, in the best sense. Other such books have had chapters covering blood groups, or basic genetics, or effects of radiation, or race differences, but here the achievement is in handling and synthesizing these things everywhere as related aspects of a fully developed biological view of man, rather than as topical expositions. With such a book we leave far behind simpler "genetic" treatments of man (heredity vs. environment, dominant brown eyes, etc.). Beginners in genetics reading Mendel's experiments must have usually formed the impres-

of the maria. The majority of the craters on the Moon are pre-mare in age. This period of rapid cratering may have been the terminal phase of sweeping up of solid objects left in the vicinity of the Earth during the condensation or accretion of the planets. It is not clear whether any "original" or undisturbed surface will be found on the Moon, as suggested by Kuiper (1954, p. 1104). My impression from study of the terrae is that all parts of the terrae lying outside of craters may be covered with the rim material or with more distant ejecta from very large craters. The search for an original surface on the Moon may prove illusory as it has on the Earth.

A fairly complete record of the cosmic radiation during geologic time may be preserved by the stable spallation products formed in the different stratigraphic units. Those units which have been continuously exposed since their formation will exhibit the integrated effects of cosmic radiation from the time they were formed. Elsewhere, units that have been sufficiently deeply buried under later strata will preserve only the effects of radiation for the period they were exposed. In principle, it should be possible to work out a very detailed picture of the history of high energy radiation in the vicinity of the Earth, which would be a record of outstanding importance for astrophysical theory and cosmology.

In a similar fashion, a partial record of the flux of solid particles may be preserved on buried surfaces, though most of the solids impinging on the Moon will be returned to space. The record will consist mainly of the craters formed. Among the few solids arriving at sufficiently low speed to "stick" there may be living or dead microscopic organic forms (Lederberg, 1960). If prebiotic evolution or formation of organic chemicals has taken place in the surface materials of the Moon, this too will be recorded in the stratigraphy.

The most exciting and perhaps the most fruitful discoveries to be made on the Moon, however, will be the ones which are entirely unanticipated. If experience with the Earth proves to be any guide, the surface of the Moon will probably present far more complex problems than anyone has imagined. It is to seek the unknown, after all, that we are exploring space.

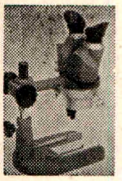
The American Program and the Strategy of Exploring the Moon's Surface

The national program of lunar exploration has been developed in two somewhat distinct phases, the first concerned mainly with unmanned exploration and the second with landing men on the Moon. A program of unmanned lunar exploration was already at an advanced stage of planning and development in the spring of 1961 when the manned lunar mission was established as a major objective of the National Aeronautics and Space Administration. This decision placed new requirements on

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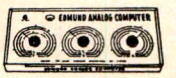
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sion than heterozygotes are somehow unnatural and that selection pushes for the successful homozygote—now we have a book from one who has done so much himself to establish the view that fitness normally resides in the heterozygote, and who can illustrate directly from *Drosophila* his well-known dictum that human races are not individual types but populations, differing not absolutely but in gene proportions. Using careful explanation of often difficult subjects, and a fine gift for metaphor, he does not write down but does not get out of an ordinary reader's reach. The treatment of race is excellent. It is too sensible to appeal to doctrinists of the racial right or left, but it could serve as just the kind of explanation needed by citizens who are puzzled by our own racial problems, and perhaps partly persuaded by the arguments of the present crop of racists. May the book have big sales.—*W. W. Howells*

the unmanned phase of the program.

Now that a decision has been reached to try to place men on the Moon, the first step must be to obtain detailed information about physical characteristics of the lunar surface for use in selecting potential landing sites. One of the over-riding considerations in the program to send men to the Moon (the Apollo program) is the safe landing and return of the men from the Moon. To insure the safety of the mission, it will be necessary to ascertain the roughness and bearing properties and also the variation, within certain limits, of these characteristics at the site chosen for the first manned landing. These will determine the engineering requirements in the design of the landing gear. In the absence of reliable data, it is not known whether this problem will be simple or severe.

The heterogeneity and complexity of the lunar surface demands a strategy of exploration to insure effective and meaningful results. It is in large part this heterogeneity and complexity that excites scientific interest and that calls inevitably for man, with his versatility and adaptability, to visit the Moon.

None of the detailed information necessary for the selection of sites for manned landing or bases is now available. The necessary data on landing sites can only be acquired by a process of investigation analogous to that employed to determine regional and local geology on the Earth, and in order not to create superfluous words for the same process of investigation, it may be called geologic investigation. The requirements for scientific interpretation of the lunar surface go hand in hand with the technical requirements of the manned lunar mission and lunar basing.

Regional geologic mapping of the Moon, based on information that has been obtained telescopically, is already underway for the general target areas selected for the first rough-and soft-landing lunar spacecraft. Detailed information acquired at and near the landing points of these spacecraft will afford samples of the small-scale surface features and some of the physical, chemical, and mineralogical characteristics of the broad units recognized. There will be a serious gap, however, in our understanding of the distribution of surface material between the regional distribution of gross geologic units and the very local detailed variations in the vicinity of a small number of soft-landed spacecraft. This gap can be most readily filled by photography and other information of intermediate-scale resolution acquired from spacecraft placed in fairly low orbits about the Moon.

The unmanned phase of the national exploration program includes the launching of spacecraft that will carry capsules designed for rough landing, soft-landing spacecraft, lunar orbiters, and finally soft-landing spacecraft delivering roving vehicles and rockets for return of samples to the Earth (Hibbs, 1961). Two projects are now at a stage of launching or design and fabrication of spacecraft, Ranger and Surveyor. The Ranger

Oceanography, edited by MARY SEARS; 666 pages, $14.75; Publication No. 67 of the American Association for the Advancement of Science, 1961.

This is a collection of thirty papers which were presented as invited lectures at the International Oceanographic Congress in New York in 1959. Nominally the papers are grouped under five headings with from five to seven papers in each heading: I. History of the Oceans; II. Populations of the Sea; III. The Deep Sea; IV. Boundaries of the Sea; and V. Cycles of Organic and Inorganic Substances in the Ocean. In actual fact, this organization is somewhat artificial and is more indicative of the intent of the program planners than of the finished product. However, this listing helps to show the general scope of coverage.

The book largely consists of articles in which the authors' intent is to review—to cover the pertinent data and literature on their several topics, to evaluate and synthesize varying points of view, and to point out aspects of their subjects that particularly need further attention. However, a significant minority of the papers are more concerned with presentation of new material than with literature review. This gives a measure of zest and freshness to the book although it adds to the general impression, which is apparent throughout, of unevenness in both mode and quality of treatments.

In the opinion of the reviewer, about eighteen or twenty of the papers deserve a rating of good to excellent, but this statement must be qualified by adding that the scope of the book is too large for any one person to render an expert opinion on all of it. Among the best ten or twelve reviews of the classical type, there are—to mention a few—"Shape and Structure of Ocean Basins" by Maurice Ewing and Mark Landisman; "Paleobiogeography of the Marine Realm" by P. E. Cloud; "Certain Quantitative Characteristics of the Pelagic and Bottom Life of the Ocean" by L. A. Zenkevitch, and a paper by W. S. Broecker and three collaborators with a misleadingly general title but consisting in the main of an excellent account of the present status of C^{14} dating of ocean waters.

Excellent examples of articles that are partly or wholly original work are "An Estuarine Model of the Sub-Artic Pacific Ocean" by J. P. Tully and F. G. Barber, "Primary Production" by J. H. Steele, and "The Physical Chemistry of Sea Water" by L. G. Sillen.

At the opposite extreme are about five papers of minimal intellectual content, which the editor might well have considered omitting. Apparently she felt obliged to include all papers which were presented at the Congress, but in a few cases this is no kindness either to the reader or to the authors.

The volume is ably prefaced by Roger Revelle, and the editor has added excellent author and subject indices. Bibliographies appended to individual articles are as varied in content and degree of completeness as the articles themselves, but most of them will be valuable, particularly since the oceanographic literature is notoriously scattered.

This book, simply and almost cryptically titled "Oceanography," might have been called "Living Oceanography." For here we see it in the living state, as of September 1959, a big, sprawling infant of a science, drawing its sustenance from half a dozen basic disciplines and growing lustily albeit awkwardly. We see sophisticated advancements in some of its movements and ignorant fumbling in others. The outsider and the neophyte can look at this book and see what oceanographers are doing and trying to do. The oceanographer himself, often a specialist in a relatively narrow field, can get a better understanding of his brethren. The content ranges, as indicated above, from excellent to quite bad, but it is all interesting and provocative in one way or another.—*Gordon A. Riley*

The Encyclopedia of Microscopy, edited by GEORGE L. CLARK; 693 pages; $25.00; Reinhold Publishing Co., 1961.

Few indeed who seek information in this book will again return to its pages

project, which is the most advanced, includes the launching of two vehicles (Ranger 1 and 2) which were planned for long trajectories past the Moon, three vehicles (Ranger 3, 4, and 5) carrying rough-landing capsules, and a number of vehicles (Ranger follow on) designed mainly for photography, which will crash on the Moon. The first two spacecraft have already been launched, but were not injected into their programmed trajectories.

Ranger 3, 4, and 5 each will carry three principal observational instruments: (1) a vidicon television camera, (2) a gamma-ray spectrometer, and (3) a rugged seismometer, which is contained in the capsule designed for rough landing. Pictures planned to be obtained with the television cameras will have a ground resolution ranging from ½ km to 3 meters. They may thus afford the first data on topographic and geologic features of the lunar surface as much as two orders of magnitude smaller than the smallest features so far observed with telescopes. The gamma-ray spectrometer is designed to measure radioactivity associated with the decay of K^{40}, an isotope whose abundance may be taken as a rough index of the chemistry of rocks and meteorites. Measurements will be taken at intervals during the descent to the lunar surface so that the spectrometer will receive radiation first from a major part of a lunar hemisphere and then a progressively restricted part of the lunar surface. These observations may therefore provide evidence concerning the chemical heterogeneity of the lunar surface. The spectrometer data should provide the first test of the question of whether the rocks on the lunar surface may be chemically similar to the rocks of the Earth's crust. The seismometer, designed to operate for a period of 30 to 60 days, is planned chiefly to test the internal seismicity of the Moon, though it will also be sufficiently sensitive to detect some seismic disturbances likely to be generated by meteorite impact. Any internal seismicity detected will provide the first direct data on the internal processes of the Moon and on internal structure (Press and others, 1960).

Photography to be acquired in the Ranger follow-on program is planned to extend the high-resolution photographic sampling of the lunar surface and also to increase the maximum resolution. This part of the Ranger project is still in the development stage.

The Surveyor project includes the launching of seven spacecraft capable of soft landing on the Moon and an undetermined number of spacecraft to orbit the Moon. At present writing, only the soft-landing spacecraft are at an advanced stage of design. The first of these is scheduled to be launched in 1963 (Hibbs, 1961).

The Surveyor soft-landing spacecraft will be equipped with a number of vidicon television cameras, a drill to bring up samples from shallow depth beneath the lunar surface, and a variety of instruments to measure physical characteristics and determine the chemical and mineralogical

to broaden their knowledge or to check a matter of theory, practice or design in most realms of microscopy. "Encyclopedia" in name and alphabetic arrangement only, the book is poorly organized and edited, quixotically eclectic, of very uneven treatment, poorly cross-referenced and wholly unindexed. It would seem that the 75 contributors, some of whom are exemplary choices, were not party to a well-contrived plan for this volume, or provided by the editor with a common standard for content and level.

True, there are some fine articles of the quality and authority to be expected in a volume of this sort; among others, as examples, the articles by Hallen (Microtomy), Inoué (Polarizing Microscope, Design for Maximum Sensitivity), and Koester(Interference Microscopy, Theory and Techniques). But many papers, perhaps most, range from trite, often uselessly general statements, to frankly irrelevant matters. In fact nowhere in this remarkable book can an encyclopedic article be found on the simple principles of microscopy, or on phase or polarization microscopy, on means for testing and correcting performance of objectives, condensers and oculars, on stand, stage, and lens design, on alignment problems, on light sources, on micromanipulators, and so on for a very long list of the commonest important topics and needs of the microscopist's world. Indeed the section on "Fluorescence Microscopy," signed by the editor, is acknowledged as reprinted from a commercial brochure. But there are articles such as "Opium, Origin of," "Microscopists and Research Management" telling in part of American Cyanimid's organisational hierarchy, and, under the section on electron microscopy, "Specimen Preparation—Special Techniques at Los Alamos."

Reinhold Publishing Corporation would have it believed that "here, in one convenient volume, is the most extensive and important collection of information ever published on microscopy . . . of great value to everyone who uses a microscope . . ." Those who buy this $25.00 book, and most of those who make reference to it, will fervently wish that this were so. If Krause's grand *Enzyklopädie der Mikroskopischen Technik* of yesteryear be taken as a standard, then the volume under review may be seen at proper perspective.—*K. W. Cooper*

Biochemists' Handbook, edited by C. Long; 1192 pages; $25.00; Van Nostrand Co., Inc., 1961.

The 171 contributors to this volume have provided the biochemist and those engaged in the allied fields of biology and physiology with an extremely useful and comprehensive handbook of biochemical information.

The book is divided into six sections. The first on "Chemical Data" includes numerous tabulations of physicochemical data on buffers and salt solutions and information on such topics as radioactive isotopes, manometry, spectrophotometry, chromatography and various analytical procedures used in the investigation of polysaccharides. A brief discussion of enzyme kinetics in the second section is followed by an excellent review of the properties of about 300 enzymes. A concise and well-illustrated presentation of twenty-four metabolic pathways is given in the third section. An extensive, yet comprehensive, tabulation of data on the chemical composition of animal and plant tissues represents the fourth and fifth sections respectively. The final section consists of information on a variety of topics associated with nutrition and physiology.

References to the literature are collected at the end of each topic discussed. The Handbook contains a very complete and extensive index to some 8800 entries, a feature which makes information easy to find.

With the rapid progress of research in the many fields of biochemistry the Handbook will, no doubt, be subjected to several revisions and modifications in future years. Nevertheless this first edition provides the research worker, teacher and student with a reliable source of factual biochemical information as of 1959.—*Raymond F. Jones*

constituents of the materials both at the surface and in the drill hole. Several functions will be performed by the television system. Pictures will be taken during descent of the spacecraft to determine the position of the landing point. Some of the television cameras will be used to monitor the operation of the instruments on the ground. The principal function of the television cameras will be to survey the vicinity of the landing point. Pictures acquired will have a range of resolution providing information on the surroundings of the spacecraft approaching in detail that which a human observer of normal vision could see with the naked eye standing by the spacecraft.

No valid scientific criteria exist at the present to make a selection of the landing sites for the Ranger and Surveyor vehicles. Our ignorance about the Moon is so great, all regions of the lunar surface may be considered equally interesting from a scientific basis. The choice that any investigator might recommend on the basis of scientific interest would merely reflect his personal interest or bias. In terms of strategy, we are now at the stage of arbitrary or random sampling for information. Once a certain amount of data has been obtained in a given area, however, we will be specifically interested in comparing it with other areas, known from telescopic evidence to be different. In choosing the distribution of landing points for the limited number of spacecraft that will be launched, one objective will be to test the variation in the physical and chemical characteristics of a given geological unit and another will be to test the variation in physical and chemical characteristics between as many geological units that can be found as possible.

The general target area for Ranger 3, 4, and 5 has been determined by the requirement of bringing down the rough-landing capsule with as little lateral component of velocity as possible within the thrust and guidance capabilities of the Ranger system. The target area is defined by the impact points of minimum or near-minimum energy trajectories that meet this requirement and is a region of low latitude in the preceding half of the lunar hemisphere. Some choice in the landing points is available through selection of launch dates, within limits imposed by the system of orientation and communication with the spacecraft and the need for the lunar surface to be illuminated for photography. The feasible target area for the Surveyor soft-landing spacecraft is subject to similar constraints but is larger owing to greater thrust capabilities of the second stage rocket and flexibility in payload weight.

The most important restriction in the deployment of the Ranger and Surveyor spacecraft will stem from the need for information about the lunar surface for use in the program to land men on the Moon.

The necessary information on the roughness of the lunar surface may be provided directly by high resolution photography (10 cm ground resolution or finer). The Ranger follow-on program has been planned to

The Hubble Atlas of Galaxies by A. SANDAGE; 82 pages and plates; $10.00; Carnegie Institution of Washington, 1961.

Buy this magnificent volume at once. Priced at $10.00, it is a bargain you are not likely to find again in your lifetime.

In it, Allan Sandage has presented in superb format a wonderful collection of photographs of galaxies. They are as intriguing as ever, these galaxies, in spite of the enormous research done on them. Like the multifarious forms of the animate world, they have survived scientific analysis, and still embody the mystery we find in Nature Unknown.

An artistic achievement, this volume is also a scholarly work of major proportions. Everyone knows that Hubble discovered the expanding universe. His work on the individual galaxies is less well-known, but it is to him that we owe the classification of galaxies into spirals, ellipticals, and irregulars. Now, of course, spirals have been shown to be those galaxies where star-formation out of interstellar gas still proceeds, and the ellipticals those where the job is done; they are swept free of interstellar remains and seem to have no course open to them but Eddington's heat death.

One of the missing items in Hubble's scheme was a connecting link between spirals (which are flat as pancakes) and ellipticals (usually roundish). Hubble in his later years discovered many so-called "SO" galaxies which he believed to be the missing links. Sandage, working from years of experience as Hubble's collaborator, and from notes left by the great scientist, has made a very convincing case for this in this book.

Especially interesting among the various photographs are those of certain peculiar systems, such as the Sextons System (a square galaxy) and NGC 2685, (a cigar shaped one). Of course the old favorites—spirals like Andromeda and M51—are reproduced in breath-taking illustrations.

Anyone who enjoys pondering the mysteries of the heavens will be proud to own this volume. Even the experts who will refer often to Sandage's commentary will find relief from their humdrum data reductions in pouring over its images of creation.—*George Field*

Ore Microscopy by E. N. CAMERON; 293 pages; $10.50; John Wiley & Sons, 1961.

It was not until the middle of the second decade of this century that the necessary equipment became readily available so that geologists could study opaque ores in polished section to obtain better data regarding mineralogy and mineral genesis of ore deposits. The ore microscopy was used for the identification of opaque grains in polished section, and for the study of textures and structures which, with the mineral assemblage, permitted some insight into both the sequence and the conditions of deposition of the ores. Very early the ore microscope also became a friend of the extraction metallurgist.

Many textbooks, handbooks, and papers have appeared in the meantime, covering such aspects of ore microscopy as mineral textures and structures, mineral descriptions, relative and absolute hardnesses and reflectivities of minerals, and diagnostic etch and microchemical tests. The theory of reflected light, although discussed at an early stage by both Berek and Wright, was virtually omitted from subsequent textbooks because of its apparent lack of direct application to the study of polished surfaces.

For over ten years Cameron and his students have been applying certain techniques based on optical theory to the study of ore minerals. Using the polarization figure, they have been making observations on the dispersion of reflection rotation. With monochromatic light and some special accessories for their ore microscope, they have measured the angle of this rotation as well as the magnitude of the phase shift. These measurable properties are diagnostic to varying degrees for each mineral: it is yet another property that can be used as an aid in mineral identification. The description of these techniques and of the necessary optical equipment, as well as the presentation

acquire samples of such high resolution photographs to assess the problem. Radar data suggest that most of the lunar surface may prove to be relatively smooth at the scale of 10 cm. If this is so, the selection of a site and acquisition of the necessary information of the roughness at an appropriate manned landing site may be relatively easy.

Data on the bearing properties of the surface can only be obtained from impacting of rough- to soft-landed instruments, and instruments to measure the bearing strength at the surface are part of the Surveyor spacecraft scientific payload. Successful landing of these spacecraft alone would provide a significant amount of information. Even if all seven of the currently planned spacecraft are successfully landed on the Moon, however, they will provide determinations of the bearing strength and other characteristics of the surface at only seven points. If the bearing strength at all measured points on the lunar surface proves uniformly high, the problem of manned landing, again, will be simplified.

If, on the other hand, the lunar surface should appear hazardous for manned landing, either because the roughness or bearing strength lies beyond the range that can be safely accommodated by engineering design, we will be faced with a serious problem of strategy. The site for the first Apollo landing is likely to be chosen in the near vicinity of at least one and probably two successfully landed Surveyor spacecraft. Simple prudence dictates that detailed information on the terrain and some information on the variation in bearing strength of the surface at the landing site be obtained in advance. All of the successfully landed Surveyor spacecraft may be required to provide this information if the landing conditions are near the limit of safety or are highly variable. In the light of the requirements of the Apollo program, the first soft-landing Surveyor spacecraft may be available for launching before we know how to use them most effectively in the choice of Apollo landing sites.

Extensive high-resolution photography obtained from a lunar orbiter could provide the data needed to select potential Apollo landing sites and to design the most efficient use of the Surveyor soft-landing spacecraft. Not only will the photographic data permit the recognition of areas with suitably smooth terrain, but it will also permit analysis of the scale of heterogeneity of other physical characteristics. Complete coverage of an area with high-resolution photography will also permit a much more refined study of most aspects of the geology than can be made from present telescopic information. Indeed, photographs with resolution adequate to assess the roughness of the lunar terrain for manned landing would contain an immense amount of information at a scale sufficient for unequivocal identification of a great number of lunar features, if they are similar in the amount of surface detail to terrestrial features.

The present effort and rate of mapping the geology of the Moon from telescopic information is insufficient to provide an adequate base for use

of the underlying optical theory are the main subjects of Cameron's book.

However, he also describes other things. There is a chapter on the preparation of polished sections and polished thin sections. There is a thorough and critical presentation of the methods of measuring hardness and reflectivity, and of the methods of etch and microchemical testing. The pertinent data appear in seven tables in the Appendix, a convenient check-list for mineral identification. A short chapter concerns the various systems of mineral identification, and the final chapter deals with the application of ore microscopy, its purposes and limits.

Because Cameron has limited his subject to ore microscopy, the study of ore minerals in reflected light, his text book does not duplicate to any great extent the subject matter of any of the standard references, and conversely, it obviously fills a gap that has long existed. For this reason it will be a welcome and useful reference to all who work in this field.—*J. Kalliokoski*

Family Growth in Metropolitan America
by C. F. WESTOFF, *et al.;* 433 pages; $10.00; Princeton University Press, 1961.

This book is a product of one of the two most important recent surveys of American fertility conducted under non-governmental auspices. The other work, by Ronald Freedman, P. K. Whelpton, and A. A. Campbell is *Family Planning, Sterility, and Population Growth.* The major purpose of the book under review is to study the factors which might account for three important variables concerning family growth: number of children desired, fertility-planning success, and tempo of reproduction.

The survey is confined to a relatively narrow segment of white married women: those in large metropolitan areas who had recently borne their second child. Confining the study to women with two children made possible study of the correlates of the three family-growth variables without having to make allowances for differences dependent on number of children previously borne.

Some of the principal findings of the book are as follows: (1) There are major differences in the three family-growth variables by religion but only relatively minor differences by social class. Catholics desire the most children (a mean of 3.6) and Protestants and Jews the least (means of 3.0 and 2.8). Jewish women have the highest fertility-planning success and the lowest tempo of reproduction (measured inversely as mean interval between marriage and second birth); Catholic women, the least fertility-planning success and the highest tempo. White-collar wives desire only slightly more children (3.3) than blue-collar wives (3.2), but the difference is magnified for Catholic wives (3.8 *versus* 3.5). (2) Among Catholics, religious ardor has a substantial positive correlation with the three family-growth measures. (3) The sex distribution of existing children is a determinant of desired number of children; couples having two children of the same sex desire the most children. (4) Measures of social mobility, social relations within the family (with the exception of duration of wife's employment), and of personality characteristics have low correlations with family-growth variables. The low correlations of measures relating to social relations within the family and to personality characteristics may be due to inherent difficulty in obtaining valid measures rather than to intrinsic lack of association.

This book is an important contribution to our knowledge of fertility behavior.—*David Heer*

The Origins of Scientific Thought, Vol. I
by G. DE SANTILLANA; 320 pages; $5.95; University of Chicago Press, 1961.

Greek science will always be an inexhaustible source of fascination for every student of Greek antiquity. The amazing multitude of its facets offers a rich variety of approaches and presentations, ranging from a straight description of scientific achievements in the strictly technical sense to a history of

in planning the lunar exploration program. Less than a dozen scientists in the United States are working on the preparation of such maps or are obtaining basic data that is useful in analysis of the geology. A far greater effort will be required to analyze extensive information from a lunar orbiter. There is a pressing need to survey the entire field of possibilities for choice of potential Apollo landing sites and to attempt to look beyond Apollo to the long-range development of the lunar exploration program. Without the long-range consideration the tendency will be for the first landing sites of unmanned spacecraft to become the sites for manned landing and for the manned landing sites to become, in turn, the sites of future lunar bases whether best suited or not. Information and material tend to pyramid at these localities. If at all possible, it is desirable that these localities also be advantageous for the long-range exploration of the Moon and other phases of space activity.

Choice of landing sites for Surveyor spacecraft, therefore, should be delayed as long as possible in order to take advantage of as much information as becomes available and can be utilized prior to launch. The choice of target for each successive Surveyor launching should depend on the cumulative results of all prior successful landings. An orbiting spacecraft should, above all, be launched as early as possible in the Surveyor series.

Purely scientific objectives will play a secondary role in the opening stages of exploration of the lunar surface. As a consequence of the acceleration of the Apollo program, most or all of the spacecraft now scheduled for the first phases of unmanned exploration may be required to provide specific information needed for planning manned landing. As full an understanding of the geology of the lunar surface as can be obtained is also needed for this purpose, but it is the "engineering" aspects of this information that is the most urgently needed. The scientific knowledge gained will be a byproduct, in a sense, of the effort to select and determine the characteristics of the first Apollo landing site. Unfortunately, the most suitable sites for manned landing—those that are smooth and uniform in physical characteristics—are the least suited for the determination of geologic relationships. The best areas for investigation of stratigraphy and structure are those with considerable relief where a variety of material is exposed. The walls and throw-out from craters, for example, will provide the most accessible data on the distribution of materials at depth but present the most hazardous landing conditions.

A further reason why the most important scientific targets will be reached only in later stages of lunar exploration is that they cannot be identified at the outset. Key localities for the solution of outstanding scientific problems will probably be reached by men after the surface has been extensively investigated with the aid of lunar orbiters and remotely

scientific ideas seen as an integral part of Greek philosophy or civilization. Professor de Santillana pictures the growth of Greek science essentially against the background of Greek culture, as a significant chapter of the history of civilization, and he has carried out his task in a very eloquent and—on most of the issues—convincing way, well illustrated by many quotations.

Particularly welcome is the emphasis put on the early period, comprising both the mythological origins of Greek science and the Presocratic era. In this difficult part of the story (taking up more than half of the book) where one has to rely on scarce and fragmentary documentation, Santillana has succeeded most in giving an integrated and balanced account. He not only deals extensively with issues having a direct bearing on later scientific developments, such as the Pythagorean foundations of mathematics and cosmology and the dynamical concepts of Empedocles, but he gratifies the reader by presenting him with detailed accounts of the Eleatics as well as the Sophists. Many a philosopher may disagree with Santillana's interpretation of Parmenides, but there can be no doubt as to the place he deserves in a history of science, as so many threads are leading from the Pythagoreans to him and from him to Plato and his conception of mathematics. The chapter on the Sophists lucidly brings to our mind that much discussed topic of today—the interaction of science and society. It all began in Athens in the 5th century B.C. through the medium of those men whom Gomperyz once so aptly characterized as "half professors, half journalists."

After two rather condensed but well written chapters on Plato and Aristotle, the last part of the book does not come up to the level of the first as far as the selection of topics and their mutual balance is concerned. The story of astronomy, and partly that of mathematics, deserves a more methodical account. In the last chapter, Santillana gives a very instructive comparative survey of Atomism, Stoicism and Neo-Platonism. Again, one would have liked to have a less aphoristic treatment of the Neo-Platonists who had such great insight into the function of mathematics as the language of science. Santillana also discusses the main reasons for the final frustration of Greek science—a subject which will keep the minds of historians of science busy for some time. With forceful arguments he stresses the influence of political and economic factors, and perhaps underrates the powerful psychological forces which prevented the Greeks from developing systematic experimentation and, in its wake, a technology which might have led Greek science out of its impasse. Although one can always argue about details, Santillana's book is an imaginative and stimulating contribution to a subject of central interest to philosophers, scientists and classical scholars alike.—*S. Sambursky*

Handbook of Automation, Computation, and Control, Vol. III: Systems and Components, edited by E. M. GRABBE, *et al.*; 1158 pages; $19.75; John Wiley & Sons, Inc., 1961.

"Philosophically minded technologists do not accept automation merely as a third Industrial Revolution. They see it . . . as a manifestation of one of the greatest Intellectual Revolutions in Thinking . . . They see that it has brought great changes to our conventional way of thinking about the human use of human beings . . . and about the organization of workers in order not to hold captive bodies of workmen for jobs that automation, computation, and control have swept or will soon sweep away." Thus the foreword indicates the inspiration for this work.

Volume 1, *Control Fundamentals*, consists largely of the mathematics of feedback systems. Volume 2, *Computers and Data Processing*, is a comprehensive treatment of digital and analog computers. Volume 3 (the last) integrates information from the earlier two into descriptions of complete systems and also contains details about many of their components.

controlled roving vehicles.

At present, it cannot be said definitely whether, or at what stage, men on the Moon will be useful in studying and mapping its surface. The usefulness of men will depend entirely on the mobility of the men and on the scale of heterogeneity of the surface. If the surface of the Moon is as varied as most parts of the Earth over the range of scales that a man can observe standing on the ground, there is no question that a man would be useful in determining the relationship between the different observable components of the lunar surface. At least some parts of the Moon, for example, areas near fresh meteorite impact craters, may be expected to have this type of diversity. On the other hand, some interpretations of the reflected radar data and considerations of the cumulative effects of bombardment of the surface by small meteoroids and cosmic dust suggest the possibility that at just the scale most suited for observation by man, the lunar surface may be peculiarly monotonous. Pictures taken at close range, especially from television cameras mounted on soft-landing spacecraft, will have resolved this question before men are sent.

In any event, men are going to be sent to the Moon, and the pertinent question is how to make optimum utilization of their capabilities. Men may be used in preparing a rocket for a return trip, in base construction, in adjusting scientific instruments, or in on-site scientific observations. As far as scientific objectives are concerned, man's abilities should be compared with those of remotely controlled instruments. He should be used for those phases of research for which his sensing capabilities and versatility provide distinct advantages over unmanned instruments. In principle, there is nothing a man can do that could not be performed by a remotely controlled probe. In practice, however, there are certain kinds of scientific operations for which a man, even with all the life-supporting paraphernalia he needs and the added burden of returning him to Earth, may prove to be the cheapest, lightest, and most efficient system.

Men on the Moon may be able to provide more flexible response in some cases to unexpected observations than remotely controlled probes; they could also make preliminary on-site observations and decisions in those studies where the particular observations that will be most fruitful cannot be anticipated in advance. In general, however, the use of men on the Moon for operation of instruments to make high precision analyses or measurements would be very inefficient. Such instruments can, in most cases, be designed to operate by remote control, or samples can be more effectively studied by returning them to Earth. Man's superiority as an on-the-spot observer, is in his ability to combine acuteness of vision with manipulation of all the observed material. The simple process of acute seeing, of adjusting field of vision rapidly, while moving

It begins by discussing the sometimes maligned term, "systems engineering." In describing this analytical approach to complete systems, the authors must include obvious facts about one common component: the human being, such as noting that he is nonexpendable. And although information about such complex matters as the human transfer function is very incomplete, this volume should give the reader some appreciation of the systems approach. A large part of the book discusses mechanical and chemical process control, emphasizing practical examples of systems. The last third is devoted to components, particularly amplifiers, actuators, and semiconductor devices. The treatment of most transducers is limited essentially to tabulations of their properties, but continuous end point analyzers are given an entire chapter. This is probably appropriate since they are being rapidly developed compared to other transducers, which may be considered as only indirect means of determining the success of a system's operation.

Although sometimes the theory seems obscured by the practical examples, and there is redundancy because of the large number of contributors, they have produced a very interesting and valuable set of books.—*Peter L. Balise*

Shock Tubes by J. K. WRIGHT; 164 pages; $2.95; John Wiley & Sons, 1961.

In nine short chapters, the author of this monograph describes the principles which control the design and the operation of shock tubes and of some of the experimental techniques which have been developed to study the waves thus generated. In the introduction, he summarizes the standard equations for one-dimensional compressible gas flow. This is followed by a discussion of shock tube theory and brief but informative descriptions of both optical and nonoptical techniques for measuring the propagation and characteristics of normal shocks. The longest chapter is devoted to reflection, refraction, and diffraction of shock waves. Shock front structure, the production of strong shock waves, and their investigation are then covered in sequence. This booklet can serve as a handy reference for the basic equations which are used by workers in a shock tube laboratory. The author includes useful references to most of the significant papers.

Dr. Wright was not fully successful in attaining some of his objectives. His account is far from balanced; it suffers particularly from a limited analysis of what characterizes the behavior of a real gas. He does not discuss such exciting applications as shock tunnels, wave superheaters, or the possibilities of single-pulse operation of shock tubes. The exciting aspects of chemical kinetic studies at high temperatures are practically unmentioned, nor is any space devoted to the current utilization of shock tubes for the study of the spectroscopic properties of gases at very high temperatures. This treatment is certainly not the first book on shock tubes as is stated in the flyleaf. Indeed, it suffers considerably by comparison with a small book on chemical reactions in shock waves by Greene and Toennies, published in 1959. Finally, since there are few references in this book as late as 1959, the author inserted four pages of notes and 25 references covering the period 1958–60. These are helpful but are far from adequate, for they do not give the reader a feeling for the growth and extensive development of shock tube technology during the past four years.—*S. H. Bauer*

Argon, Helium and the Rare Gases (The Elements of the Helium Group); Vol. I: History, Occurrence, and Properties, edited by G. A. COOK; 427 pages; $17.50; Interscience Publishers, 1961.

This book is the first part of a two-volume reference work concerned with the rare gas group of chemical elements and contains material from nine contributing authors.

Apart from an occasional specialized review article, there has been no attempt in recent years to survey the properties of the rare gases. In view of

a specimen, while digging a shallow hole, or while witnessing a natural process in action is extremely difficult to duplicate by telemetry. A man is extremely effective in analyzing complex interrelations of materials and can facilitate selection of samples or measurements to be made or design a specific attack on a problem.

Detailed geologic mapping is simply a formal way of recording some of these observations. It is a process of integrating a wide variety of observations into a conceptual scheme that may be portrayed in two or more dimensions. It should not be supposed that geologic mapping would involve the preparation of a base map on the Moon. This would be an extravagant use of men and time. For effective utilization of men on the Moon, they should arrive with very detailed maps already prepared, portraying the topography around the landing site, the photographic tone, and a preliminary analysis of the geology based on all information available prior to departure. To this base they would add new observations and make revisions or additions to the interpretation of the geologic relations.

An example of the kind of information that may be recorded in map form by on-site observations in a brief period of time is afforded by a reconnaissance geologic map of the New Quebec Crater in the Canadian Arctic (Fig. 7) prepared from observations acquired in a period of three days. The purpose of this particular work was to evaluate whether a more intensive geologic investigation of this crater should be undertaken. The first men to reach the Moon are likely to be much less mobile than the writer was at the New Quebec Crater, and the period of their visit may be shorter. The area they can effectively study will be correspondingly smaller.

The map shows the distribution of five geological units, each characterized by a specific set of lithologic or physical features. Information on the topography is also available for this crater and the three-dimensional relationships of the units are known but are not portrayed in this illustration. During mapping, qualitative information was obtained by visual examination on the structure, on the lithologic composition, and on some of the physical characteristics of each map unit. Several dozen samples, accurately located, were collected for more intensive investigation in the laboratory.

Preliminary examination was made of low altitude aerial photographs before going to the crater, and features observable in the photographs (Fig. 8) were compared with reports of previous visitors to the crater. At the crater, mapping was accomplished chiefly by studying in detail selected parts of the crater rim, and tying the various selected parts together by a continuous traverse around the rim. Widely scattered observations on the ground were also made by helicopter, and the different observations made on the ground were extended by flying back

the increasingly important role that they now play in science and technology, this is surprising and one reason why the volume will find a place on many bookshelves.

This does not claim to be a textbook; even so, a more detailed outline of the relevant theory would greatly improve some of the sections. At present one gets the impression that treatment of the subject matter is somewhat arbitrary. In some instances important work has been dismissed briefly or left out altogether. For example, in the chapter on liquid and solid state properties, no mention is made of the heat capacity measurements by Jones and Walker or the helium II experiments carried out by Pellam. The treatment would also be improved if liquid and solid state properties were discussed in separate chapters.

Some sections are already out-of-date—for example, those dealing with density and optical properties. This is not the fault of the authors, but rather the fate of any reference volume in this age of rapid progress. The book serves a useful purpose. If it were revised and expanded so as to be slightly more comprehensive, it would become even more useful.—*Brian L. Smith*

Francois Matthes and the Marks of Time (Yosemite and the High Sierra), edited by F. FRYXELL; 192 pages; $7.50; Sierra Club, 1050 Mills Tower, San Francisco 4, California, 1962.

About a hundred years ago in the formative period of our knowledge of geology there was the happy combination of leading scientists gifted with the ability to set forth the results of their work in such form as to appeal to a reader without technical knowledge, and at the same time to the cultivated Englishman, widely read, interested and knowledgeable in the development of science.

Fryxell gives delightful account of Matthes' early life which so well fitted him for his professional career. Matthes was born in Amsterdam in 1874, of a family of means and was sent to Switzerland for schooling, because of the widespread malaria then in Holland. There he developed a love for mountains and natural history, and showed great talent in drawing which was to be of much use later. He started engineering training in Germany but left Frankfurt, to come to MIT where he specialized in geodesy. As a young man he joined the U.S. Geological Survey where he showed extraordinary ability as a topographer and later developed his interest in the study of land forms, applied particularly in the Yosemite, where he worked over a forty-year period.

The present book is a series of essays mostly published originally in the bulletin of the Sierra Club. They all deal with the Sierras and various features, small and large, which bear on the development of one of the most magnificent and beautiful regions of the world. They are written with a clarity and simplicity of style that bring the reader to a comprehension of the many events, particularly those related to glaciation, that have resulted in the High Sierras. To those who have had the joy of intimate acquaintance with the region they "flash upon the inward eye." To those not acquainted they stand as superb expositions of the working of the scientific mind starting with simple features and ending with those of great complexity but all made thoroughly comprehendible.

The illustrations are superb and Fryxell and the Sierra Club are to be congratulated in making the essays available to the public.—*Edward Sampson*

A Glossary of Geographical Terms (Prepared by a Committee of the British Association for the Advancement of Science), edited by L. DUDLEY STAMP; 539 pages; $10.00; John Wiley & Sons, 1961.

American geographers and geomorphologists will find this book very useful as an addition to the "Glossary of Geology and Related Sciences" of the American Geological Institute. It supplies many terms of geographic import not included in the American book. The two books differ in that the Amer-

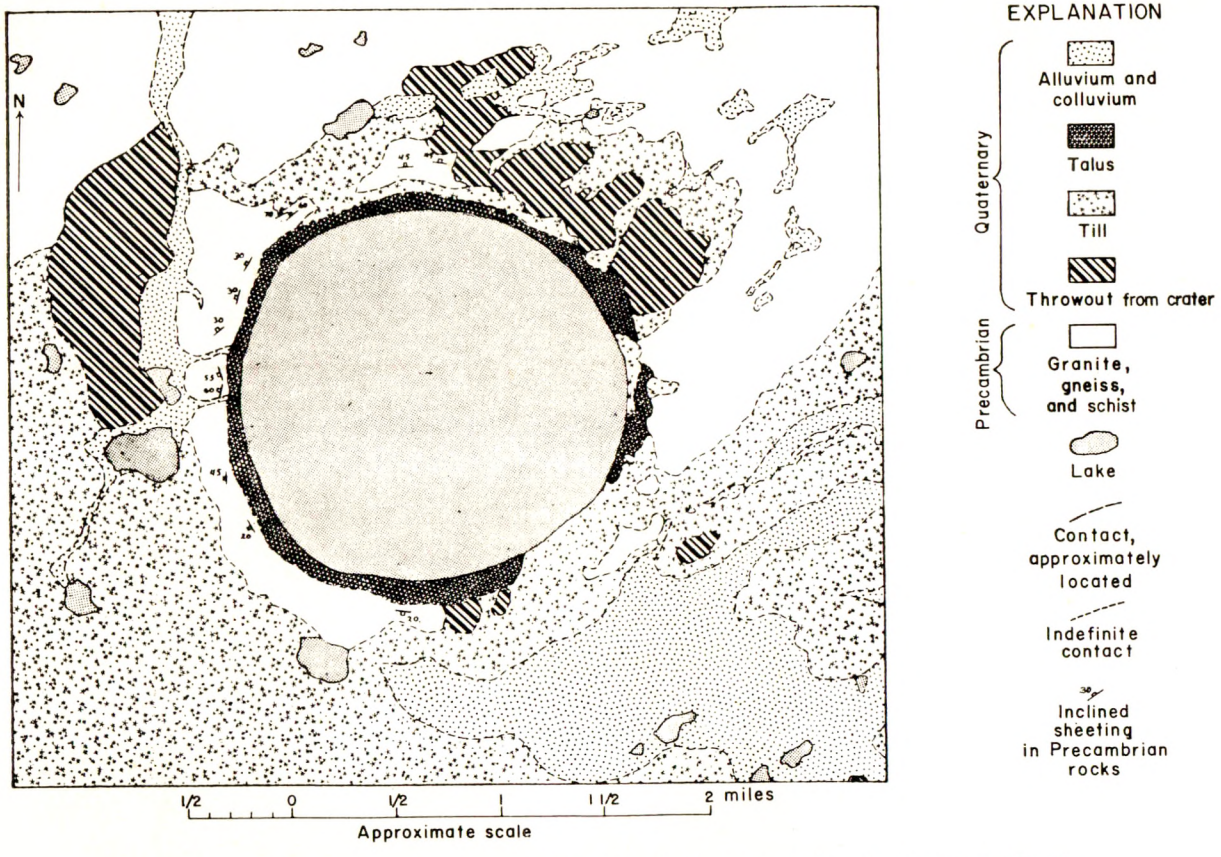


FIG. 7. Reconnaissance geological map of the New Quebec crater, Canada. Geology by E. M. Shoemaker, 1961.

ican Steering Committee enlisted subcommittees of men most active, and presumably knowledgeable in the various branches of the subject, and entrusted to their judgments the most acceptable current usage and definition; with a minimum of historical background or only a single original reference. On the other hand, the British Committee—"Rather than attempt an agreed definition—which all members of the Committee would accept—we have quoted the original definitions and usage of standard authors, though venturing in some cases to add comments" (p. xi). This system has required more space; hence fewer terms than the American book. Ordinary dictionary words such as thermometer, barometer, etc., are not included nor are terms proper to surveying, mathematical geography, astronomical geography, map projections, geological periods, beds, rocks, and minerals. Included are terms shared by geographers and geologists and terms from meteorology and climatology likely to be found in regional geographic descriptions (such as local names of winds) similarly from soil science and ecology such terms as are likely to be found in geographical works are included. The book is a complement to the American Glossary. Appendix I is a list of Greek and Latin roots used in constructing terms, and Appendix II is a list of words in foreign languages which have been absorbed into English literature.—*Paul MacClintock*

The Biology of Hydra and of Some Other Coelenterates, edited by H. M. Lenhoff and W. F. Loomis; 467 pages; $4.50; University of Miami Press, 1961.

There is a considerable number of biologists presently doing some exceedingly interesting work on hydra and other coelenterates and this volume is the record of a symposium on this subject. Like all symposia volumes it has its good and bad points. The good is the excellence of the contributions themselves which cover such varied subjects as fine structure, nematocyst toxins, feeding mechanisms, tissue culture, senescence, and of course a large section on growth and development. From this many interesting facts emerge; for instance, no nerve fiber has ever been seen in an electron micrograph of hydra, yet cruder techniques with the light microscope show them without difficulty.

The bad comes from the fact that, quite understandably, not all the workers on hydra in the world could be present and therefore the picture it paints is not a comprehensive one. This is a pity because the subject is of such great importance that it would be possible and much more valuable to give a really integrated picture of the whole field. Let us hope that this will be done soon by one of our twentieth century Trembley's.

Another criticism is the inevitable dialogue after each paper. One has come to expect this Socratic touch in symposia and even enjoy the occasional flashes of irritation and temper. But it does seem to me that if one is going to bother to set type for a question and answer period, one should bother to look up a few things and give some valuable information. On page 267 there is the following bit of repartee:

"Fulton: I think some German workers have succeeded in reinfecting white *Chlorohydra*, but not other species of hydra. Incidentally, we found that the antibiotic chloramphenical cures green hydra of the algae in a couple of days, much faster than glycerine.

"Muscatine: What concentration was used?

"Fulton: I'm not certain, but I think it was 200 μg. per ml."

Despite the rush of publishing the book, surely someone could have looked up those German workers and that concentration.—*John Tyler Bonner*

Chemical Thermodynamics by J. G. Kirkwood and I. Oppenheim; 261 pages; $8.75; McGraw-Hill Book Co., 1961.

The book is designed as a logical sequential development of thermodynamics, starting from operational

and forth at low level in the helicopter. The information thus obtained consists essentially of linear and point samples of the geology at varying scales; the map is produced utilizing additional information contained in the aerial photographs. All data and sample points were plotted on the photographs while in the field by identifying the points on the ground in the photographs with the aid of stereoscopic viewing.

It may be seen that the geologic map represents a synthesis of observation and interpretation—chiefly an interpretation of how the materials are distributed—but by the law of superposition, a dimension of time is also established. Genetic names have been attached to most of the map units, but need not have been. The units could just as easily be designated by arbitrary symbols, each symbol representing a detailed, purely factual description. The genetic interpretations are made both by the correlation of the observed characteristics of the units with the

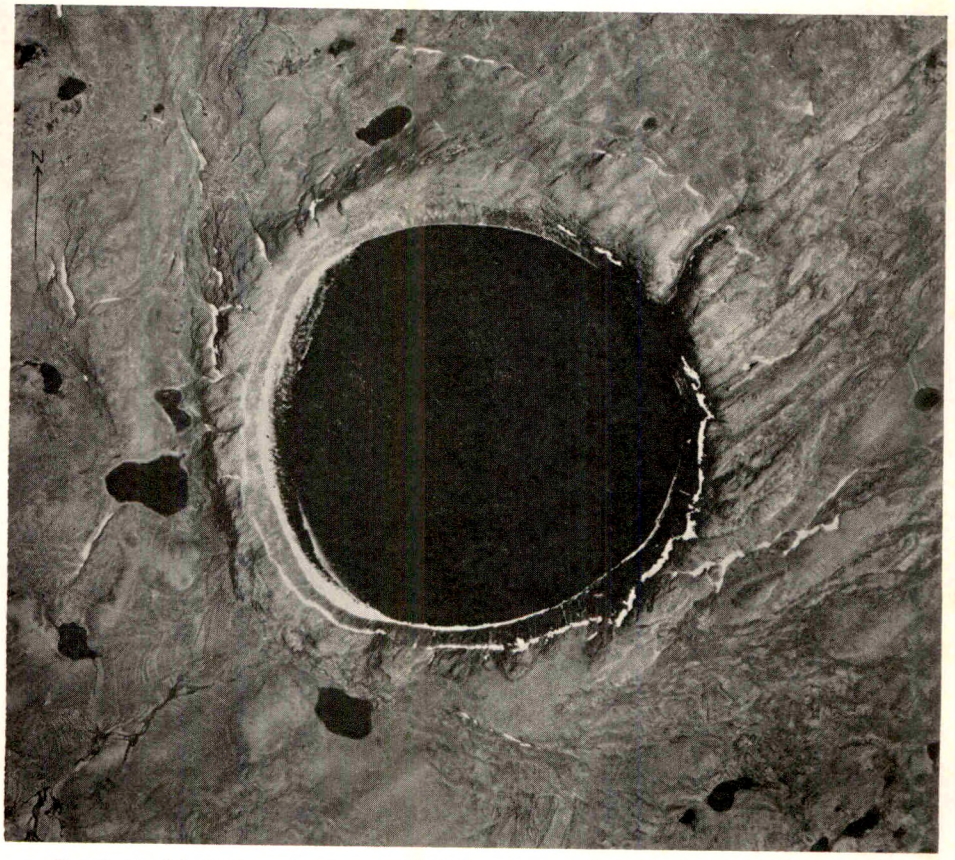


FIG. 8. Aerial photograph of the New Quebec crater. Photograph by Royal Canadian Air Force.

definitions of fundamental variables and the three laws. Mathematical rigor is uniformly maintained, with no recourse to heuristic arguments. Teachers of thermodynamics will find in this book not only a useful compact text for seniors and first year graduate students; they will also find a model for clear, concise and logical presentation of a technical subject. Kirkwood's many brilliant and fundamental contributions in original research are indispensable parts of chemical physics today; this presentation of thermodynamics should become equally significant in the field of scientific pedagogy.

The plan of the book is as systematic as Euclid; concepts and definitions are always introduced as they are needed, and in proper logical sequence. The pattern is best described by quoting the opening sentence of Chapter 1. "Thermodynamics is a discipline that involves a formalization of a large number of intuitive concepts derived from common experience. As such, it employs a number of terms which are used loosely in everyday language. Before beginning the presentation of thermodynamics as an exact science, it is essential to define the terms with some exactitude." This point of view is the Leitmotiv of the treatise; both semantic and mathematical rigor are preserved without, however, any sacrifice in readability of style or clarity of presentation. One other detail of design calls for favorable comment: each chapter begins with a brief review of the material to be presented, so that the student can start with a mental outline of what is to come.

The book contains a surprising amount of material, thanks to its concise style. For example, only eleven pages are used to present the second law; nevertheless, in this brief space, two equivalent physical expressions of the second law (one based on Kelvin's principle and one on a generalization of Clausius' postulate) are considered, and then it is shown that these are equivalent to the mathematical statement of the second law by deriving Carathéodory's principle from them. The third law is covered in four pages, half of which describe cases where values of absolute entropy from thermal data and the third law disagree with the values calculated from enthalpy and free energy changes, and furthermore accounts for these typical non-conformities. As one might expect from Kirkwood's long interest in solutions, the chapters on solutions of neutral substances and of electrolytes are excellent, with the possible exception of the treatment of weak electrolytes where the "progress variable" is formally associated with an activity coefficient. The treatment here is admittedly self-consistent, but could lead to confusion because the thermodynamic activity coefficient in equation (12-56) has been approximated by unity. The last two chapters of the book, on the dependence of thermodynamic properties on electrical, magnetic, gravitational and centrifugal fields are welcome additions to the conventional catalog of subjects usually treated in texts on thermodynamics. In these, as in many other parts of the book, original, rather than traditional, derivations are presented.

In order to achieve compactness, equations which do not belong to the direct development of the thermodynamic theme (for example, the Debye specific heat equation, the Debye-Hückel activity equation, the Onsager conductance equation, the Einstein turbidity equation) are occasionally inserted without derivation when they are needed in the exposition. Frequent references are also made to classical statistical mechanics and to quantum statistics, which will not be understood by a beginner in thermodynamics. Presumably, therefore the book is intended as a text for students who have already had a sound course in physical chemistry and are now ready for a rigorous and critical development of thermodynamics as a logical entity. To these and to their teachers, this reviewer strongly recommends the book. It will also be most useful to the research worker as a source of precisely formulated equations in which all symbols are unambiguously defined in terms of observable operations.—*Raymond M. Fuoss*

characteristics of geologic units elsewhere which have been exhaustively studied, and by geologic principles known to be valid on the Earth. Genetic interpretation of the units of the first map of a crater or other part of the Moon, prepared by on-site investigation, will present a somewhat different problem. The mapping does not depend on speculations about the origin of the crater or of any of the map units, although insights into the origin of the map units generally facilitate their recognition and discrimination.

Acquisition and synthesis of observations to produce a geologic map is a complex problem of strategy in itself. The problem comes in two parts. The first part is to recognize or determine the units that can be mapped and the second part is to discover the mutual relations and trace out the boundaries of the units. One begins on this problem by necessity with some arbitrary or random observations. Such observations can be made by walking around on the ground or by study of photographs or of any other information available. Decisions on units to be mapped depend upon the ability of the observer to discriminate between different materials, his ability to recognize how the materials are related to one another, and on such practical considerations as the time available to study a given area. In tracing out the relations between units, the observer must be guided by his understanding of what he has observed to find the points or kinds of observations which provide the most critical evidence. The efficiency or completeness with which relevant geologic information is extracted from an area in a given amount of time, therefore, depends heavily on the skill of the observer and the familiarity he has at the start with the features of the area and the type of geology.

Predictions for the Near Future

By way of summary, the following predictions are offered on the future course of the American program of lunar exploration.

The most important data to be acquired from the Ranger project will be the highest resolution pictures obtained, though these pictures will not necessarily yield the results of greatest scientific significance. I believe these high-resolution photographs (at least one-half meter ground resolution or better) will be obtained from several different localities on the lunar surface. The landing site for the first soft-landing Surveyor spacecraft almost certainly will be at one of the localities of this high-resolution photography and will not be selected therefore until such photography is available.

In the meantime a major effort probably will go into design and construction of several spacecraft to orbit the Moon. In addition to a capacity for high resolution of the ground from a height on the order of 100 km above the lunar surface, one or another or all of these spacecraft might carry radar, infrared detectors, and a gamma-ray spec-

The Conservation of Wild Life and Natural Habitats in Central and East Africa (UNESCO) by J. HUXLEY; 113 pages; paper-bound; no price given; Columbia University Press (International Documents Service), 1961.

This small-octavo, paper-backed publication with a good set of half-tone illustrations is deceptive: it is not an ephemeral trifle but a closely packed account of a long African tour by one of the sharpest pairs of eyes in the academic world. The eyes are backed by a mind with an extraordinary capacity for soaking up the relevant scientific literature. Deceptive quality rests in the ease of reading, for meaty as it is there is not a trace of turgidness or feeling of the meal lying heavy. Those who have read the author's *In an Antique Land* will know what a fortunate choice UNESCO made in asking Sir Julian to undertake this long and arduous journey, particularly as he made a comparable one over much the same ground in 1930. The last sanctuaries of wild life in Africa, the richest mammalian fauna on earth, are threatened by meat hunger, pragmatism, apathy and some notion of pushing back animal diseases.

Huxley states the African situation in ecological terms: fundamentally a problem of balancing resources against human needs, both in the short and long view. There must be a proper evaluation of human needs, based on resource conservation and use, optimum land use and conservation of the habitat. The terrain of Africa south of the Sahara has suffered great damage at the hands of man so that few ecological communities below 6000 feet have remained intact and in general they have suffered degradation.

In the course of this journey, example after example was encountered of almost complete neglect of the wild life in so-called surveys and land-use projects, often on penny-wise grounds. Even in the so-called Ngorongoro and Crater Highlands Conservation Unit, a sum of well over half a million dollars is about to be spent without an adequate master plan. Huxley fits the National Parks essentially into the picture of land use much farther afield and explains that Africa cannot afford to squander its deeply stratified wild-life resource. The final chapter of conclusions and recommendations is an important document which must surely go far beyond UNESCO.—*F. Fraser Darling*

Researches on Meteorites, edited by CARLETON B. MOORE; 227 pages; $7.00; John Wiley and Sons, 1962.

This book contains a series of short papers presented or read by title in a symposium held at the University of Arizona in March 1961.

E. L. Krinov briefly makes a case for the science of meteorites as a domain of its own. He is an eminent authority in the U.S.S.R. so that his opinions and historical summary are of particular interest.

Papers by Signer and Nier; Goel; Arnold, Honda and Lal; and Goel and Kohman, have in one way or another to do with the effects of cosmic radiation on meteorites in space. The first of these discusses concentration of rare gases in iron meteorites particularly He, Ar and Ne isotopes; the second, rates of production of specific nuclides in irons and the third, after examination of the evidence, concludes that cosmic radiation intensity has been approximately as observed today for the past several hundred million years. The last dealing with C^{14} suggests there could be short term fluctuations in cosmic radiation on the 1000-year scale which might iron out in a multi-million year scale. C^{14} is useful in determining the number of years since the meteorite arrived on Earth.

Massolski considers metallurgical aspects of meteoritic iron. He suggests that coarse Widmanstatten patterns could have been produced in 10^7 years during cooling. Marringer and Manning discuss deformation and thermal alteration in meteorites from a metallurgical point of view and deplore that metallurgists have not contributed more to the science of meteoritics.

Mason gives a list of minerals found in meteorites, 34 in all. He mentions

trometer. The first spacecraft should be designed to obtain complete stereoscopic coverage of the Moon with a resolution at least one order of magnitude greater than can be currently achieved by visual telescopic observation under optimum conditions of seeing. It could also be designed to obtain much higher resolution pictures of small areas at intervals along the orbital path of the spacecraft. Later orbiters may be planned to increase the higher resolution coverage and to expand the spectral region covered, particularly into the ultraviolet, possibly to the X-ray region. Analysis of the spectral components and polarization of light reflected from the lunar surface may be additional objectives.

Data acquired from the lunar orbiters will provide the foundation for great advance in the science of the lunar surface. From the characteristics of their orbits, a detailed determination of the figure of the Moon will be made. The new data on the figure will provide the necessary control needed for preparation of high quality topographic maps by photogrammetric methods. Topographic mapping of the Moon will be started at a scale of 1:100,000 or larger. An intensive effort will be begun to work out the stratigraphy and structure of the lunar surface at this scale. The photometric and topographic characteristics of the stratigraphic units will be correlated with the polarimetric data, spectral information extending from radiowave lengths to ultraviolet, and with the distribution of radioactivity. The stratigraphy will be defined on the basis of properties of the materials that can be determined from a distance, but it will be possible to infer much about the physical properties and the chemical and mineralogical composition of the surface materials from this data. In short, the study of the lunar surface will blossom as a systematic science.

We may note in passing that all of this work is not going to be accomplished by a dozen people. The number of scientists and technicians required more likely will be of the order of magnitude of at least 1000, and the demand for qualified scientists to go into this essentially new field in the next few years will be correspondingly great. While some of the methods of approach in this new science are fairly well developed, many of the techniques for reduction and interpretation of the information are yet to be invented.

The most intensive effort, at first, without question will be devoted to search for and investigation of a site or alternate sites for the first manned landing. Once these have been selected they will become the targets for most of the Surveyor soft-landing spacecraft. My guess is that most parts of the lunar surface will be both smooth and firm enough to pose no great difficulty for manned landing. Two or possibly three spacecraft will probably be used to investigate each of the alternate sites chosen; one or two spacecraft may be used in "prospecting" for scientific information in areas not chosen or unsuited for manned landing.

that tridymite is not stable at any temperature at pressures in excess of 3000 km/cm^2 which places a rather specific restriction on the size of the parent body or depth within it at which tridymite-bearing achondrites could have formed. Moore presents some interesting data on the bulk petrochemistry of achondrites but suggests that study of the chemistry of individual phases, their proportions and petrography is badly needed for progress in this field.

Lovering makes a rather convincing case for the coexistence of chondrites, achondrites and irons all within a single parent body rather than that the first and last two represent two sorts of bodies which exist separately in space. Ringwood reviews the status of the chondritic Earth model He believes that chondrites do give a good approximation of the major chemical elements in the Earth but from a study of element variation among chondrites themselves suggests that elements volatile at 1600°C under reducing conditions have escaped from chondrites and presumably the Earth in varying proportions. Potassium is one of the volatile elements and K^{40} if present in the Earth in chondritic proportions would be the main source of radioactive heat within the Earth. Its partial elimination would permit an Earth model which is cooling with time.

Finally, Nininger presents a well reasoned plea to put more effort into the finding of meteorites and demonstrates from his own long experience that this would be effective.

A comprehensive review of the state of the science of meteoritics is urgently needed at the present time. This book should be welcomed as going a short distance in the right direction. It is of course not comprehensive but a collection of papers on very recent research results. Many of the leaders in the field are included among the authors but the absence of such men as Anders, Urey, Kuiper and a number of others who are in the forefront of research in this area should be noted.

The book is beautifully bound, and has very nice paper, but the format (typed, single spaced) is abominable, difficult to read and full of typographical errors. The reader should equip himself with a magnifying glass to read many of the tables and illustrations.—*H. H. Hess*

A Sterotaxic Atlas of the Cat Brain by RAY S. SNIDER and WILLIAM T. NIEMER; $12.50; The University of Chicago Press, 1962.

A Stereotaxic Atlas of the Monkey Brain by RAY S. SNIDER and JOHN C. LEE; $12.50; The University of Chicago Press, 1962.

The stereotaxic technique is today, perhaps, the most widely used manipulative device for research on internal aspects of the central nervous system. Each of these atlases represents the brain by a double series of photomicrographs (a nuclear and a fiber stain) for half millimeter intervals extending from anterior to the diencephalon posteriorly to include most of the brain stem.

The large plates are well-labeled, but, as a practical guide in the laboratory, would be much improved if a grid had been superimposed on them instead of indicating the coordinates via marginal scales along two sides. The nuclear plates would be less confusing if some dotted lines had been employed to help define nuclear areas.

Nevertheless, these atlases which represent the most complete coverage ever assembled of the cat and monkey brains in stereotaxic coordinates will prove most useful.—*Robert D. Lisk*

Theory of Elasticity by V. V. NOVOZHILOV; 448 pages; $12.50; Pergamon Press Inc., 1961.

This book is the translation of the original Russian publication which appeared in 1958. The author develops linear and non-linear elasticity theory in a detailed and general fashion in the first five chapters (approximately five-eighths of the book). The remaining portion of the book is devoted to detailed and comprehensive treatments of two applications of elasticity in classical linear theory—the St. Venant's problem of torsion and flexure of cylinders

The National Aeronautics and Space Administration has publicly indicated that scientific investigation of the Moon is the specific objective of the Apollo program (Webb, 1961). It will be imperative, therefore, to include at least one highly qualified scientist as a member of the Apollo team. This person will probably be found among the new breed of scientists who become involved in investigation of the lunar surface, for it will be essential that he be intimately familiar with the available data and the problems of its interpretation. He should also be highly competent and experienced in geologic studies (Urey, 1960).

The period of time spent on the Moon in the first landing probably will be very short, perhaps less than a day. Duration of the landing may depend somewhat on the area that can be covered by the lunar scientist and the heterogeneity of the landing site. The time will be spent principally in geologic investigation or, more specifically, in field checking and adding to detailed maps already prepared from Ranger and Surveyor data, and in the collection of samples. Special attention will be given to geological information to be used in planning later landings and the construction of a lunar base.

Looking beyond the first Apollo landings, I foresee a stage of lunar exploration when most of the data obtained directly at the surface will be acquired from remotely controlled roving vehicles. Men will at first be confined to small areas around the landing sites. Unless water proves to be a fairly abundant constituent of most material at the lunar surface, a specific objective of later stages of exploration will be to locate rocks rich in hydrous minerals or permafrost.

Water will be the most important resource that can be found on the Moon. It may be important not only for use in manned bases but also in space travel to more distant regions. If adequate sources of water can be found, the Moon will ultimately become an important refueling stop, either for hydrogen-oxygen chemical rockets or for nuclear rockets using hydrogen as the working fluid.

Anyone wishing to look still further into the future is welcome to borrow my crystal ball.

ACKNOWLEDGMENTS

This paper has drawn on research done on behalf of the National Aeronautics and Space Administration. Investigations at the New Quebec Crater were carried out under the auspices of the Dominion Observatory of Canada. I am indebted to Audouin Dollfus for preparing photographs for me from the Observatoire de Meudon.

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of constant cross section and the plane problem. The book can be recommended as collateral material to graduate students in engineering and to professional engineers desiring familiarity with the modern theory of elasticity.

Two adverse comments must be made. The book lacks a much needed index. The text is produced by non-letterpress setting and photo-lithography which creates difficult reading in a few crucial passages where broken characters appear.

The publisher can supply English translations of any Russian article mentioned in the bibliography or referred to in the book—*Floyd Dunn*

The Psychology of Aggression by ARNOLD H. BUSS; 307 pages; $7.95; John Wiley & Sons, 1961.

This book presents a good selective coverage of the literature on aggression, anger, and hostility. The term aggression is defined as "a response that delivers noxious stimuli to another organism." Aggressiveness is seen as a "personality variable, a class of response that is both enduring and pervasive. In this behavioral approach to aggressiveness, a crucial role is played by *habit*: aggressiveness is the habit of attacking. . . . aggressiveness is treated as an entity in order to simplify the exposition but it is really a habit system with component systems." The author's selection of topics has been strongly influenced by his behavioral approach but he does present the views of the personality theorists and psychoanalytic schools. He does a good job in summarizing the major articles and books in the area, adding his evaluation of some major hypotheses such as the conclusion that the frustration-aggression hypotheses should be discarded. A new formulation of catharsis is presented, using instrumental aggression as different from angry aggression. In the chapter on Inventories as a Method of Investigation, he is harsh on the work of others, but overly lenient with the Buss-Durkee inventories. It is not clear from the presentation that any criteria were used in designing the instrument. The chapter on Investigating Aggression in the Laboratory is good, including an interesting and novel "aggression machine" which warrants further study. After reading the book, one is left with the strong impression that really very little is known in this area. In spite of this, the book should be of interest to both lay and professional readers who want to know more about the subject. There is an adequate author and subject index.—*Gary Carlson*

Computer Control Systems Technology, edited by CORNELIUS T. LEONDES; 649 pages; $16.00; McGraw-Hill Book Co., 1961.

As might be expected from its twenty-one contributors, this book contains a wealth of information and variation in its treatment. It shows the thinking of leaders in control systems development, and the numerous descriptions of applications illustrate the diverse but interrelated problems in complex automatic control of processes ranging from nuclear reactions to space travel.

The explosive growth of technology makes it difficult to decide how much basic material to include in advanced texts. This book has abbreviated treatments of analog computer theory, digital computer programming, nonlinear system analysis, and Boolean algebra. The problem of getting adequate explanations into minimum space is compounded by the large number of contributors; so, for example, the mechanical integrator is described twice, but analog computer scaling is incompletely presented. Phase-space synthesis is very well explained, while the describing function is briefly mentioned. It might be better to give a few good introductory references and not require the familiar reader to scan basic theory in getting to the significant content. Surprisingly, this book does not mention many good texts on subjects such as analog and digital computation.

Its strength lies in topics not generally found in other books. Included are space vehicle control system synthesis (with body flexibility and fuel sloshing), computer control of industrial processes, error analysis techniques for computers,

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and optimalizing control (particularly for discrete time systems). There are also elementary or less unique presentations of sampled-data control, random processes, machine-tool control, air traffic control, and basic control and computer techniques.

Because no major subject is covered very completely, this book would rarely serve as a text, but its unique contributions in some fields and its good descriptions of applications will generally interest control engineers.—*Peter L. Balise*

Functional Circuits and Oscillators by HERBERT J. REICH; 466 pages; $12.50; D. Van Nostrand Co., 1961.

The type of nonsinusoidal waveform generating and pulse circuits treated in this book find a wide application in instrumentation, control and computing systems. The author presents an extensive catalog of the electronic circuits used in these fields, together with a general development of the basic principles of operation of certain classes of such circuits, particularly those based on the use of negative resistance elements. Vacuum-tube and transistor circuits for performing similar functional operations are treated concurrently.

A general knowledge of elementary electronic circuits is assumed, including the equivalent circuits and methods of analysis of transistor and vacuum-tube amplifiers. The first few sections of the text are concerned with summing and mixing circuits, linear and non-linear waveform shaping circuits, electronic switching and elementary logic circuits. Numerous illustrations of specific circuits are given, usually in the form of circuit diagrams together with a brief qualitative description of their operating characteristics. A more general analysis of negative resistance circuits is then developed, followed by a set of specific examples of their application to form multistable, bistable, and monostable circuits and to form relaxation oscillators. The final third of the book is devoted to the presentation of various types of sine-wave oscillators, together with a general, and usually qualitative discussion of their characteristics.

The lack of a more quantitative analysis of many of the circuits illustrated is partially alleviated by the inclusion of a set of problems. These indicate the method of analysis to be used for various types of circuits and provide some of the derivations of operating characteristics which are omitted in the text.

The types of circuits being used in electronic systems are evolving very rapidly, due both to the growth of this field and to the introduction of new types of devices. This should be a useful reference book to illustrate the types of circuits available at this time for performing certain of the functional operations required in data processing systems.—*William H. Surber*

The Action of Insulin on Cells by M. E. KRAHL; 202 pages; $7.50; Academic Press Inc., 1961.

This is a well integrated monograph on the complex effects of insulin on the various tissues. Emphasis is placed on the study of isolated tissues; those acted upon by insulin are listed and such tissues as muscle, liver and adipose tissue receive detailed attention. Other chapters review the relation of insulin and cell permeability, the interactions of insulin and pituitary adrenal factors, insulin effects in cell-free systems, notes on the structure of insulin, and finally, speculations on insulin action. The frequent tabular summaries, as well as the clear text, put together many experiments in such a way as to funnel them to concise conclusions. The conclusions in turn consist of a clear selection of the principal facts known about the effects of insulin and brief speculation on its action. To the worker in the field, this should be an invaluable technical reference. To physicians who want to know what their colleagues in the laboratory have done, it will provide the most concise summary possible. It would be impossible to write on this subject without omitting a few details. It would be useless to write on this subject without some philosophical framework. The advantage here is that Dr. Krahl's slant and perspective add greatly to a subject which others might shade differently.

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The reviewer regards Dr. Krahl's work as a masterpiece and expects it to speak for itself.—*F. D. W. Lukens*

Astronomical Spectroscopy by A. D. THACKERAY; 256 pages; $3.95; The Macmillan Co., 1961.

Since the advent of Sputnik, there has been a noticeable upswing in the public's interest in astronomy, and to satisfy this interest, a growing number of popular books in astronomy have appeared. The present book, which is one of a series entitled *A Survey of Astronomy*, is aimed at a somewhat more sophisticated group, namely, serious amateurs and astronomy students. It appears, however, that the tailoring of the level of presentation has been accomplished by the reduction of background material rather than by an increase in the depth of penetration of the subject material. However this may be, Mr. Thackeray has admirably succeeded in catching the excitement of the research going on in the various fields of astronomy that he discusses.

The title of the book suggests the author's aim, which is to discuss the many applications of spectroscopy to astronomical research. For example, the identification of the origin of spectral lines and the measurement of their strengths can give information on chemical composition, temperature, and density for various astronomical objects such as stellar atmospheres, planetary atmospheres, gaseous nebulae, planetary nebulae and interstellar gas. The Döppler shifts of the spectral lines provide a powerful tool for studying the line-of-sight velocities of those objects producing the lines. The discovery of the expansion of the universe resulted from a study of the Döppler shifts in the spectral lines of distant galaxies.

This book can be read with profit by its intended audience who will receive an interesting, though cursory, survey into a number of current astronomical research fields. The book is clearly illustrated and well produced.—*John B. Rogerson, Jr.*

Photogeology by V. C. MILLER, assisted by C. F. MILLER; 248 pages; $13.50; McGraw-Hill Book Co., 1961.

Geologic interpretation of aerial photographs involves techniques widely used today but about which there is unfortunately a dearth of book literature. This book is a timely addition to this literature. It is divided into three parts the first of which is designed to acquaint the reader with types of aerial photographs, stereoscopy, and general methods for making a geologic study from aerial photographs. Part Two discusses principles of photointerpretation, with particular emphasis given to criteria used for identification and interpretation. The third part of the book is a collection of stereoscopic illustrations together with accompanying explanations of problems of identification and interpretation. Liberal use of line drawings, block diagrams, and mosaics complement the nearly 80 stereoscopic pairs of aerial photographs and will contribute much to the usefulness of the book. Exercises designed for student use are given with selected stereoscopic pairs of photographs.

The author's organization and presentation of subject material is perhaps unique, and this is reflected in his division of illustrations into four geomorphic categories (elementary, supplemental, complemental, and independent geomorphology) and three geologic categories (routine, moderately difficult, and difficult photogeology). This scheme of presentation, related perhaps to the commercial orientation that is noticeable in numerous places throughout the book, is bound to generate useful thought on the subject of geologic photointerpretation.

In general, Miller has handled a large body of technical data nicely, and the reviewer feels that the author's somewhat different treatment of subject material together with his many fine stereoscopic illustrations will make *Photogeology* a valuable publication.—*Richard G. Ray*

KINETIC SPECTROSCOPY AND FLASH PHOTOLYSIS

By R. G. W. NORRISH

THIS ARTICLE* gives an account of a new and powerful method by which the intimate nature of photochemical and thermal reactions can be studied and which yields objective information about the free radicals and other transient intermediates taking part. These intermediates generally have very short lives, measured in millionths of a second; nevertheless it has been possible to photograph their characteristic spectra at successive intervals of time and so to measure the rates of their growth and decay during chemical reaction. Sometimes they are produced in a highly energised state of thermal vibration, yet "cold" as regards their translational and rotational motions, and the study of this has thrown light on the propagation of chain reactions, and the transfer of energy from vibrational to other thermal degrees of freedom.

The method may be called "kinetic spectroscopy": it involves the use of the photochemical technique of flash photolysis, discovered and developed in the Laboratory of Physical Chemistry at Cambridge University, England.

The light which stimulates chemical reactions extends from the red end of the visible spectrum at 7000 Å to the extreme ultraviolet. In practice, with a few exceptions, direct observation of reaction kinetics is limited by the transparency of quartz to about 1900 Å, though photochemical effects can be inferred from spectroscopic observations in the vacuum ultraviolet as far as 500 Å.

Fundamental Photochemical Principles

The absorption or emission of light by atoms and molecules takes place in terms of "quanta," and to the photochemist the photon takes the

INDEX TO BOOKS REVIEWED. *Acting upon the suggestion of Professor Donald S. Miller of the Department of Geology at Rensselaer Polytechnic Institute, the Editors are pleased to try out an index for quick reference of the books reviewed in this issue. Reader comment will be welcome.*

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Andrewartha, H. G.	Introduction to the Study of Animal Populations	196A
Bates, D. R., ed.	Quantum Theory: Vol. I, Elements	200A
Berkner, L. V. and H. Odishaw	Science in Space	190A
Bourne, C. H. and J. F. Danielli, eds.	International Review of Cytology, Vol. XI	186A
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Goodspeed, T. H.	Plant Hunters in the Andes	195A
Grabbe, E. M., *et al.*, eds.	Handbk. of Automation Computation and Control, Vol. III	214A
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If every molecule in a gram molecule absorbed a photon amount of energy absorbed would be

$$E = N\epsilon = \frac{Nhc}{\lambda}\ (\text{ergs}) = \frac{6.06 \times 10^{23}.\ 6.6 \times 10^{-27}.\ 3 \times 10^{10}}{\lambda\ (\text{in cm.})\ J}\ (\text{cal.})$$

For yellow light	E = *ca.* 45,000 cal.
For blue light	E = *ca.* 65,000 cal.
For ultraviolet light (3000 Å)	E = *ca.* 95,000 cal.

Chemically speaking these are very large amounts of energy.

When a photon of the spectral range with which we are concerned here is absorbed by a molecule, only a small proportion gives rise in the first instance to heat in the form of vibrational and rotational displacements of the molecule, and in the case of monatomic substances none of the energy appears initially as heat, although in both cases it may be degraded to heat by secondary processes.

The photon, absorbed by the atom or molecule, brings about a change in the electronic configuration which effectively produces a new atom or molecule of higher energy, changed reactivity and new characteristics. Such an atom or molecule is said to be excited. There are in general more than one energy level, and as many excited states and an atom or molecule may thus absorb more than one wavelength to give characteristic absorption spectra. The study of photochemistry is concerned with the physical and chemical consequences resulting from the absorption of a photon by an atom or molecule.

The photochemist distinguishes three stages in a photochemical process:

1. The process of light absorption: "one molecule, one photon"—leading to the excited state.

2. The primary reaction: the immediate chemical change suffered by the excited molecule, e.g., dissociation:

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KINETIC SPECTROSCOPY AND FLASH PHOTOLYSIS

By R. G. W. NORRISH

THIS ARTICLE* gives an account of a new and powerful method by which the intimate nature of photochemical and thermal reactions can be studied and which yields objective information about the free radicals and other transient intermediates taking part. These intermediates generally have very short lives, measured in millionths of a second; nevertheless it has been possible to photograph their characteristic spectra at successive intervals of time and so to measure the rates of their growth and decay during chemical reaction. Sometimes they are produced in a highly energised state of thermal vibration, yet "cold" as regards their translational and rotational motions, and the study of this has thrown light on the propagation of chain reactions, and the transfer of energy from vibrational to other thermal degrees of freedom.

The method may be called "kinetic spectroscopy": it involves the use of the photochemical technique of flash photolysis, discovered and developed in the Laboratory of Physical Chemistry at Cambridge University, England.

The light which stimulates chemical reactions extends from the red end of the visible spectrum at 7000 Å to the extreme ultraviolet. In practice, with a few exceptions, direct observation of reaction kinetics is limited by the transparency of quartz to about 1900 Å, though photochemical effects can be inferred from spectroscopic observations in the vacuum ultraviolet as far as 500 Å.

Fundamental Photochemical Principles

The absorption or emission of light by atoms and molecules takes place in terms of "quanta," and to the photochemist the photon takes the same importance as the atom did to the chemist one hundred and fifty years ago. Photochemistry is the study of the interaction of photons with atoms and molecules.

The magnitude ϵ of the photon or quantum of light is related to its colour, i.e., its wavelength,† and is given by

$$\epsilon = \frac{hc}{\lambda} = h\nu$$

It therefore increases as we pass from red to blue into the ultraviolet.

* This article is slightly abridged from the Presidential Address to the Chemistry Section of the British Association at Norwich, England, August-September 1961. The original article appeared in *Advancement of Science*, Nov. 1961 and is reprinted here with the permission of Mr. J. M. Robertson, Deputy Secretary of the British Association.

† h = Planck's constant; c = velocity of light; λ = wavelength; ν = frequency; N = Avogadro's number; J = Joule's equivalent.

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If every molecule in a gram molecule absorbed a photon the tota. amount of energy absorbed would be

$$E = N\epsilon = \frac{Nhc}{\lambda} \text{ (ergs)} = \frac{6.06 \times 10^{23}.\ 6.6 \times 10^{-27}.\ 3 \times 10^{10}}{\lambda \text{ (in cm.)}\ J} \text{ (cal.)}$$

For yellow light	E = *ca.* 45,000 cal.
For blue light	E = *ca.* 65,000 cal.
For ultraviolet light (3000 Å)	E = *ca.* 95,000 cal.

Chemically speaking these are very large amounts of energy.

When a photon of the spectral range with which we are concerned here is absorbed by a molecule, only a small proportion gives rise in the first instance to heat in the form of vibrational and rotational displacements of the molecule, and in the case of monatomic substances none of the energy appears initially as heat, although in both cases it may be degraded to heat by secondary processes.

The photon, absorbed by the atom or molecule, brings about a change in the electronic configuration which effectively produces a new atom or molecule of higher energy, changed reactivity and new characteristics. Such an atom or molecule is said to be excited. There are in general more than one energy level, and as many excited states and an atom or molecule may thus absorb more than one wavelength to give characteristic absorption spectra. The study of photochemistry is concerned with the physical and chemical consequences resulting from the absorption of a photon by an atom or molecule.

The photochemist distinguishes three stages in a photochemical process:

1. The process of light absorption: "one molecule, one photon"—leading to the excited state.
2. The primary reaction: the immediate chemical change suffered by the excited molecule, e.g., dissociation:

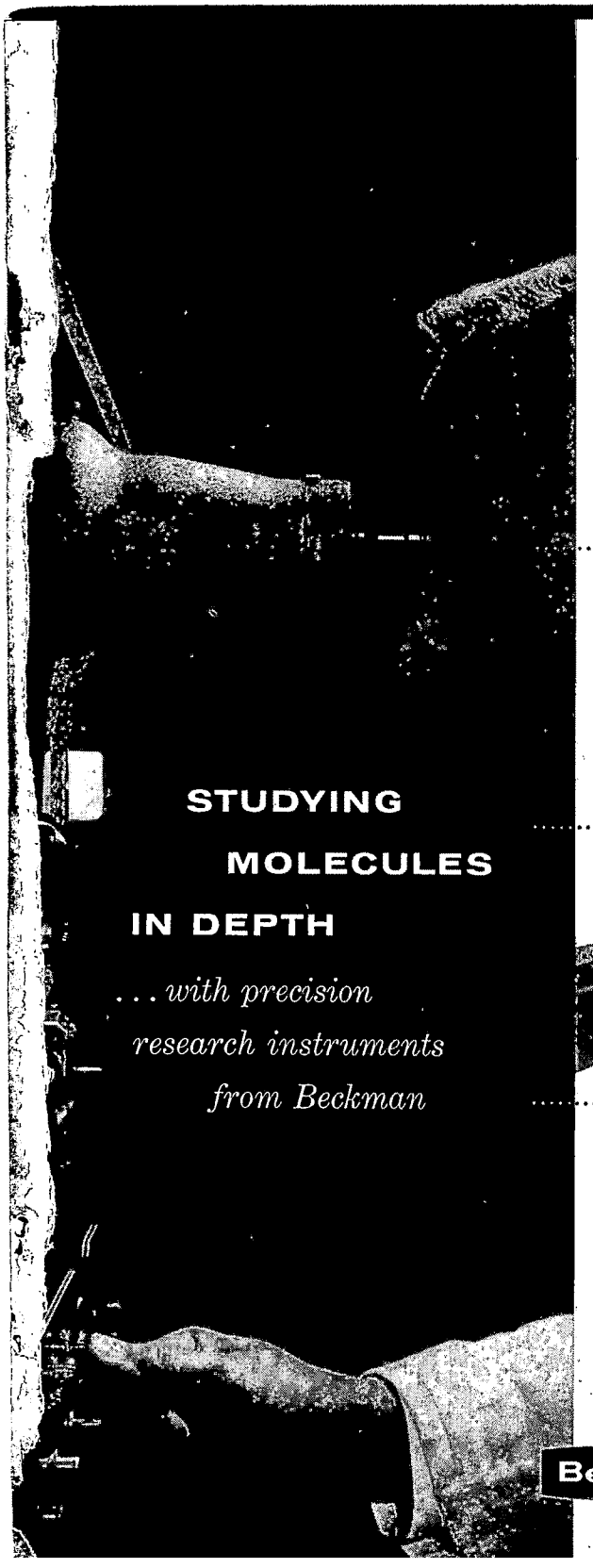
$$NO_2 + h\nu \rightleftarrows NO_2^* \rightarrow NO + O$$

3. The secondary reactions—the subsequent "dark" reactions of the products of the primary process:

$$O + NO_2 = NO + O_2$$

Often the energy absorbed in process (1) does not give rise to reaction; the process may be reversible, in which case we get fluorescent emission closely related in structure to the absorption spectrum. Alternatively, the energy of the excited molecule may be degraded to heat by collision and lost.

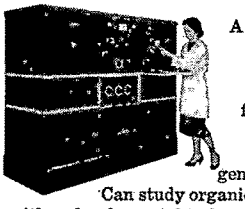
The maximum life period of an excited molecule or atom is $10^{-8} - 10^{-7}$ sec: whatever is going to happen must take place in that interval, otherwise the reverse process occurs and the energy is rejected as fluorescence. Some molecules when excited are so unstable that they



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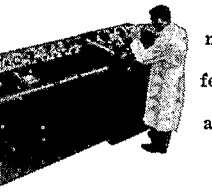
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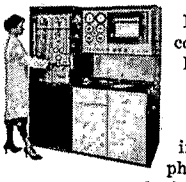
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break down in the course of $10^{-13} - 10^{-10}$ sec to give atoms or free radicals or simpler molecules by processes known as optical dissociation and predissociation, as for example:

$$Cl_2 \rightarrow Cl + Cl$$
$$S_2 \rightarrow S + S$$
$$CH_3COCH_3 \rightarrow \cdot CH_3 + \cdot COCH_3$$
$$CH_3COCH_2CH_2CH_3 \rightarrow CH_3COCH_3 + C_2H_4$$

Such processes, especially in simple molecules, may often be recognised spectroscopically by a diffuseness which overtakes the vibrational or rotational structure of the absorption spectrum in the region corresponding to the photochemically active wavelengths.

It may happen also that, instead of reacting or losing its energy as fluorescence, an excited atom or molecule may collide with an atom or molecule of another species with transference of the bulk of the energy from the one to the other by a radiationless process, causing the second to become excited and to react. Such a process is known as photosensitisation; it is well represented by photosensitive action of mercury vapour in dissociating hydrogen, hydrocarbons, etc.

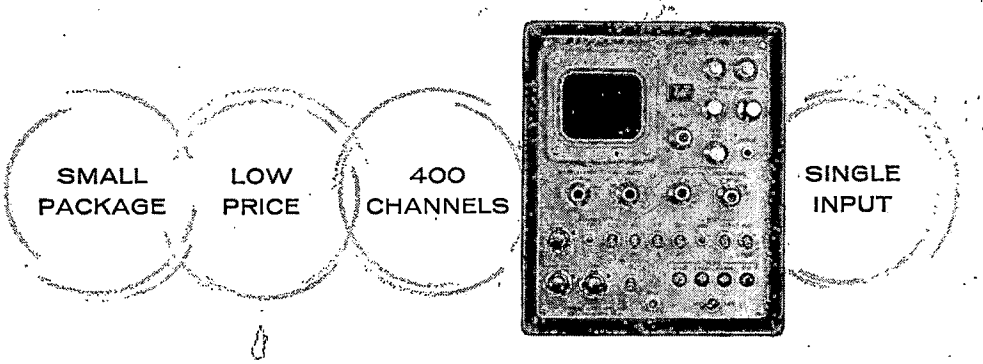
$$Hg + h\nu(2536\ A) \rightarrow Hg^*$$
$$Hg^* + H_2 = Hg + H + H$$

In other cases the photosensitiser may itself participate in the reaction as with the photosensitised decomposition of ozone by chlorine; in such cases the "sensitiser" is regenerated intact.

$Cl_2 + h\nu = Cl + Cl$	Primary reaction
$Cl + O_3 = ClO + O_2$	Secondary (chain) reactions
$ClO + O_3 = Cl + O_2 + O_2$	
$ClO + ClO = Cl_2 + O_2$	Sensitiser regenerated

Of the secondary reactions we may distinguish two types—those corresponding to over-all endothermic processes in which the energy of reaction is provided directly by the photon, and those whose over-all processes are exothermic, in which the light acts, as it were, catalytically, initiating a series of self-propagating reactions able to multiply to explosion, just as a minute spark can set off a great conflagration. The former of these two types is exemplified by the photolysis of nitrogen peroxide, already cited, in which the maximum yield cannot rise above two molecules of NO_2 reacted per quantum of light absorbed. The latter type comprises all those chain reactions initiated by light, such as the chlorination of hydrocarbons, or the photosynthesis of hydrogen chloride, where the quantum yield may be as high as 10^6 molecules reacting per quantum absorbed, e.g.

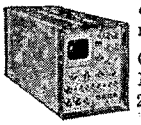
$Cl_2 + h\nu = Cl + Cl$	Primary reaction
$Cl + H_2 = HCl + H$	Secondary (chain)
$H + Cl_2 = HCl + Cl$	reactions repeated n times
$Cl + Cl = Cl_2$	Chain termination
$nH_2 + nCl_2 + h\nu = 2nHCl$	Over-all reaction



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The principles of photochemical reaction which I have just summarised were first conceived some fifty years ago through the work of Warburg, Bodenstein, Henri and Weigert and took final shape through the work of others who followed. Into the whole framework have been fitted innumerable photochemical and thermal reactions whose postulated mechanisms, involving atoms, free radicals or excited species, are entirely derived from the circumstantial evidence afforded by spectroscopic observations and quantitative measurements involving photochemical kinetics, and end products. In general, the consistency of the mechanisms within the framework is so good as to lead to confidence in our conclusions, but the absence of any objective proof of the existence of

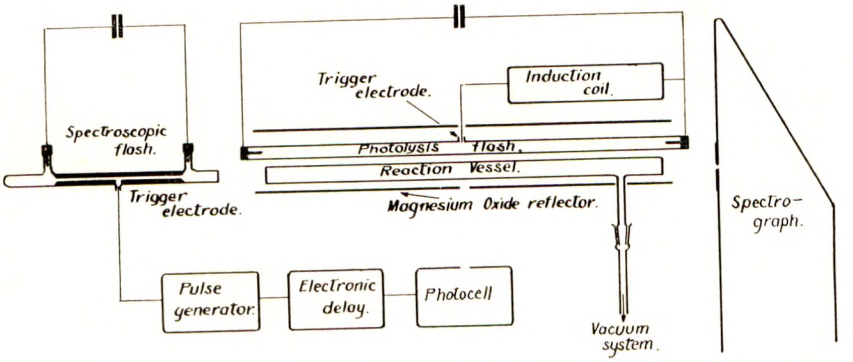


Fig. 1.

the postulated intermediates has constituted a permanent challenge to the photochemist, and has sometimes given rise to criticism from unbelievers which weakened his position.

Flash Photolysis

It was for this reason that in 1946 we decided to break new ground by endeavouring to obtain direct spectroscopic evidence for the free radicals postulated as intermediates in photochemical reactions. This was made possible by the discovery by Porter and myself [1] that very large measures of photolysis (sometimes complete) could be achieved by the application of a powerful light flash to suitable reactants, such as nitrogen peroxide, iodine, or acetone, contained in a quartz vessel. These early flashes produced by discharging a bank of condensers through argon were of 1–2 milliseconds duration and dissipated up to 10,000 Joules of energy, and it seemed obvious that if free radicals were produced in the reactions, they must momentarily have been at such very high concentrations as to invite efforts to photograph their absorption spectra. This was achieved in the first instance by Porter [2]

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in 1949, who, using a second flash as a photographic source triggered mechanically at short intervals after the first by the method of Oldenburg, was able to demonstrate the dissociation of chlorine and to obtain the spectrum of the CS radical by the photolysis of carbon disulphide. This early success was followed by the development of the technique: shorter photolysis flashes of 100 microseconds duration and of 1000–3000 Joules were devised, and shorter intervals between the photolysis flash and spectroscopic flash were achieved by using electronic delay circuits capable of precision timing to a few microseconds. That which follows is a description, with reference to Figure 1, of the improved apparatus described by Norrish, Porter and Thrush [3], which is basically typical of the method used today.

The Apparatus. By the discharge of a bank of condensers through an inert gas such as Krypton at about 10 cm pressure, powerful flashes dissipating some 1000 to 3000 Joules of energy may be produced. The silica flash tube is 50 cm in length and 1 cm internal diameter, and the discharge takes place between thick tungsten electrodes. The energy dissipated is given by

$$E = \tfrac{1}{2}CV^2$$

where C is the capacity and V the potential between the electrodes. Ideally, for a given energy, the shortest flash is obtained by making the capacity as low and the voltage as high as possible, while at the same time reducing to a minimum the self-inductance of the system. For a tube 50 cm in length we generally use a capacity of 40 μF and a voltage of 10 kv giving a flash energy of 2000 Joules and about 50 μsec duration.

The flash is applied to the contents of a quartz reaction vessel 50 cm long and 2 cm diameter with plane end plates, lying parallel and close to the flash tube; the whole is surrounded by an aluminium tube coated on the inside with magnesium oxide to act as a reflector. The second lamp giving the spectroscopic flash is arranged to throw a beam through the length of the reaction vessel by means of which the absorption spectrum of the contents of the tube is photographed by a Littrow spectrograph (Hilger 478). The spectroscopic flash of 100 Joules and 10 μsec duration is obtained by discharging a 2μF condenser at 10 kv through a capillary tube about 10 cm long containing Krypton or other inert gas at about 10 cm pressure. It gives a fairly continuous spectrum which allows an absorption spectrum to be photographed with one flash.

It is fortunate that neither lamp is discharged until a pilot or trigger current from an induction coil is passed, and this makes it possible by means of a photocell and a suitable calibrated electronic delay circuit to cause the flash from the first lamp (the photoflash) to discharge the second (the specflash) at suitable controlled intervals varying from a few microseconds to seconds. Thus it is possible by taking a series of

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separate spectrum photographs at increasing intervals of time to observe the growth and decay of any identifiable intermediate species in the reaction vessel over periods of a few milliseconds, and further by means of methods of plate photometry to put these observations on a quantitative basis.

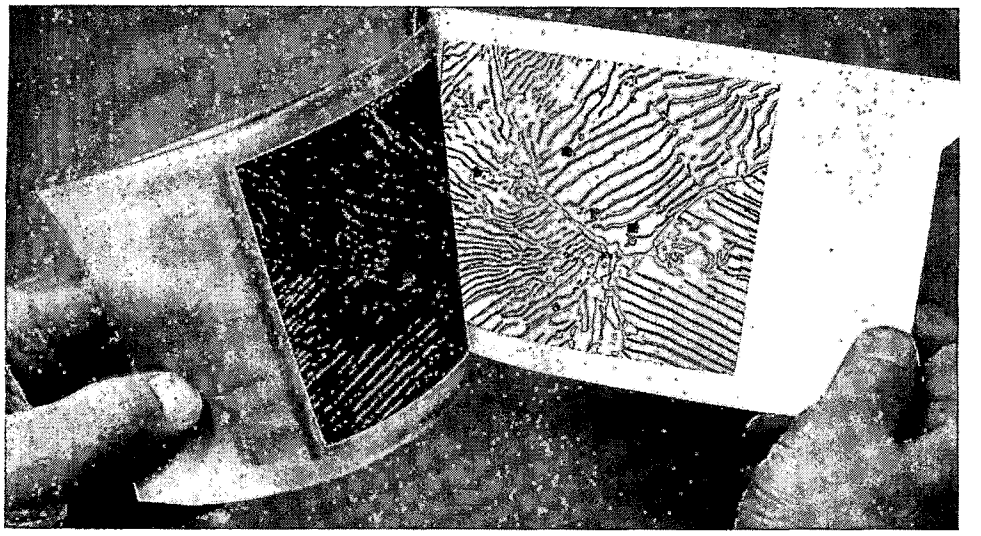
It soon became apparent that the early results of flash photolysis in the gaseous systems which I have previously exemplified were vitiated by a great rise of temperature, which resulted in thermal as well as photochemical dissociation. This arises from the fact that when the intense flash is applied to a suitably absorbing system such as chlorine, or nitrogen peroxide, the absorbed light energy is rapidly degraded to heat: it may be calculated that a reactant at a pressure of 1 mm absorbing one-thousandth of the emitted energy will instantaneously reach a temperature measured in some thousands of degrees. Only if a relatively large excess of inert gas (say 500 mm pressure) is present to act as coolant, or the process is carried out in solution, can this temperature rise be kept in check, and so limited to a few—say ten—degrees in the case of a gaseous system. Thus, under the adiabatic conditions resulting from lack of temperature control, thermal cracking or explosion as well as photolysis may be induced in suitable systems and the method of kinetic spectroscopy provides a valuable means for the observation of the rapid elementary reactions of free radicals or atoms occurring in explosive media initiated photochemically.

We thus have a comprehensive method which can be applied to "instantaneous" reactions in the gas phase both under adiabatic and isothermal conditions, and also to reactions in solution. It should be emphasised that, since the spectral spread of the photolysis flash while fairly continuous is limited by the transparency of quartz to 1900 Å, none of the phenomena of ionisation characteristic of the vacuum ultraviolet are involved in the reactions studied.

I shall now endeavour to give briefly some typical examples of the various types of reaction studied by the method of kinetic spectroscopy.

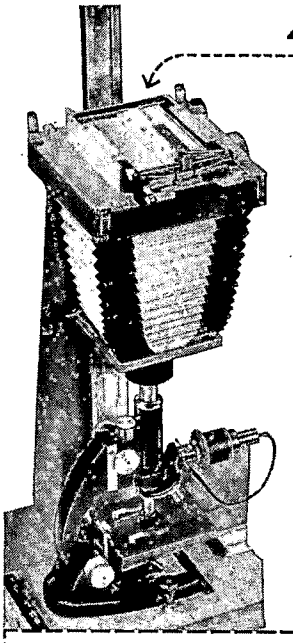
Adiabatic Reactions

To illustrate the thermal effect of the flash when no inert gas is added as coolant, we may cite the remarkable case of sulphur dioxide [4]. Plate I (*a*) shows a series of photographs of the absorption spectrum of sulphur dioxide at 1 mm pressure taken at increasing intervals after the photoflash. It is seen that the characteristic absorption disappears completely for 1 millisecond and is then gradually regenerated within the course of say 20 milliseconds, the final state being the same as the first. There is no appreciable photolysis of SO_2 which is feebly sensitive photochemically only below 2200 Å, and, added to this, when an excess of nitrogen is present as coolant the effect is completely suppressed: it is thus truly



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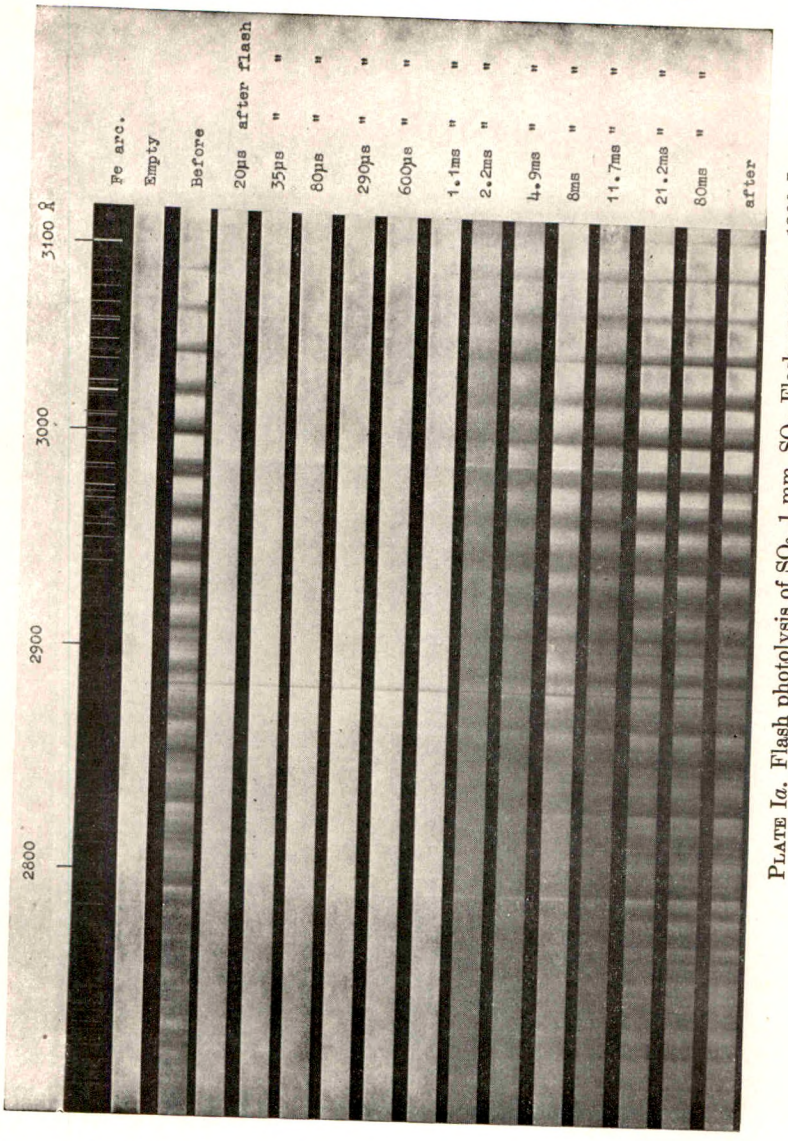
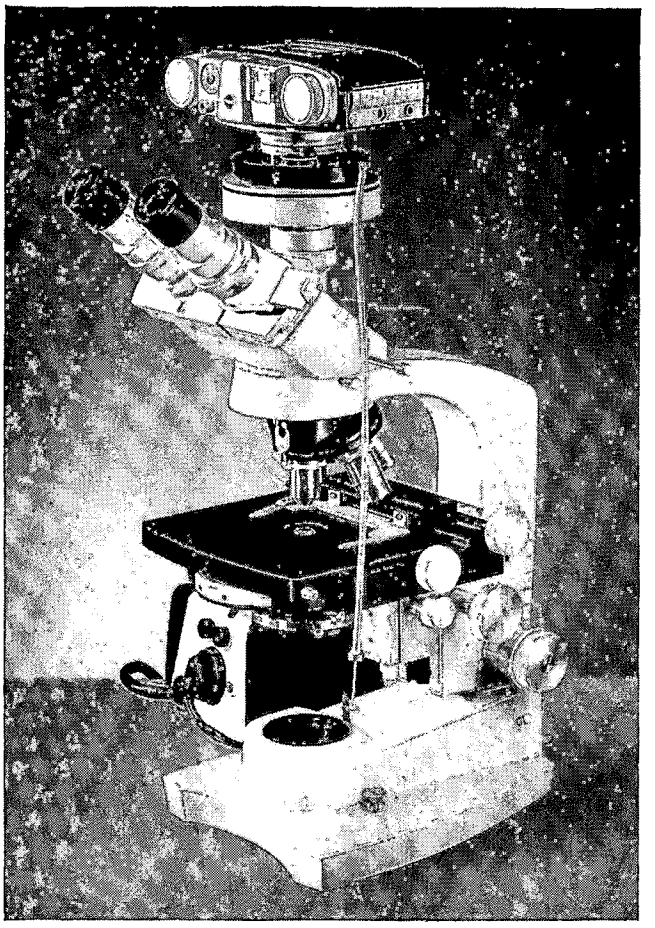
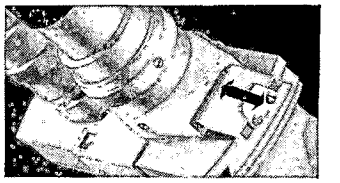


PLATE Ia. Flash photolysis of SO_2. 1 mm. SO_2. Flash energy = 1300 J.

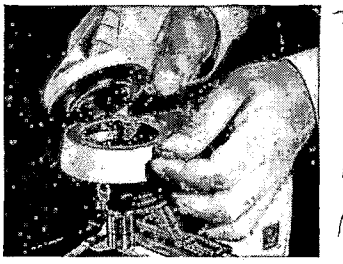
thermal in origin. It was at first suggested that the disappearance of the spectrum on flashing as in the case of that of chlorine under similar circumstances is to be explained by the supposed cooling effect of the walls on the strongly heated gas in producing a very high radial pressure gradient, so that the central core of the reaction tube is almost completely devoid of gas. That this is not true is proved by the observations of Oldershaw who, by extending the observations to shorter wavelengths, showed that a new transient band spectrum is generated between 2250 and 2400 Å immediately after flashing, together with a strong con-

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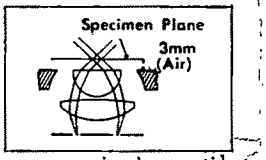


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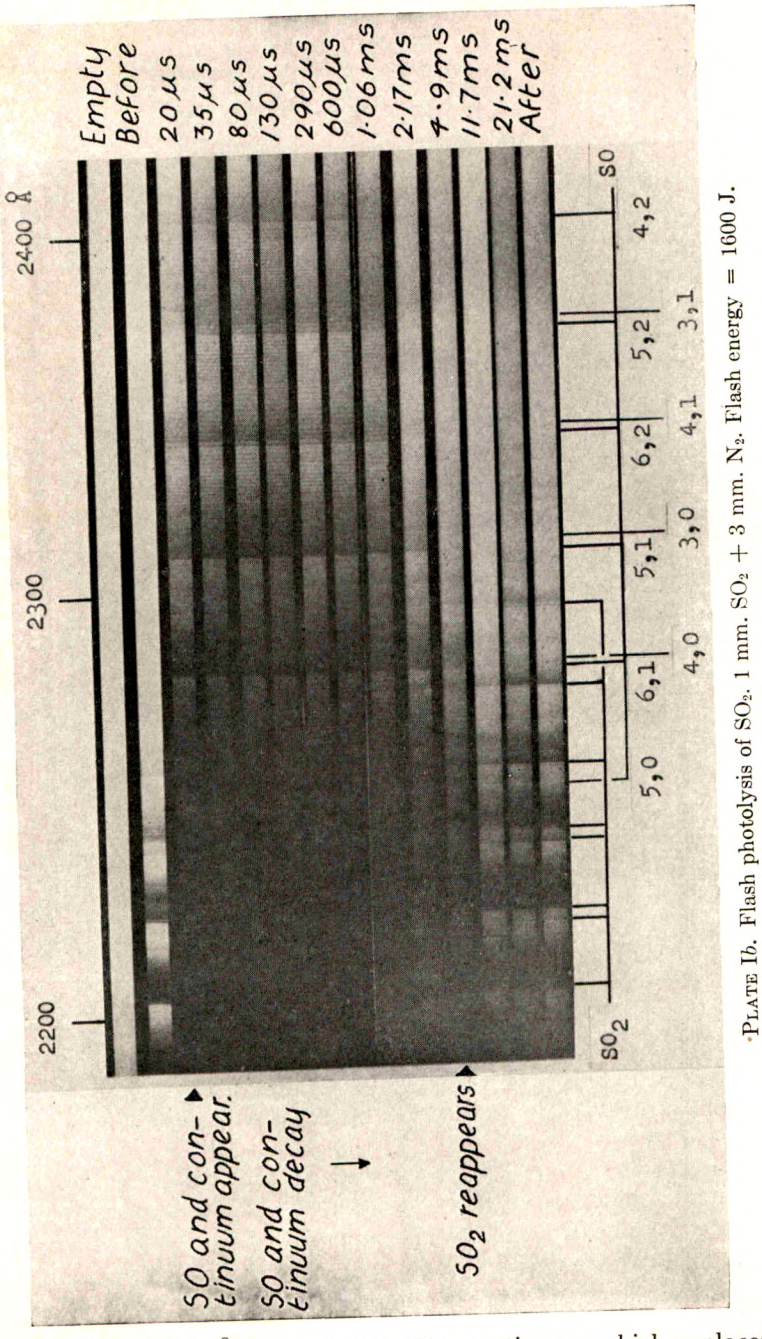


PLATE Ib. Flash photolysis of SO_2. 1 mm. SO_2 + 3 mm. N_2. Flash energy = 1600 J.

tinuum below 2250 Å (Plate I (*b*)). The continuum which replaces the SO_2 spectrum persists until the original spectrum is regenerated: it is completely complementary to it in time. On the other hand, the transient

band spectrum has been identified as that of the free radical SO; it agrees with the bands predicted by Martin's results for the known emission bands of SO at longer wavelengths. Its duration is markedly reduced by the addition of a little oxygen as would be expected if the SO radical were oxidised under these circumstances. None of these effects at short wavelength could possibly be observed if the disappearance of the SO_2 spectrum were due to the establishment of a pressure gradient as first suggested.

It may be estimated that on flashing without coolant the temperature of the sulphur dioxide is raised momentarily to 2500°C. There seems to be no other explanation than that some form of thermal isomer is generated reversibly at high temperature giving the continuous spectrum which reverts to that of normal SO_2 as the gas cools. It may be suggested that

$$SO_2 \rightleftarrows SO_2'$$

this corresponds to some form of metastable triplet state or bond dissociation, e.g.

$$\begin{array}{c} \cdot \quad | \quad | \\ O{:}S{-}O \end{array}$$

and that the SO radical is derived from the photolysis or pyrolysis of this isomer rather than the normal SO_2 molecule.

These observations, depicted in Plate I (*a*) and (*b*), are important to the understanding of the reactions of sulphur dioxide at high temperatures as well as the characterisation of the spectrum of the SO radical, which we have also obtained by the flash photolysis of sulphur trioxide under isothermal conditions.

Explosive Processes

The importance of flash photolysis to the chain processes of combustion lies in the fact that it can be used as a means of administering a quasi-adiabatic shock to the potentially reactant system as well as a source of the free radicals or atoms essential to the initiation of the reaction chains. With systems showing strong absorption, such as formaldehyde [5] or hydrogen sulphide [6] and oxygen, for example, either slow combustion or explosion can be induced according to the strength of the photolysis flash, and after an induction period of usually a few hundred microseconds, free radicals such as OH and HCO in the case of formaldehyde, and HS, OH and SO in the case of hydrogen sulphide, become apparent. By studying the rise and decay of these radicals against time under conditions of varying temperature and partial pressures of reactants, or added inert gases, much may be learned about the mechanism of the slow and explosive processes of these reactions which, when combined with classical kinetic studies, make possible a more complete and more

definite picture of the processes than was possible before.

For systems composed of hydrogen, carbon monoxide or hydrocarbons and oxygen, however, which do not absorb the radiation of the photolysis flash there is no response, since neither primary photolysis nor temperature rise can be induced; in such cases a photosensitiser is essential.

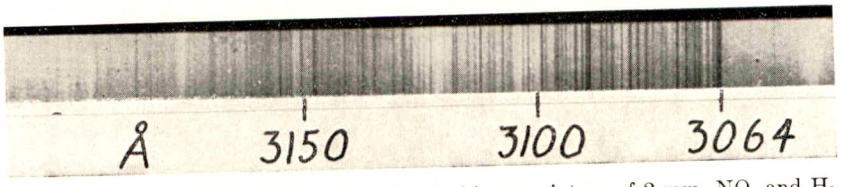


PLATE II*a*. O,O band of OH obtained by flashing a mixture of 2 mm. NO_2 and H_2.

Nitrogen peroxide absorbs strongly throughout the visible and ultraviolet and is photolysed by light of wavelength less than 4100 Å according to the equation

$$NO_2 + h\nu = NO + O$$

It is therefore an ideal substance, not only as an absorbent of radiation

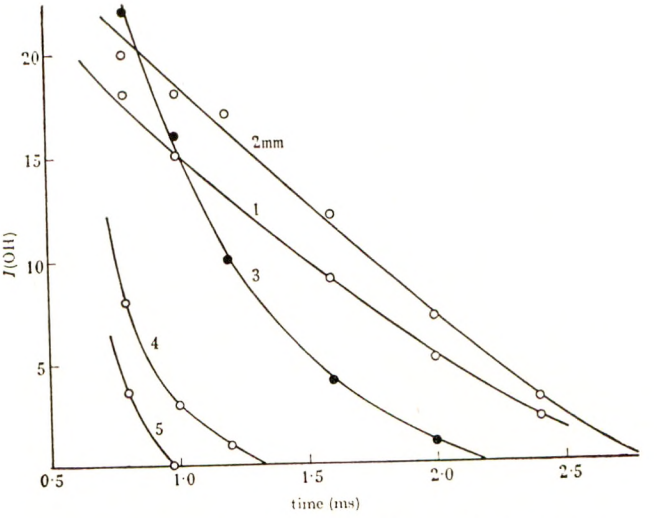


Fig. 2. OH intensity against time: effect of $P(H_2)$; $P(NO_2)$ = 2 mm. All figures against curves show pressures in mm.

to give rise of temperature, but also as a source of oxygen atoms capable of acting as chain initiators. Added in small quantities to mixtures of hydrogen, or hydrocarbons and oxygen, it acts as a most efficient sensitiser. In addition, for similar reasons, we have found that alkyl nitrites make excellent photosensitisers for hydrocarbon combustion.

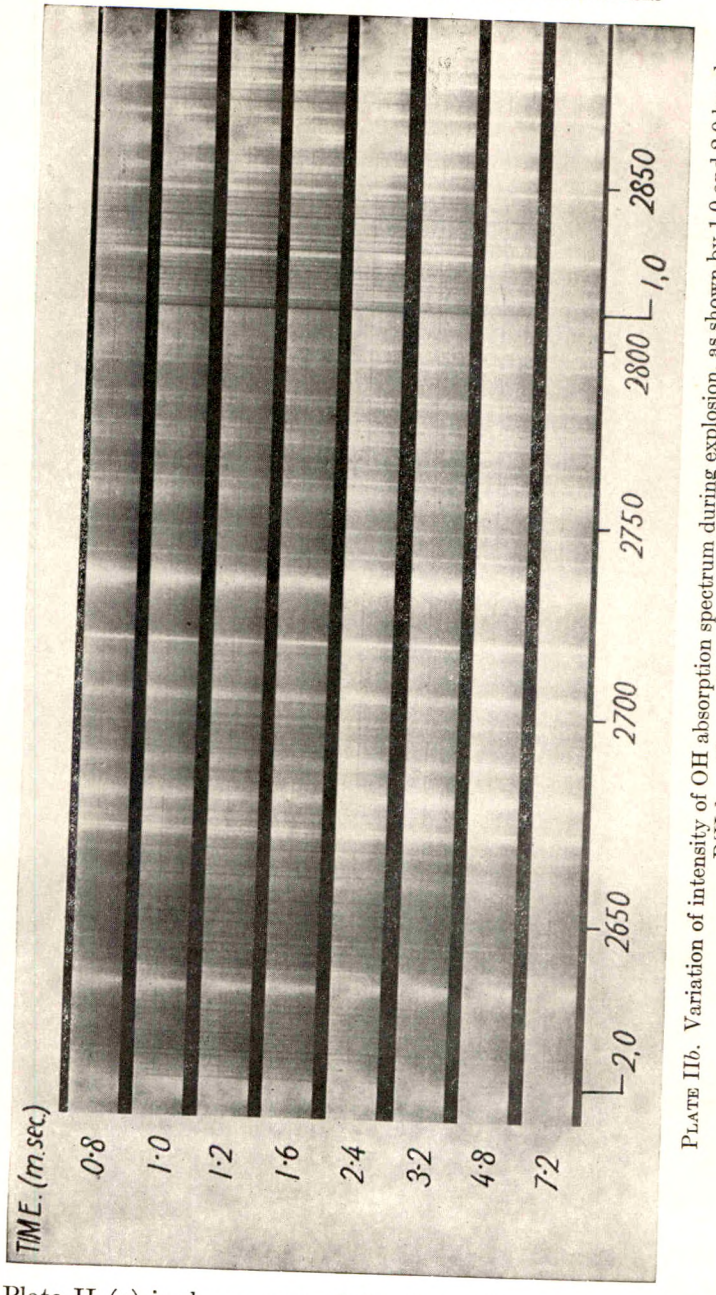


PLATE IIb. Variation of intensity of OH absorption spectrum during explosion, as shown by 1.0 and 2.0 bands. $P[H_2] = 10$ mm. $P[O_2] = 5$ mm. $P[NO_2] = 0.75$ mm.

In Plate II (*a*) is shown part of the absorption spectrum of the OH radical produced by flashing a mixture of 2 mm H_2 + 2 mm NO_2, a result which constitutes proof of the elementary reaction

$$O + H_2 = OH + H$$

PLATE III. (*a*) Absorption spectrum of CH: $^2\Sigma^+ - {}^2\Pi$ system. (*b*) Absorption spectrum of NH: $^3\Pi - {}^3\Sigma$ system. (*c*) Absorption spectrum of C_2: "Swan Bands." (*d*) Absorption spectrum of C_2: "Mulliken Bands." (*e*) Absorption spectrum of CN: $^2\Sigma - {}^2\Sigma$ 1.0 progression. (*f*) Absorption spectrum of CN: $^2\Sigma - {}^2\Sigma$ 0.0 progression.

In Figure 2 the decay of the OH radical spectrum is plotted as a function of time, the decay being more rapid the greater the excess of hydrogen over the equi-molecular stoichiometric mixture of $H_2 + NO_2$. This may be taken as evidence of the elementary reaction:

$$H_2 + OH = H_2O + H$$

Finally, it is found that the rate of decay of OH, when there is no excess of H_2 to remove it, is greatly reduced by adding increasing amounts of inert gas (N_2) showing how its removal by diffusion to the wall of the reaction vessel is hindered by increase of pressure. Thus three elementary reactions previously deduced from the kinetics of the hydrogen-oxygen reaction are objectively demonstrated.

In Plate II (*b*) is shown a record of an explosion of $2H_2 + O_2$ sensi-

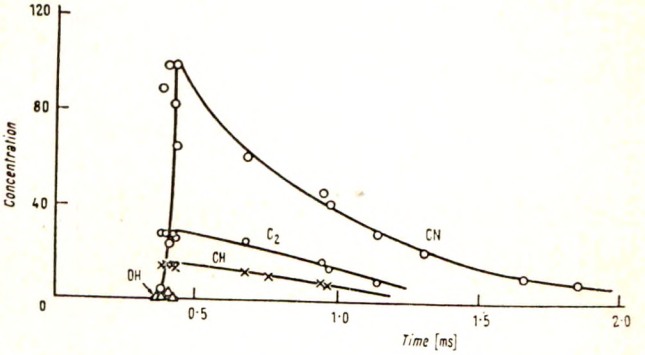


Fig. 3. Radical concentrations against time; $P(C_2H_2) = 13$ mm, $P(O_2) = 10$ mm, $P(NO_2) = 1.5$ mm.

tised by NO_2 in which the explosion as demonstrated by the growth and decay of OH can be clearly followed in both the 1.0 and 2.0 bands. Although the photograph was taken in the early days with a relatively long flash, an induction period of about 0.8 millisecond is evident, while the disappearance of the radical takes about 7 milliseconds [7].

An essential feature of flash photolysis as a means of studying combustion is that by its means it is possible to generate approximately homogeneous explosions, so that if the reaction tube is ½ m long, we virtually have a flame front which is ½ m thick and free radicals with suitable absorption spectra readily become visible spectroscopically.

It therefore happens that in the method of kinetic spectroscopy we have a unique method for observing some of the reactions in the flame and pre-flame stages of combustion which would not otherwise be visible. I shall limit myself to referring to some of our earlier work based mainly on experiments with acetylene [3]. The further development of this line of investigation involving the hydrocarbons of petroleum and the effect of

additives on their mechanism of combustion has proved most fruitful [8,9].

Plate III shows some of the radical spectra observed in absorption in the explosive combustion of acetylene, ethylene, methane and ethane; most prominent among these, in addition to the OH radical, are the CN bands (derived from the NO_2), the CH, C_2 and NH bands. In addition, the C_3 radical was detected for the first time in absorption being identified by the "4050 Å bands" previously known in cometary emission. In Figures 3 and 4 are depicted curves showing the waxing and waning of the free radicals mentioned above in the explosive combustion of acetylene, as measured on an arbitrary scale by plate photometry using the

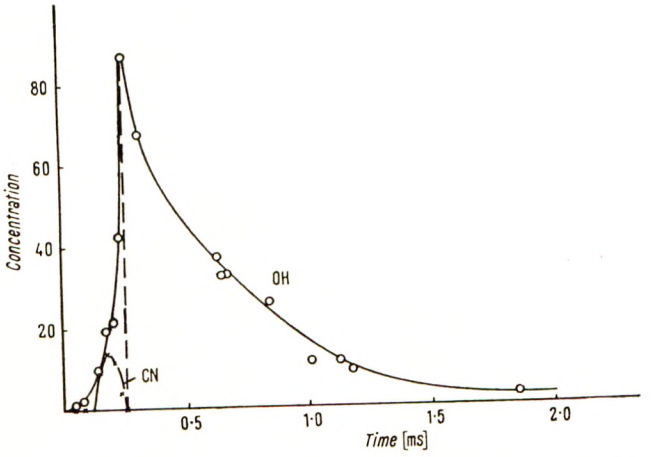


Fig. 4. Radical concentrations against time; $P(C_2H_2)$ = 10 mm, $P(O_2)$ = 10mm, $P(NO_2)$ = 1.5 mm.

two-path method. The appearance of these radicals is greatly dependent on the composition of the explosive mixture, being apparently conditioned by the stoichiometric equation of Bone [10]

$$C_2H_2 + O_2 = 2CO + H_2$$

according to which, if the fuel be in excess of the 1:1 ratio with oxygen, free carbon is formed as smoke, while if the mixture be oxygen-rich, steam is formed. Our results strongly support Bone's results as is shown by plotting the maxima of the free radical curves against mixture composition (Fig. 5). Counting nitrogen peroxide as oxygen for the purpose of combustion, it is seen that the OH radical is predominant in oxygen-rich systems while the carbon radicals appear in the fuel-rich mixtures, and the switch takes place sharply at the fuel: oxygen ratio of 1:1. At this point the only radicals visible are OH, CN and NH, the

latter being confined to a very narrow range of composition, and probably arising from the reaction

$$OH + CN = CO + NH$$

On examining Figure 4 it is seen that the hydrocarbon radicals all arise after an apparent induction period of about 0.5 millisecond. The OH radical under these conditions is barely visible but in all cases is seen slightly before the other radicals. If a tracer compound such as benzene be added to the mixture, its spectrum completely disappears before the end of the induction period and further, the bands of nitric oxide visible immediately before the end of the induction period show a high rotational temperature of over 2000°C. These observations lead us to the

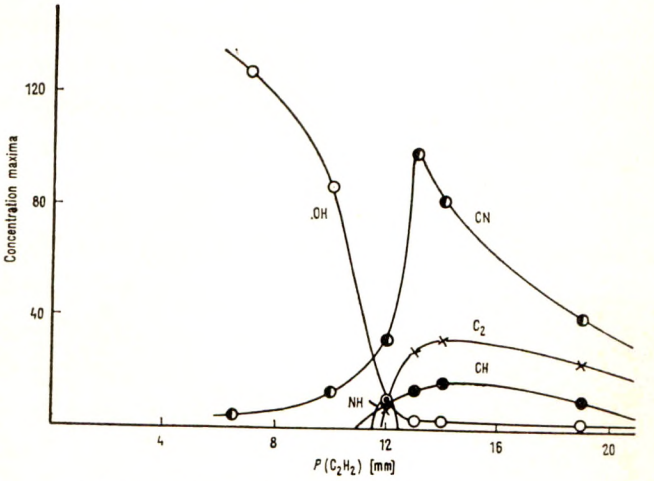


Fig. 5. $P(O_2) = 10$ mm, $P(NO_2) = 1.5$ mm.

conclusion that at the instant when the carbon radicals are released the explosion is over—that we are in fact witnessing the after-cracking of excess acetylene in the heat of the flame. The induction period undoubtedly marks the exponential development of pre-flame reactions, with temperature rising very strongly at the end, and the OH radical—the only intermediate species visible—appearing immediately before the explosion. In oxygen-rich mixtures, typified by Figure 3, the explosion is marked by the peak of the CN radical while the peak of OH absorption appears later as a result of the after burning of the hydrogen. Here again the OH radical is visible during the induction period, showing that it is involved in the pre-flame reactions.

The effect of additives to explosive combustion is illustrated in Plate IV which illustrates the photochemical ignition of heptane with and without the addition of lead tetraethyl [8]. The incubation period of the

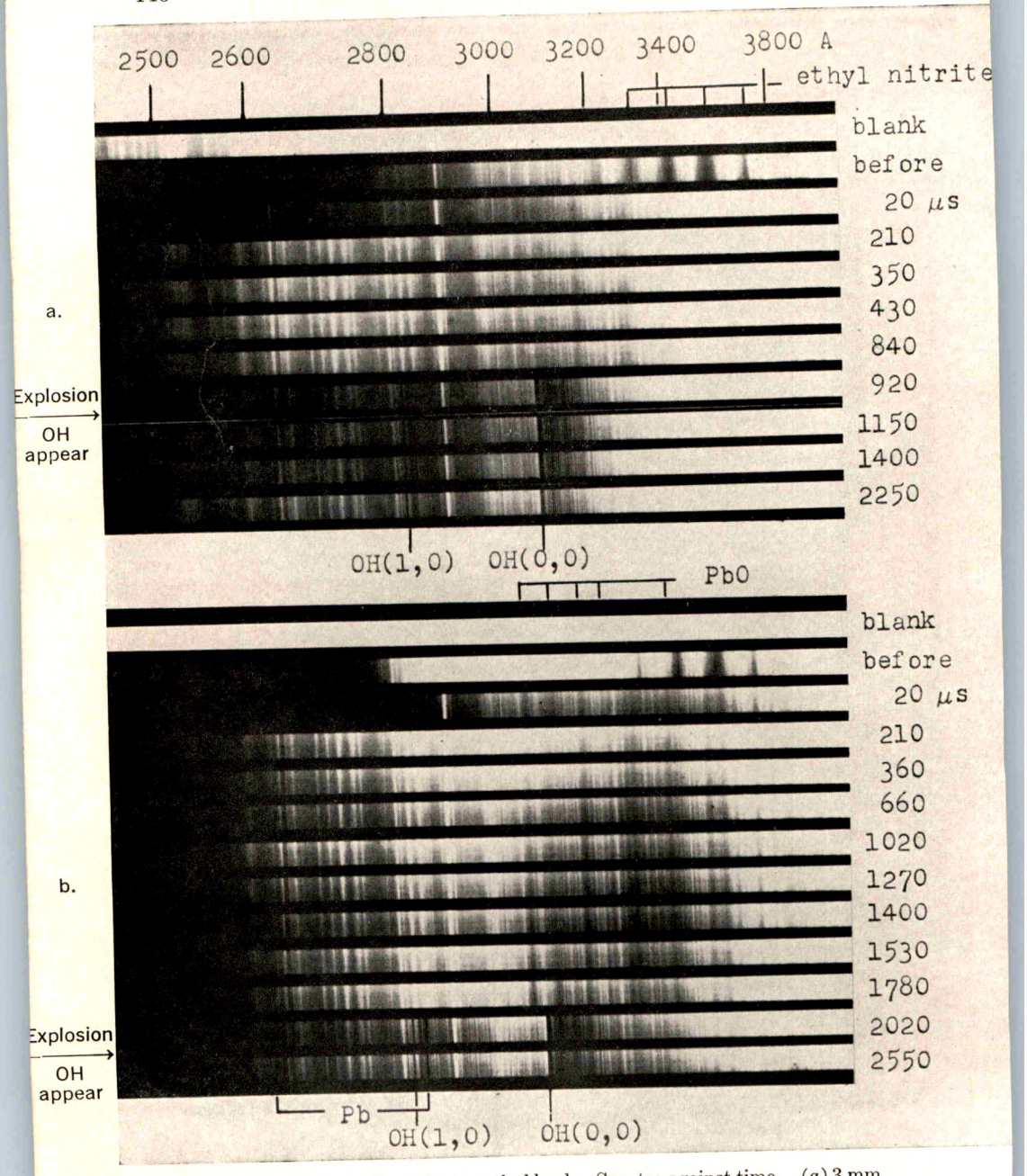


PLATE IV. Antiknock effect of tetra-ethyl lead. Spectra against time. (*a*) 3 mm. C_2H_5ONO + 3 mm. C_7H_{16} + 50 mm. O_2. (*b*) 3 mm. C_2H_5ONO + 3 mm. C_7H_{16} + 50 mm. O_2 + 0.14 mm. TEL. Flash energy 1000 J.

reaction is finalised in both cases by the appearance of the OH radical spectrum, at which point the stoichiometric combustion is complete; and the fact that the incubation period is increased more than two-fold by the additive, with the appearance of the lead oxide bands in absorption, has suggested that the anti-knock effect of lead tetraethyl is exercised in the pre-flame period and is associated with the production of lead oxide in the gaseous phase, and its dissociation to gaseous lead at the instant of ignition.

Free Radicals and Isothermal Gas Reactions

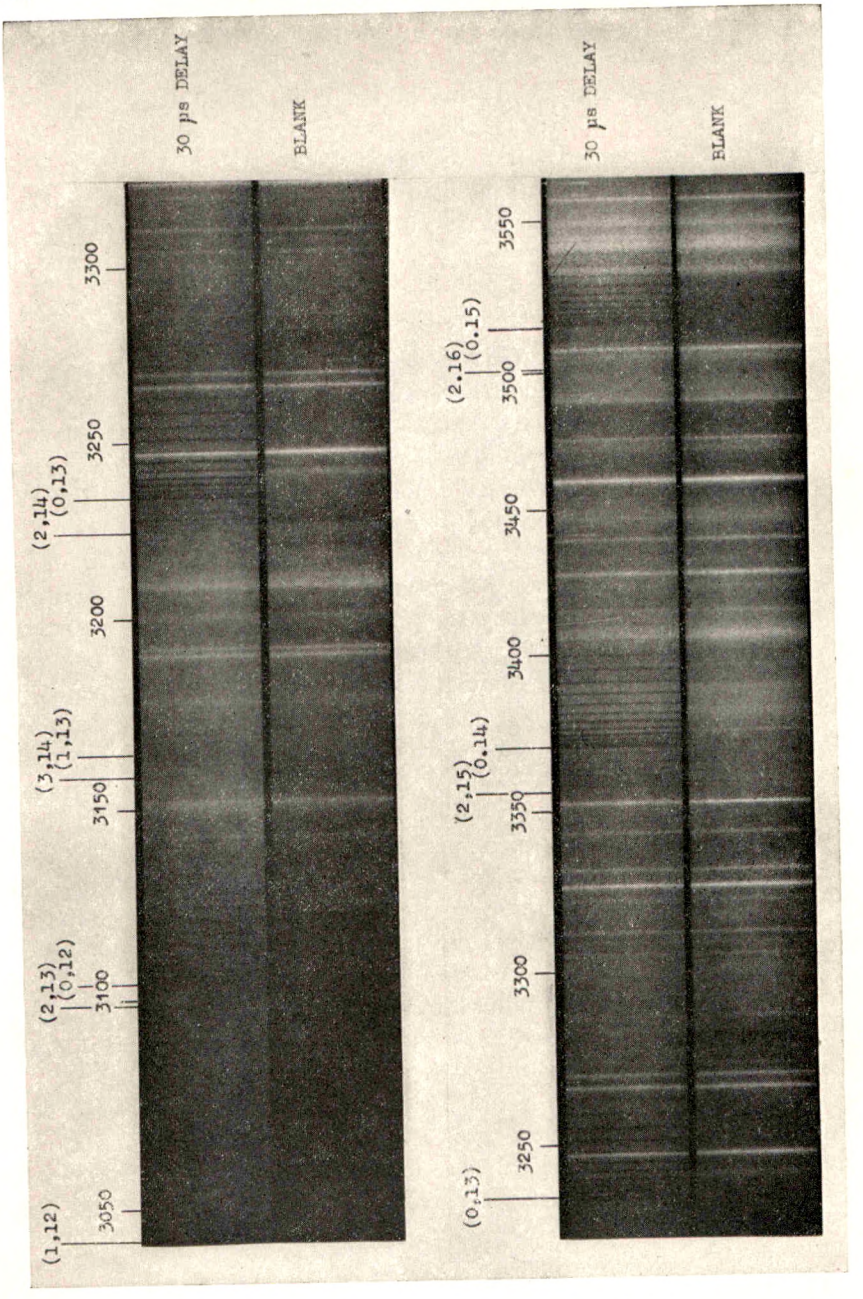
In addition to the free radicals referred to above, which have been produced in explosive processes, several others derived from primary photochemical processes have been identified by their absorption spectra under isothermal conditions. Mention may be made of the HCO radical [11] produced by the photolysis of aldehydes, the HS and HS_2 radicals from hydrogen sulphide [12], the N_3 [13] radical from hydrogen azide, the NH_2 [14] and NH [15] radicals from ammonia and the PH_2 [16a], PH and PO [16b] radicals from phosphine. One of the most extensively studied radicals, the ClO radical is obtained by the photolysis of chlorine dioxide [17] or chlorine monoxide [18] as well as by the photochemical reaction between chlorine and oxygen [19] or ozone [20]. Its transient spectrum which lasts only a few milliseconds has been analysed by Porter and Wright and its dissociation energy accurately determined, while the bimolecular decay to Cl_2 and O_2 can be readily followed. Recently Herzberg has isolated the CH_3 and CH_2 radicals [21] by flash photolysis and photographed their absorption spectra in the vacuum ultraviolet. Flash photolysis indeed provides a happy hunting ground for the spectroscopist and theoretical chemist interested in the ultimate detail of structure and energy levels of molecules and free radicals.

The method of kinetic spectroscopy not only makes possible the identification of free radicals produced by photolysis but also gives information about their stability and reactivity. I will briefly describe the flash photolysis of ozone to illustrate this.

The photolysis of ozone in the ultraviolet has been characterised as a chain reaction by Heidt and Forbes, the quantum yield reaching 7 under favourable conditions [22]. This was explained by the scheme proposed by Beretta and Schumacher [23] involving excited oxygen molecules, the essential features of which are

$$\begin{aligned} O_3 + h\nu &\rightleftarrows O_2 + O \\ O + O_3 &= O_2 + O_2^* \\ O_2^* + O_3 &= 2O_2 + O \\ O + \text{wall} &= \tfrac{1}{2}O_2 \end{aligned}$$

Owing to the simplicity of the reaction we are limited to a scheme of this kind, but the nature of the excited oxygen molecule was not clear, though



it was generally considered to be an unidentified excited electronic state.

By the flash photolysis of pure ozone in the presence of excess inert gas, we were however able to detect the spectrum of vibrationally excited oxygen in absorption (Plate V) [24]. This spectrum consisted of an

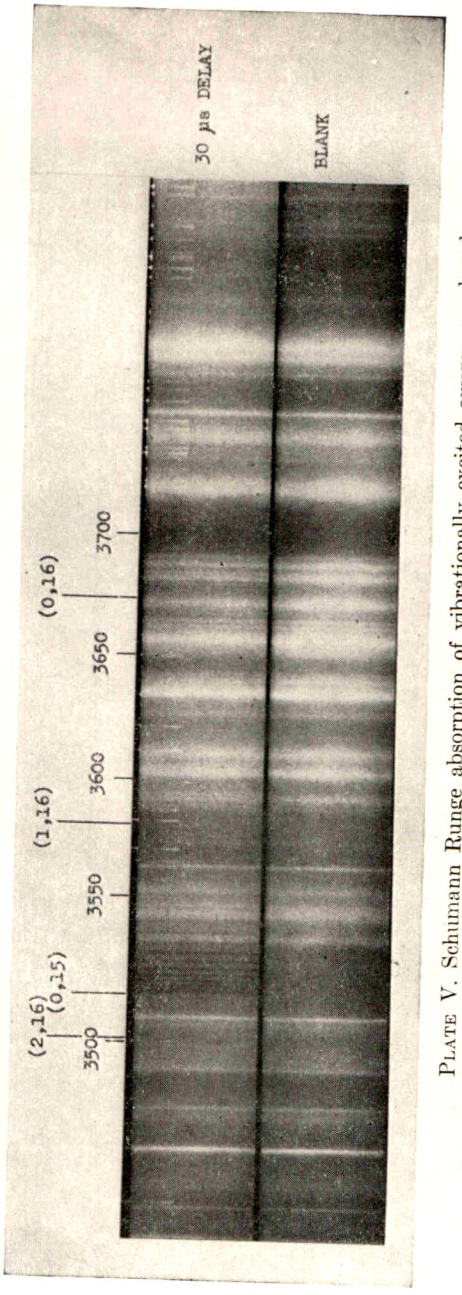


PLATE V. Schumann Runge absorption of vibrationally excited oxygen produced during the isothermal flash photolysis of ozone. O_3 pressure = 20 mm. Hg; O_3/N_2 ratio = 1/40. The three spectral regions are shown on pp. 148 and 149.

extension towards longer wavelengths of the spectrum of normal oxygen known previously only in the vacuum ultraviolet (the Schumann Runge bands) and indicated that the oxygen molecule is highly vibrationally excited, but otherwise "cold" as regards rotation and translation. More-

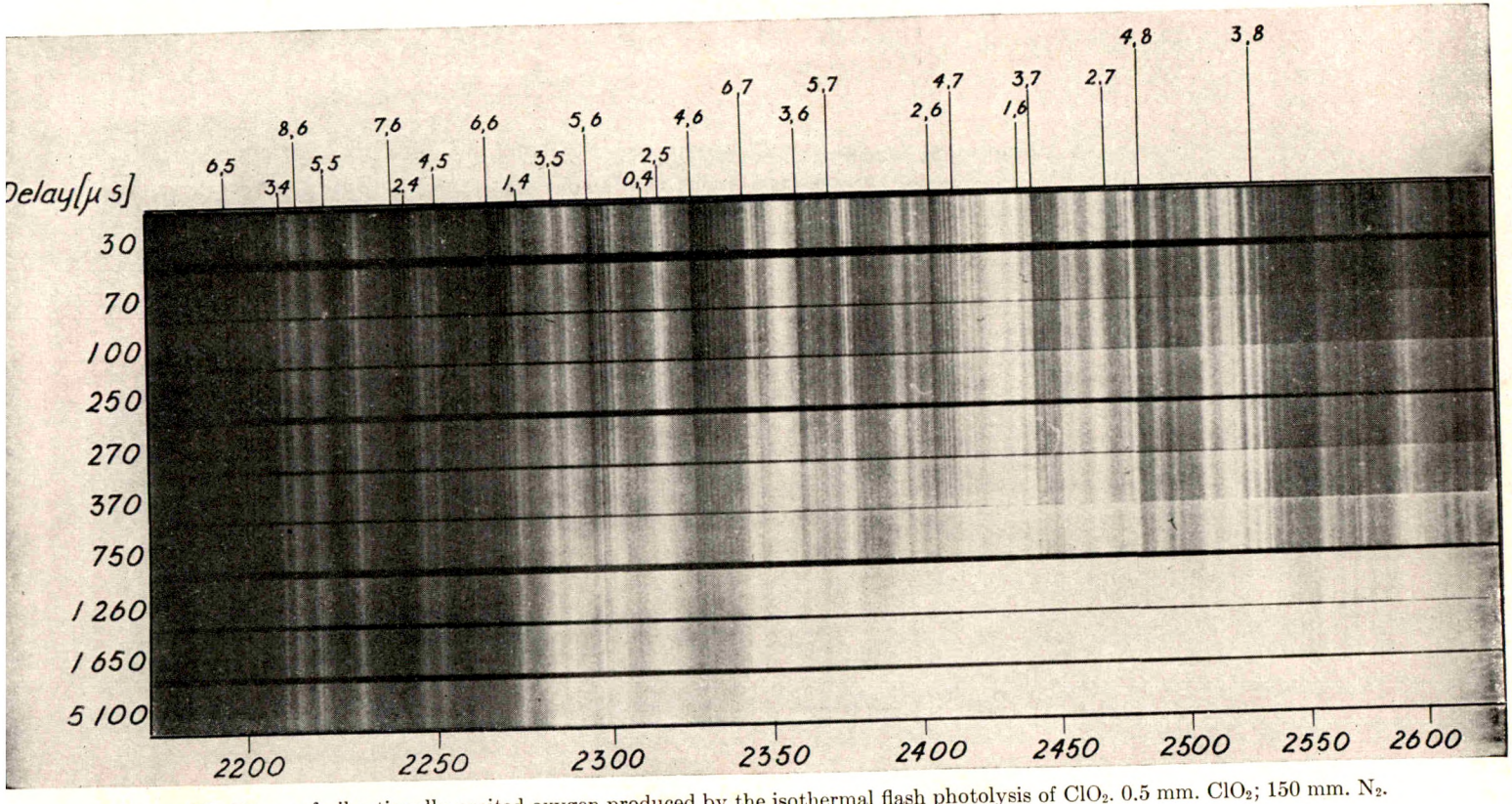


PLATE VI. Decay of vibrationally excited oxygen produced by the isothermal flash photolysis of ClO_2. 0.5 mm. ClO_2; 150 mm. N_2.

over, up to 17 quanta were detected in the vibration, corresponding to more than half the thermal energy of the reaction†

$$O + O_3 = O_2 + O_2$$

This remarkable fact brought to light a new generalisation which we were able to establish, namely, that with reactions of the type

$$A + BCD = AB + CD$$

a large fraction of the energy of reaction appears unequilibrated as energy of vibration in the newly formed bond AB—a circumstance of great importance to the understanding of the mechanism of chain reactions. In the case cited above, molecules of 18, 19, 20, etc., quanta of vibration are conspicuously scarce or completely absent; these are the molecules with just enough energy to bring about the endothermic reaction

$$O_2^* + O_3 = 2O_2 + O$$

and so to propagate the chain. They are immediately removed by this reaction. On the other hand, oxygen molecules with 17 quanta or less are not sufficiently energised. They have a life long enough to be seen in absorption, and their decay with time can be observed.

Plate VI shows the decay of similarly excited oxygen molecules produced by the flash photolysis of ClO_2 and derived from the reactions

$$\begin{aligned} ClO_2 + h\nu &= ClO + O \\ ClO_2 + O &= ClO + O_2^* \\ 2ClO &= Cl_2 + O_2 \end{aligned}$$

a process closely analogous to the ozone reaction.

Here again we have an example of the generalisation cited above. Many others have come to light, of which we may mention the following [25].

$$\begin{aligned} H + O_3 &= OH^* + O_2 \\ Cl + O_3 &= ClO^* + O_2 \\ Br + O_3 &= BrO^* + O_2 \\ O + H_2 &= OH^* + H \\ O + H_2O &= OH^* + OH \\ O + CH_4 &= OH^* + CH_3 \\ O + HCl &= OH^* + Cl \\ O + NH_3 &= OH^* + NH_2 \end{aligned}$$

In all cases but the first, which was discovered by McKinley, Garvin and Boudart by an alternative method, we have observed the vibrationally excited product spectroscopically.†† Always it is the molecule

† The oxygen atom derived from the ultraviolet photolysis of ozone has been proved to be in the first excited electronic state (^1D) [20,25]. The argument unfortunately cannot be reproduced here.

†† The oxygen atoms cited in the equations listed are derived from the photolysis of ozone, and are in the ^1D electronic state [20,25].

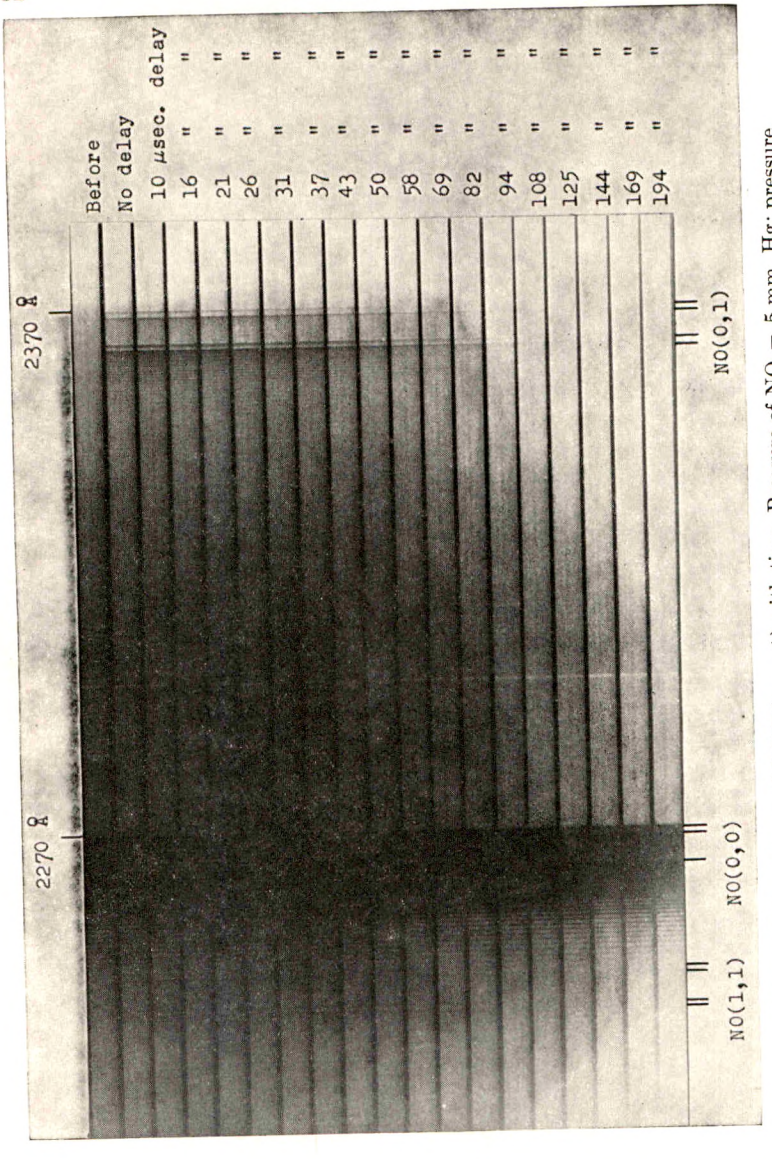


PLATE VII*a*. Decay of $NO^2\Pi(v = 1)$ with time. Pressure of NO = 5 mm. Hg; pressure of N_2 = 600 mm. Hg. Flash energy = 1600 Joules.

containing the newly formed bond which is stimulated vibrationally, taking preferentially a large part of the exothermic energy of the reaction.

Energy Transfer

In the cases cited above the vibrationally excited molecule is produced by chemical reaction. In addition we have been able to populate the vibrationally excited levels of the electronic ground state of nitric oxide without reaction [26]. This depends on excitation to an upper electronic

state followed by fluorescence as shown in the diagram (Fig. 6). In Plate VII (*a*) is shown the decay of the vibrating NO molecule produced in this way. It is possible to put the measurements on a completely quantitative basis and to determine the absolute efficiencies of collisions with various other molecules in removing energy from the vibrational to the other degrees of freedom. This process is known as relaxation, and its measurement has not before been possible because we were not previously able to provide a finite concentration of molecules exclusively stimulated in one vibrational degree of freedom. An accurate under-

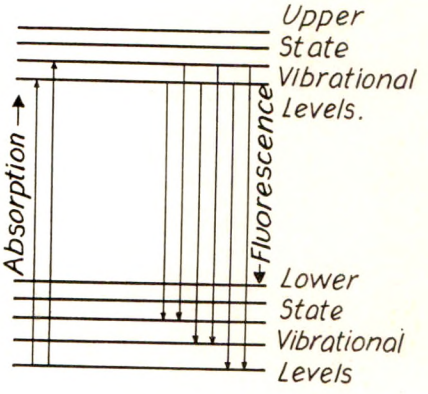


Fig. 6. Diagrammatic excitation of lower levels by fluorescence.

standing of the mechanism of collisional deactivation of this kind is of great importance to the theory of reaction kinetics in general.

Recently Callear has succeeded in populating the $^3P_0$ state of the mercury atom by flash irradiation [27], and to obtain its absorption spectrum. The rate of decay of the spectrum, of which one line is shown in Plate VII (*b*), can be measured photometrically, and the relative efficiencies of colliding molecules in degrading the electronic energy of this metastable triplet state determined. Observations of this kind will be of great importance to the understanding of the mechanism of photosensitised reactions to which I have already referred.

Reactions in Solution

Many reactions in solution may be studied by flash photolysis. In all such cases the system remains strictly isothermal. We may mention in particular the important investigations by Porter and his colleagues of the triplet or diradical state of aromatic hydrocarbons and dyestuffs [28]. These metastable states may be readily populated by flash irradiation, and are highly reactive. In particular, their study is very relevant to the fading of dyes by light, and to many photochemical reactions of complex molecules dependent on the primary generation of diradicals, such as the initiation of polymerisation [29]. In Plate VIII (*a*) the genera-

tion and decay of the triplet state of anthracene, which is typical of many similar aromatic hydrocarbons, is shown.

Plate VIII (*b*) is typical of the remarkable transient spectra observed on flashing alkali halides [30], identified as due to the intermediate X_2 ion formed in the electron transfer processes leading to the X_3 ion. This reaction is typical of a wide range of electron transfer reactions in solution which are available for study by kinetic spectroscopy.

The simple reactions which we have discussed above are typical of

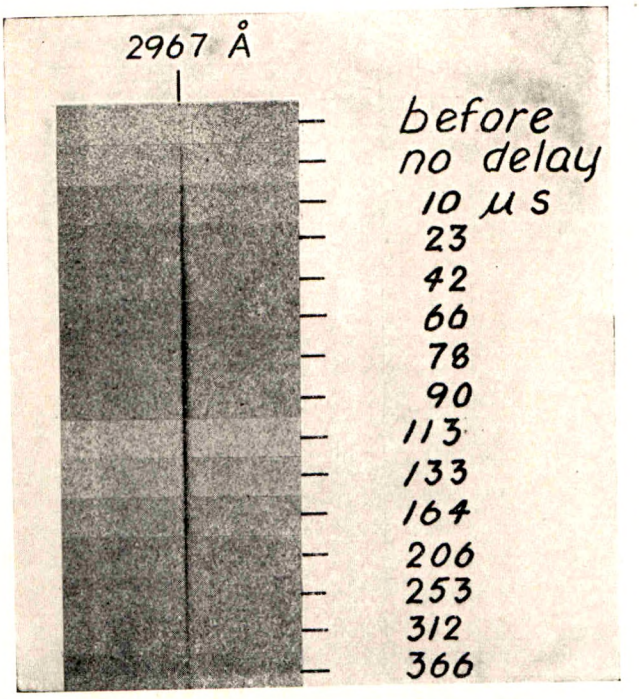


PLATE VII*b*. Absorption spectrum of Hg $6(^3P_0)$.

many others. We have already seen how they can give information about the mechanism of combustion, and so suggest means by which these processes can be controlled. This is not only true of hydrocarbons. It applies to the combustion for example of ammonia, hydrazine, hydrogen sulphide and aldehydes with oxygen or ozonised oxygen, and there is a wide and inviting field for the study of additives.

The very short ultraviolet rays of the sun which do not penetrate our atmosphere, bring about reactions in the upper atmosphere involving atoms and free radicals of the utmost importance to the geochemist, and much information can be obtained about these in the laboratory by the methods I have described. Thirty kilometers above the earth, oxygen is

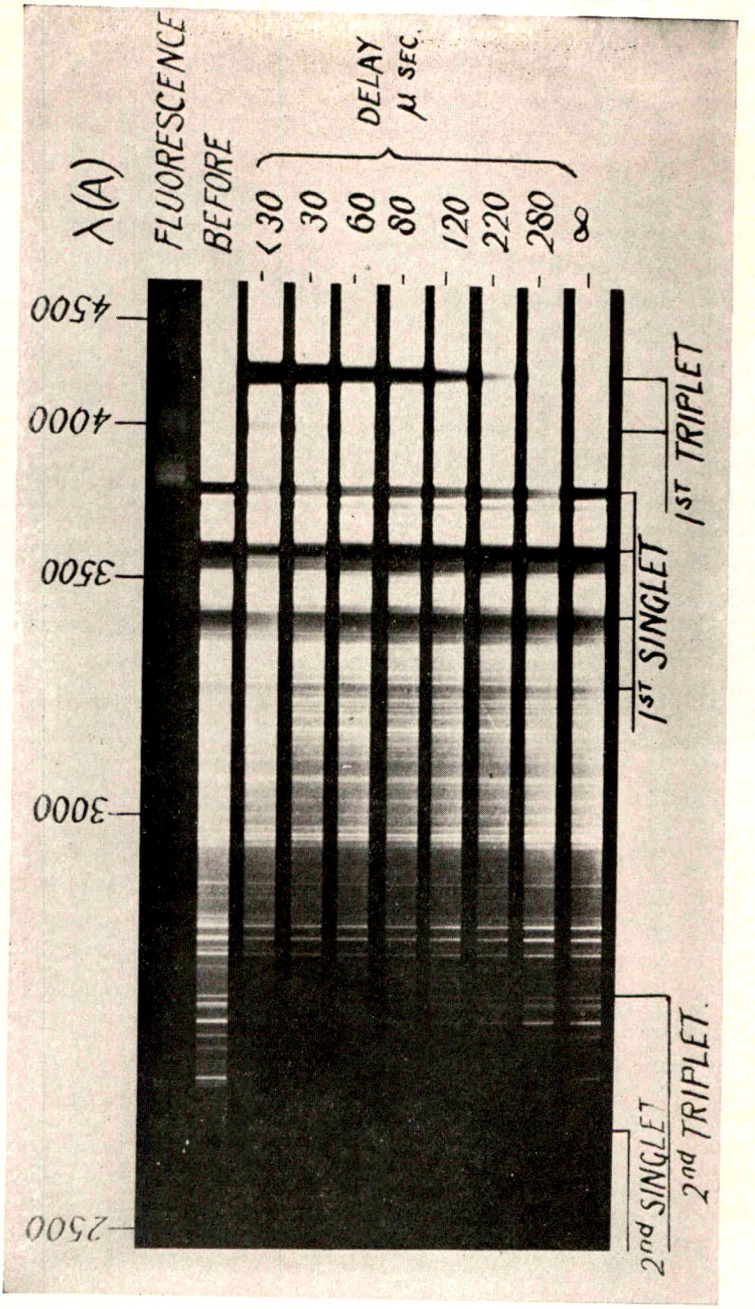


PLATE VIII*a*. The decay of the triplet state of anthracene produced at the expense of the normal (singlet) state by flash photolysis (Porter).

dissociated into atoms, and these form ozone by reacting with oxygen molecules. This produces a blanket of ozone round the earth, which ab-

sorbs the radiation of the sun of wavelength less than 2500 Å and, as we have seen, is destroyed in the process. Thus there is a stationary concentration of ozone produced which is being formed and destroyed at equal rates, and were it not for this protective canopy all life on earth would be destroyed by the short-wave radiation which it excludes. We have seen that the photolysis of ozone involves the generation of highly vibrating oxygen molecules showing an extension of the Schumann spectrum of oxygen into the region of the quartz ultraviolet, nearly as far as the visible spectrum (see Plate VI). One might predict that spectroscopic observation of the light reflected from the moon at sundown, would under favourable conditions of observation show evidence of the same spectral bands. The reaction of ^1D oxygen atoms derived from the

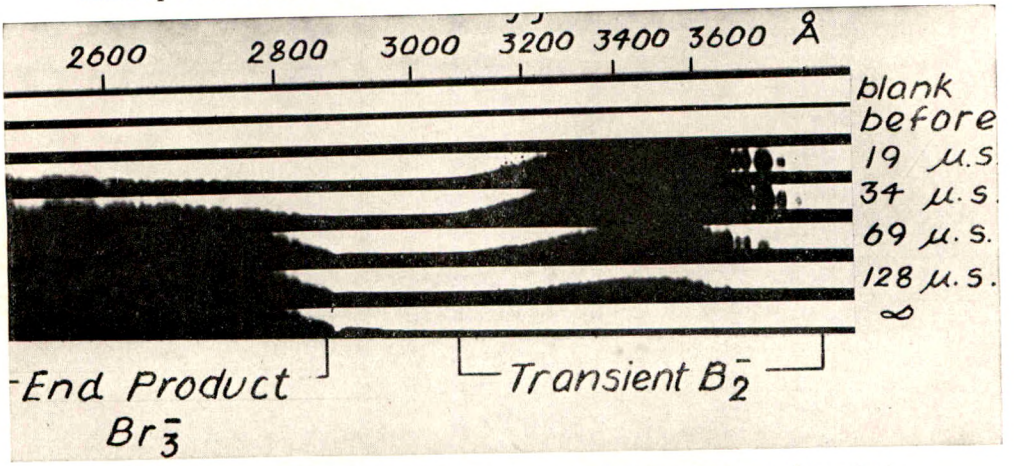


PLATE VIII*b*. Flash photolysis of 1.0 N KBr in water showing spectrum of transient Br_2. Flash energy 1280 J. For B_2^- on this plate read Br_2^-.

photolysis of ozone may also in part be responsible for the emission of the night sky. For example, Meinel has observed the vibration rotation spectrum of OH, the lower levels of which may in part be due to the reaction

$$O(^1D) + H_2O = OH + OH^*$$

The photolysis of water, ammonia and methane which are also present in the upper atmosphere should produce H atoms and free radicals of which the former can react with ozone to give highly excited OH radicals

$$H + O_3 = OH^* + O_2$$

All these reactions are of the highest meteorological importance and are in principle studied by flash photolysis.

It is now believed that our primitive atmosphere was reducing, composed mainly of hydrogen, together with methane, hydrogen sulphide,

water, carbon monoxide and ammonia, similar to the atmospheres of the major planets as we see them today. If the solar system had a uniform origin there must have been influences which changed our atmosphere and that of Mars from reducing to oxidising. The first is the progressive loss of hydrogen from our relatively weak gravitational field. The second is the photolysis of water and the other compounds, leading to the formation of formaldehyde and oxides of sulphur, phosphorus and nitrogen, which on interaction, according to the fascinating views of Urey [31] and Oparin [32], deposited the primordial organic scum on the earth and provided the necessary medium for the evolution of life. Many of the reactions involved in this brief survey can be studied by flash photolysis. Others, such as the photolysis of water, methane and other hydrocarbons, lie outside its scope, and provide a challenge to extend this powerful method to those shorter wavelengths of the ultraviolet which at present escape us.

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BIOSYNTHETIC POLYNUCLEOTIDES: MODELS OF BIOLOGICAL TEMPLATES*

By JACQUES R. FRESCO and DAVID B. STRAUS†

Nucleic acids are currently a major focus of investigation for the modern biologist. This is because it is now recognized that nucleic acids are the master molecules of living systems: these macromolecules play a central role in the hereditary process and in the dissemination of genetic information within a single generation. Among the great achievements of modern science has been the elucidation of discrete chemical steps involved in these processes. It has been found that the nature of hereditary information is the sequence of monomeric units in deoxyribonucleic acids (DNA) and that these sequences are related to the sequences of amino acids in proteins. The chemical structure of DNA has also been shown to account for the self-duplicating character of genes. The expression of genetic information, as proteins of specific structure and function, involves its direct transmission from DNA to a type of ribonucleic acid molecule (RNA) whose own sequence of nucleotides is deciphered at the site of protein synthesis with the intervention of other types of RNA. Direct correspondence between nucleotide sequences in RNA and amino acid sequences in proteins has now been determined. Thus, the code which has been deciphered since the beginning of life by the simplest cell is now intelligible even to *Homo sapiens*.

Studies with synthetic polynucleotides have been of great significance in these exciting developments. These simple analogues of nucleic acids have allowed the detailed examination of interactions between polymer strands and the effect of variation in nucleotide sequence on such interactions. Also, considerable insight into the three-dimensional architecture of these macromolecules has been obtained by such studies. Finally, investigations with polynucleotides have suggested the means by which genetic information is transferred from DNA to RNA, and then to proteins, and have also afforded an answer to the coding problem. In what follows, particular attention will be paid to those investigations made possible by the availability of enzymically synthesized polyribonucleotides.

Prior to the discovery of the enzymic synthesis of polyribonucleotides by Grunberg-Manago and Ochoa in 1955 [1], there was already an appreciation of secondary structure in DNA. The brilliant proposal

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† Established Investigator of the American Heart Association.

of Watson and Crick [2], coupling the X-ray diffraction data of Wilkins [3], and the patterns of base composition determined by Chargaff [4] with the results of their own deft model building, had drawn attention to a two-stranded helical polynucleotide structure which could account, in molecular terms, for the biological properties of DNA. This

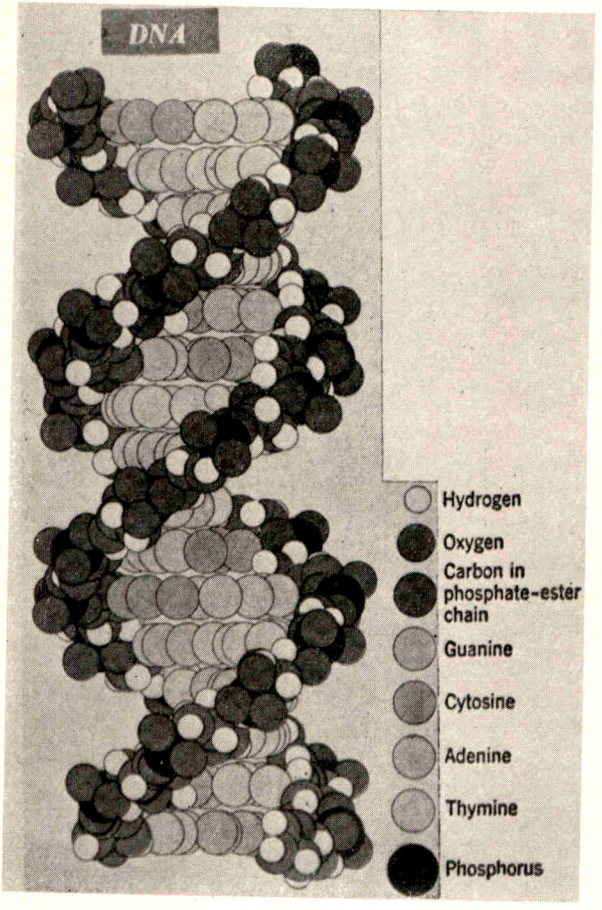


FIG. 1. A molecular model of a segment of the two-stranded DNA helix [5].

structure consists of two polydeoxyribonucleotide strands wound around the same axis to form a helix. The helix is held together by hydrogen bonds between adenine in one strand and its complementary base, thymine, in the other, and also between the other complementary base pair, guanine and cytosine in the respective strands (see Fig. 1 [5]). Since all bases are paired with their complements in this perfect helix, one chain is necessarily the complement of the other, and it appears that each chain serves as the template for the biosynthesis of its complementary partner. Subsequent work from many laboratories, (e.g.

[6–10]), which has amply confirmed this DNA model, has also shown that the integrity of the helical structure is required for some of the biological activities of DNA. This is not surprising since specific interactions between macromolecules must surely depend upon their unique three-dimensional architecture. At this time, however, definitive concepts about the macromolecular structure and function of RNA were lacking. The covalent structure of the polyribonucleotide chain was known [11] (see Fig. 2) and it was also recognized that RNA did not have the perfect periodic structure characteristic of DNA [12]. Moreover, it was already realized that RNA might well play a multifunctional role

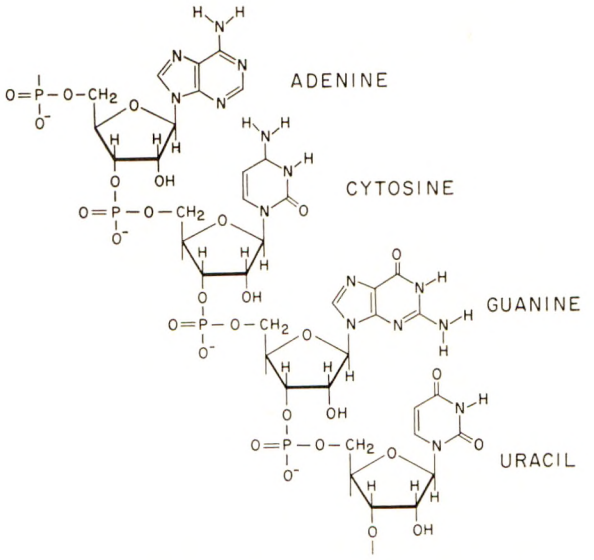


FIG. 2. A structural formula of a segment of an RNA chain showing the four principal monomeric units.

in the information transfer process and protein synthesis [13]. With the availability of biosynthetic polyribonucleotides, a systematic approach to the investigation of RNA secondary structure became possible.

Polynucleotide Phosphorylase

These RNA analogues are available through the use of the enzyme polynucleotide phosphorylase which is present in a number of bacterial species and in some higher organisms [14]. At present, the physiological function of this interesting cell catalyst is not known; it does not appear that this enzyme provides the major mechanism of RNA biosynthesis. Indeed, polynucleotide phosphorylase has a low substrate specificity, catalyzing the polymerization of a wide variety of ribonucleoside diphosphates including some not found in living organisms. The reaction is freely

reversible and the polymerization (forward) reaction involves the stepwise addition of nucleotide residues to growing chains with stoichiometric elimination of orthophosphate groups (equation 1) [15].

$$nXRP - P \xrightleftharpoons[\text{Enzyme}]{\substack{Mg^{++}\\ \text{Primers (?)}}} (XRP)_n + nP \qquad (1)$$

where X = base, R = ribose, P = phosphate

The number of growing chain ends appear to depend on the number of initiator molecules present in partially purified enzyme extracts [15,16]. When such extracts are highly purified, so that these initiator molecules are removed, a requirement for the addition of such initiator molecules is found [17]. If initiator is not added, a long lag period is observed before polymerization occurs, indicating that the highly purified enzyme preparation still retains some capacity to form initiator molecules.

The enzyme is capable of polymerizing substrates singly, to form homopolymers, as well as mixtures of nucleoside diphosphates to form copolymers of varying base composition [14]. In general, the composition of these copolymers reflects the relative concentration of substrate molecules in the reaction mixture. It has been shown that the sequences of different monomers in such copolymers is essentially completely random [18] and there is no evidence that the addition of either natural RNA or DNA to reaction mixtures can determine the sequences achieved in the synthesized polymers. It has been possible to synthesize homopolynucleotides containing four of the five bases in natural nucleic acids (polyadenylic acid, polycytidylic acid, polyuridylic acid, polyribothymidylic acid [14,19]).[1] Considerable difficulty has been encountered, however, in preparing a homopolynucleotide containing the purine base, guanine. While the reason for this is not known, it has been possible to synthesize a substitute in the form of polyinosinic acid which has some of the hydrogen bonding properties expected of polyguanylic acid. Nearly all the possible copolymer combinations have also been synthesized, including some containing the recalcitrant base guanine, e.g., poly AGUC. The molecular weight of the product polymers can be controlled [16,20] and preparations have been made with mean molecular weights from 5–10,000 up to several million by varying the reaction conditions.

Polynucleotide Interactions

Essential to the use of these synthetic polynucleotides as simple analogues of nucleic acids was the demonstration, by Heppel *et al.*, [21]

[1] The prefix *poly* followed by any one of the capitalized initials, A, G, C, U, T, I, refers to a homopolymer containing adenylic, guanylic, cytidylic, uridylic, ribothymidylic, or inosinic acid, respectively. A copolymer is indicated by the word *poly*, followed by the initials of its constituent nucleotides. A helical complex is indicated by *poly*, followed in parentheses by the summated homo and/or copolymer chain symbols.

that these polynucleotides possess the same phosphodiester linkage found in nucleic acids. This finding led to the description of the polynucleotides as "RNA-like." It is interesting, in view of this, that homopolynucleotides were first used, by Warner [22], to test the Watson-Crick proposal of complementary base pairing in nucleic acids. This was done by mixing solutions of poly A and poly U, each of which are single-stranded, and showing, by electrophoretic methods, that a complex was formed. It was also found that this complex formation is accompanied by a diminution of the ultraviolet absorption. Rich and his collaborators [23] then showed that the poly (A + U) complex has an X-ray diffraction pattern very similar to that of DNA. They also studied the stoichiometry of the interaction between the two polymers and discovered that two-stranded and three-stranded helices can be formed depending on the solvent [24,25]. These observations had several important consequences. First, since uracil bears a close structural relationship to thymine, direct evidence was obtained for the existence of one of the base pairs proposed for DNA. Secondly, these experiments emphasized what had already been discovered by Thomas [26] several years earlier, namely, that hypochromicity normally accompanies hydrogen bonding in polynucleotides. Finally, it became evident that studies with polynucleotides might lead to findings concerning information transfer between DNA, a two-stranded helix, and RNA, a single-stranded molecule.

These early studies were followed by a series of detailed investigations, in several laboratories, which revealed a number of principles concerning the molecular conformation and bonding potentialities of polynucleotide chains. The homopolynucleotides were, generally, found to exist as single-stranded molecules having the hydrodynamic properties of random coils in solvents which simulate physiological conditions [16,27,30], as opposed to the two- and three-stranded helices that are formed by mixing a variety of complementary homopolymers [22–25,31]. Under other conditions, all of these homopolymers can be induced to interact with themselves to form multi-stranded helices [16,27,32–34]. These coil to helix transitions (see Fig. 3) are accompanied by characteristic changes in such physical properties as viscosity, hypochromicity, and optical rotation. It thereby became apparent that polyribonucleotide chains have the same capabilities for helical secondary structure as DNA.

The preferred *ordered* structure for polynucleotides appears to be the multi-stranded helix. Such an arrangement removes the hydrophobic groups (bases) from the aqueous medium leaving the charged phosphodiester groups in contact with the solvent and maximally separated on the periphery of the helix. The stability of this helical architecture depends on a delicate balance between cohesive hydrogen bonds between the bases and disruptive electrostatic interactions due to the high charge density in the polyribosephosphate backbone of the polymers. Conse-

quently, its stability is favored only when the relative sequence of bases in the two chains makes possible a number of interchain hydrogen bonds, which is greater than that which can arise intramolecularly if the two strands are separated. Studies with synthetic polynucleotides enabled the realization of this obvious condition, whose recognition was crucial to

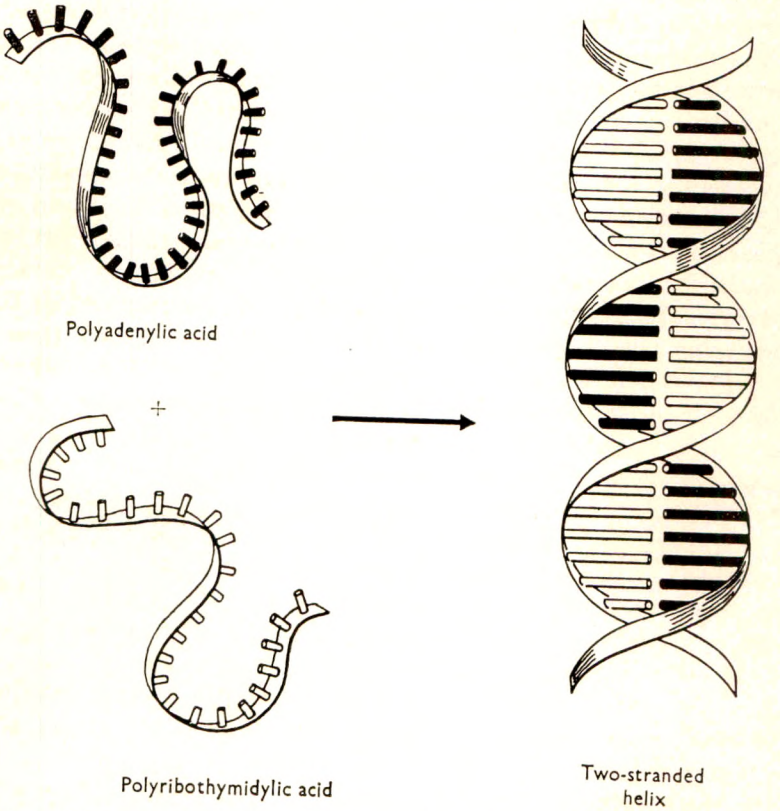


FIG. 3. A diagrammatic representation of a coil to helix transition (after Rich [35]).

the demonstration of strand separation and specific recombination in DNA [8,9], and to the development of a molecular pattern of secondary structure in RNA [36–38].

Strand Separation and Recombination

Essential to the proof of the Watson-Crick structure for DNA was the demonstration that two kinetic units could be derived from the single molecule. Such evidence was lacking until the very recent work of Doty, Marmur, *et al.* [8,9]. Earlier, it had not been found possible to demonstrate a lowering in the molecular weight of DNA when denaturation was induced at high temperature or at extremes of pH [39,40]. Thus, it was

thought that such long, intertwined chains could not unwind easily. Reinvestigation of this problem was undertaken after it became apparent from polynucleotide studies that such helical strands are separable and can be induced to combine with precise stoichiometry.

Two examples using the interacting system, poly A and poly U, can be used to illustrate the ready separation of high molecular weight polynucleotide strands under conditions which normally denature DNA. Warner [41] carried out countercurrent distribution of the poly (A + U) complex in a two-phase solvent system containing formamide, which disrupts hydrogen bonds between the heterocyclic base pairs, and was able to recover poly A from one end of the countercurrent system and poly U from the other. In effect, the solvent had denatured the helix and the distribution system prevented recombination of the two kinetic units by separating them. Another definitive proof of strand separation has been obtained by the demonstration that poly (A + A) helices are formed when solutions of the poly (A + U) complex are acidified [16]. The formation of such two stranded poly (A + A) helices, with the elimination of poly U from the helix form, obviously depends on the complete dissociation of the initial poly (A + U) helix. This interchange,

$$\text{poly (A + U)} \xrightarrow{H^+} \text{poly (A + A)} + \text{poly U},$$

is due to titration of the adenine residues on lowering the pH, which destroys one of the hydrogen bonds between adenine and uracil in the two-stranded poly (A + U). In the poly (A + A) helix, protonation of the bases not only makes possible a pair of hydrogen bonds between adenines in opposite strands, but also an electrostatic bond between the positive sites of one strand and the negative phosphodiesters of the other.

Prior to investigations of synthetic polyribonucleotides, the two-stranded structure of DNA was presumed to arise as a unique attribute of the biosynthetic mechanism. The demonstration of *spontaneous* formation of two-stranded poly (A + U), however, suggested that such helices are stable, and often the thermodynamically preferred structures, and also that their formation need not be enzyme-directed. Similar spontaneous two-stranded helix formation results on mixing poly I and poly C [31], poly A and poly I [35], and other pairs of homopolynucleotides. Even more striking was the finding that homopolymer chains can interact with copolymers to form two-stranded helices when there is a substantial degree of interchain complementarity, as in the mixture poly AU and poly U [37].

The interaction of polynucleotide strands to form helical complexes is accompanied by a marked decrease in ultraviolet absorbance.[2] For long

[2] Absorption of ultraviolet light by nucleotides and polynucleotides is due to the resonating systems of electrons in the heterocyclic bases, and this hypochromicity has been related to interactions made possible by the close parallel stacking of the hydrogen bonded heterocyclic bases perpendicular to the helix axis along its entire length [42].

polynucleotide chains, the hypochromic change reflects, in a linear manner, the proportion of nucleotides involved in the interaction. Thus, by titrating one species against another, it is possible to determine the mixture of maximum hypochromicity, and thereby accurately ascertain the molar ratio of the reacting species in the complex formed. This titration method was used by Felsenfeld and Rich [25] in determining that poly A and poly U interact to form helical complexes containing one poly A strand and either one or two poly U strands.

One of the remarkable features of these titrations (see Fig. 4) is the

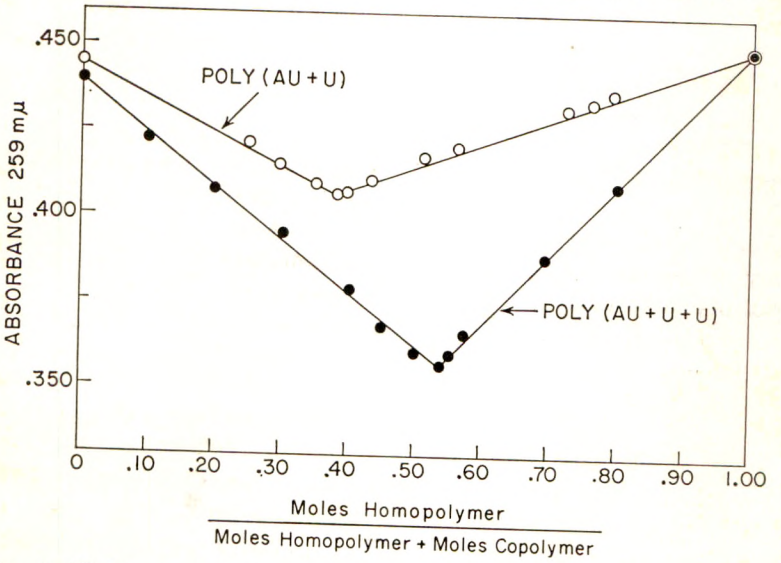


FIG. 4. Mixing curves for homopolymer-copolymer interaction. The copolymer in this case, poly AU, contains 66 moles per cent adenine. The inflection point for the two-stranded helix occurs at a point on the abscissa, where the moles of uracil in poly U (titrant) are equivalent to the moles of adenine in the poly AU. In the three-stranded helix, an additional equivalent of poly U titrant is taken up before the inflection point is reached. It is necessary to alter the solvent in order to obtain the three-stranded helix [37].

sharp break at the minimum absorbance (maximum hypochromicity). This abrupt inflection point defines the composition of the helix formed by the interacting polymer strands. The sharpness of the end point indicates that interchain reaction between bases is complete. Since the polymer strands are usually of different length, the involvement of *all* complementary residues in hydrogen-bond interactions demonstrates the capacity of the strands to explore each other so that complete fulfillment of their hydrogen-bonding capacities is realized. If this were not the case, interaction between chains of unequal length would give numbers of unbonded residues, and this would result in an ill-defined inflection at the end point of the mixing curve [43].

The interaction between homopolymer strands can be viewed as relatively simple since all the bases would be expected to form hydrogen bonds with their complements. That such interactions can occur when all bases are not in register became evident when it was found that titrations of copolymers, such as poly AU, with homopolymers, such as poly U, also give sharp end points in mixing curves (Fig. 4) [37]. It is interesting that in such mixtures the end point occurs where moles of homopolymer base equal the moles of complementary base in the copolymer. A variety of other homopolymer-copolymer interactions have now been studied [44–47] and, in each case, the same precise stoichiometry is exhibited. These facts are also relevant to the formulation of a structure for RNA and will be considered later.

The recombination of DNA strands is, of course, a much more formidable problem than homopolymer-copolymer interaction. In the former case, exact matching of a very large and complex sequence of bases involving four *different* pairs (A-T, T-A, G-C, C-G) *and* many *different* DNA molecules[3] is required in order that hydrogen bonding be complete. Studies with synthetic polynucleotides suggested that exploration of one strand by another to fulfill their complete hydrogen-bonding capabilities is an inherent property of these polymers. The DNA problem did not differ in kind, but only in complexity, and means were sought for maximizing the possibilities of complete exploration between separated DNA strands. By maintaining denatured DNA solutions (strands initially separated) under equilibrium conditions, where hydrogen bonds may rapidly form and break, nearly complete renaturation is achieved as shown by recovery of biological activity and other independent criteria [8,9]. Thus, strand separation and recombination can be shown to occur at all levels of polynucleotide helix complexity: simple complementary homopolymer interaction, intermediately complex homopolymer-copolymer interaction, and highly complex copolymer-copolymer interaction as in DNA.

Ribonucleic Acid

Several years ago it was recognized that purified RNA, from any source, in neutral saline solution, is a relatively compact polymer, lacking the regular periodicity characteristic of a two-stranded helical polynucleotide such as DNA [12,48]. It was clear, moreover, that RNA occupies less volume than a simple linear polyelectrolyte of similar charge density, and this gross conformation in solution suggested the presence of a large number of intrachain contacts in RNA. It remained to determine whether these contacts occur at random or constitute a definite

[3] Most DNA preparations studied, are heterogeneous mixtures of different DNA helices since many DNA molecules are required to carry out the genetic functions of any given organism and because isolation processes result in scission of the long DNA chains.

secondary structure. Studies of synthetic polynucleotides, particularly those based on the optical properties of these polymers, have shown that the second alternative is correct [36] and have provided insights concerning the nature of this secondary structure [38].

A particularly useful experimental technique in evaluating the secondary structure of polynucleotides depends on the reversal of the hypochromic effect which is found on mixing complementary homopolymers. In other words, when the hydrogen-bonded helical structure is destroyed, a hyperchromic change occurs [36]. The hydrogen bonds which stabilize these helices can be broken in a variety of ways, one of the most informative being to raise the temperature. In this fashion, a profile of

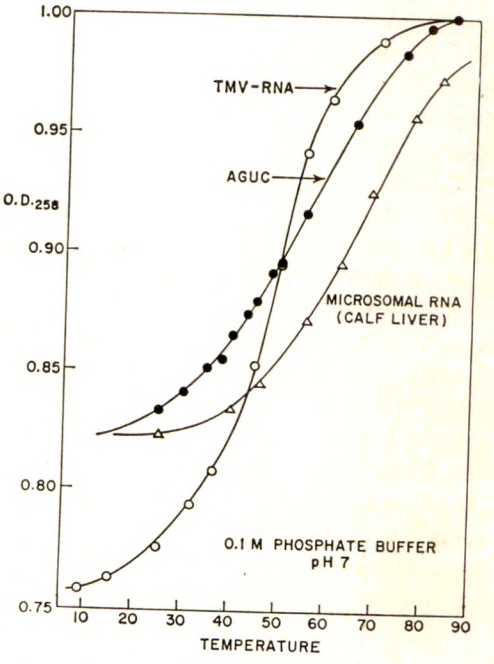


FIG. 5. Absorbance-temperature profiles of solutions of synthetic and natural single chain polynucleotides. Note the variation in amount of hyperchromic change and T_m [36].

absorbance at ambient temperature *vs.* temperature can be obtained which shows the course of the denaturation process. On heating a completely helical complex, such as poly (A + U) or DNA, the hyperchromic change takes place over a very narrow temperature range, less than 10°C. This absorbance increase parallels similar abrupt changes in the intrinsic viscosity and optical rotatory power found on heating such complexes and, therefore, can be attributed to the collapse of an ordered structure. Since these macromolecules can be viewed as crystals in one dimension (along the helix axis), their denaturation has been termed "melting," and the temperature at the mid-point of the helix-coil transition designated as the melting temperature, T_m. It is obvious, then, that, under

identical conditions of measurement, T_m reflects the relative stability of helices being examined. Several factors, related to heterogeneity in the substance, can be expected to broaden the temperature range over which the hyperchromic change occurs [36].

A hyperchromic change is also observed when a solution of RNA, from any source, is heated and absorbance measured at the ambient temperature [12,36,49], (See Fig. 5). Unlike the helix-coil transition of poly (A + U) or DNA, the temperature range over which RNA melts is very broad (60–70°C.). This type of melting profile is not inconsistent with the gross conformational properties of RNA in solution [12,49,50]. In addition, the magnitude of the absorbance increase suggests that a large proportion of the bases in RNA are hydrogen bonded and probably arranged in such a manner as to allow interaction between their π electron clouds [42]. This indicates that RNA has a secondary structure.

When these observations were first made, however, it was not clear whether this secondary structure arises from hydrogen bonds between individual strands or from intrachain interactions. Unfortunately, with the possible exception of RNA isolated from tobacco mosaic virus [49,51], the number of strands in RNA has not been demonstrated unambiguously.[4] However, a large body of evidence has accumulated from study of synthetic polynucleotides suggesting that these interactions are intramolecular.

Studies of polynucleotide synthesis catalyzed by polynucleotide phosphorylase indicate that the enzyme does not direct multistrandedness but, rather, is inhibited in both forward and reverse reactions (see equation 1) by the presence of multistranded helices [52,53]. The molecular properties of poly A [16,30,54], poly U [29], and poly I [55], as well as the copolymer poly AU [56], in neutral saline solution, at room temperature, are consistent with their being single strands. This takes on added significance because poly A [27], poly C [57], poly AU [37,44,56], and poly AGUC [36,58] all show similar hyperchromic changes, and melting profiles of the same form as those observed with different RNAs. In addition, the conformational change appears to be readily reversed on cooling in RNA, as well as in these polynucleotides [36], in contrast to DNA which has much more stringent requirements for reversibility [9]. Therefore, it can be said that polynucleotides can have secondary structure similar in kind and magnitude to that of RNA without being multistranded.

Accepting the single-strandedness of RNA, it is proper to inquire as to the form of its secondary structure and the proportion of residues involved. The hyperchromic change observed on heating RNA solutions

[4] We are concerned here with the *number* of molecular-kinetic units which give rise to the secondary structure of RNA, but not with the possibility of sub-units whose attachment is apparently not required for the integrity of secondary structure.

indicates that the hydrogen-bonded base pairs are stacked, as shown by recent theoretical analysis [42,59]. A decrease in the optical rotation of RNA solutions is also observed on heating and it is congruent with the hyperchromic change as is also found with completely helical polynucleotides [36,49]. The magnitude of this rotatory change, about 200° at the sodium D line, is so large, on going from the structured to the unstructured conformation of RNA, that it is explicable only in terms of a *helical* distribution of the optically active centers in the structured molecule [60]. These findings show that RNA must contain regions of stacked,

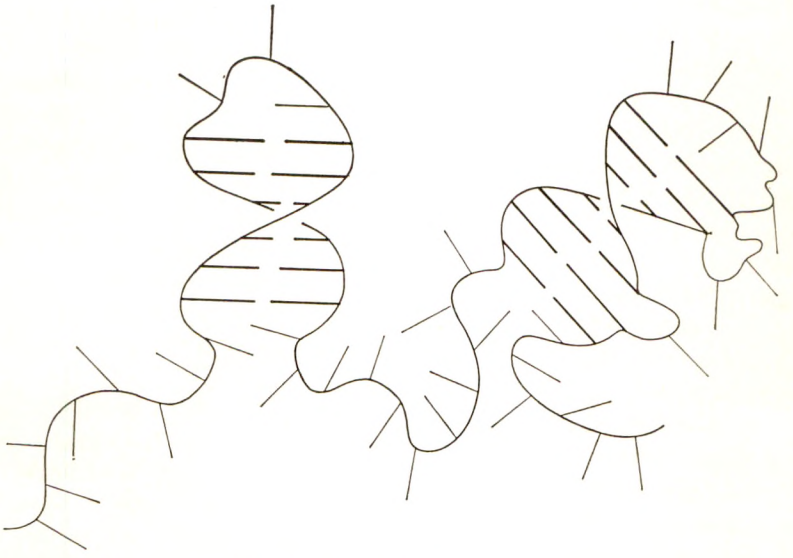


FIG. 6. Secondary structure in RNA. Note the helical twists in the hairpin turns of the single strand and the distribution of non-bonded bases indicated by the thin lines.

hydrogen-bonded bases, helically arranged. The most likely way in which these requirements can be fulfilled in a single-stranded RNA, is for the chain to form hairpin turns that are helically wound and maintained by hydrogen-bonded base pairs as in DNA [36] (see Fig. 6). In other words, the single-stranded RNA molecule is so folded that a number of short DNA-like helical regions are present. Effectively, then, the secondary structure of RNA is of the two-stranded type.

The proportion of residues comprising the helical regions in RNA has been estimated to be between 50–80 per cent depending on the RNA sample [36,60,61]. This has been done by comparing the changes in chromicity and optical rotation obtained on heating RNA with the corresponding changes found on heating the completely helical poly (A + U). That 20–50 per cent of the bases in RNA are *not* hydrogen-

bonded is consistent with the compact conformation of the molecule and the broadness of the absorbance-temperature profile. With respect to the latter, then, it is evident that heterogeneity among helical regions within a molecule, as well as molecular heterogeneity, are contributing factors. Of course, in the molecularly homogeneous RNA from tobacco mosaic virus, the broadness of the profile must be due primarily to heterogeneity among helical regions.

A large number of hydrogen-bonded base pair combinations have been shown to occur in synthetic polynucleotide structures [see 36,61]. Among these, it has been found that A-U and I-C are the most stable. It is further recognized, from extensive studies relating the G-C content of different DNAs with their T_m [62], that this pair is even more stable than A-U. Direct correlation between G-C content and T_m is not possible for RNA because there is no equivalence between G and C, or any other two bases, as there is in DNA. However, it has been possible to correlate the probability of occurrence of G-C pairs in RNA with T_m [36,63]. This correlation, together with the stability data and other information [see 38], strongly suggests that the only base pairs in the helical regions of RNA are A-U and G-C [38].

It is of some importance to the understanding of RNA structure and function to determine the minimum number of hydrogen-bonded bases required to form a stable helical region, and also the relationship between helix length and stability. This problem has been approached in two ways using synthetic polynucleotides. In the first, homogeneous oligo A (short chains), prepared by hydrolysis of poly A followed by column chromatography, was allowed to interact with poly U and the stability of the complex determined [64,65]. As might be expected, T_m increases with increasing oligo A chain length. A most interesting finding is that even the dinucleotide forms a helix with poly U. The helix-forming capabilities of oligo A were also examined under conditions where poly A forms a two-stranded helix [65]. In this case, the shortest length of oligo A forming a stable helical complex with another chain of the same length was four. These observations have made it possible to estimate that the minimum size of stable helical regions in RNA, at 25°C., is about four nucleotide pairs.

Another factor which must affect helix stability is the distribution of non-bonded bases in relation to helical regions. Since RNA contains a number of helical regions, at least some of the non-bonded residues must separate such regions. It is also possible, of course, that some non-bonded residues occur as defects within helical regions. Such defects must be present in the helical complex formed from the interaction of poly AU and poly U and similar copolymer-homopolymer mixtures because the copolymer strand contains some residues that are non-complementary to those of the homopolymer. These complexes have provided useful

models to study the way in which such defects can be accommodated [37].

One can imagine two ways in which this could be structurally effected. In one (see Fig. 7), the non-complementary bases are stacked within the helix but without hydrogen-bond interactions, like a ladder with missing rungs. The alternative is that the non-complementary bases are rotated out of the helix so that *normal* hydrogen-bonded base pairing of subse-

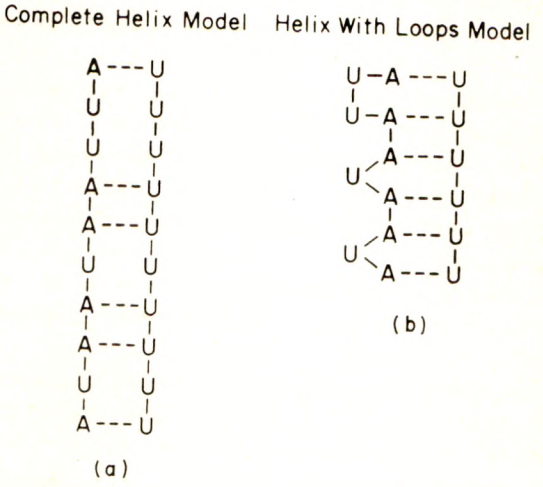


FIG. 7. Structural alternatives for accommodating non-complementary residues in two-stranded polynucleotide helices. (a) In any case of the Complete Helix Model the molar residue ratio of poly U:poly AU will always be 1:1 for the two-stranded complex. If a second strand of poly U is added, this ratio becomes 2:1. (b) In the Helix-With-Loops Model the molar residue ratio of poly U:poly AU varies with copolymer composition. In the example shown, this ratio is 6:10 for the two-stranded complex and 12:10 when a second strand of poly U is added [37]. Compare with Fig. 4.

quent sequences of nucleotides, that are in register, becomes possible. The non-complementary residues would thus appear as loops on the periphery of the helix.

These alternative hypotheses were tested [37] by measuring the stoichiometry of the interaction between poly AU and poly U, as was described above (see Fig. 4). The fact that, at the end point of the titration, the complex contains moles of homopolymer equal to moles of complementary base in the copolymer shows that the helix-with-loops model is correct. It has also been observed that the helix-with-loops is less stable than the equivalent complete helix.

These observations show how defects can occur as part of helical regions. Indeed, it is necessary to postulate the presence of such loops in the helical regions of poly AGUC in order to account for its helical content [38]. Poly AGUC contains 50–60 per cent of its bases in helical re-

gions as estimated from the hyperchromic change (see Fig. 5). Statistical analysis of a random nucleotide sequence, such as in poly AGUC, shows that, when loops are not allowed and A-U and G-C are the only base pairs, fewer than 20 per cent of the bases can occur in helical regions even if two adjacent base pairs are accepted as the critical size. The discrepancy between the observed and predicted values for helical content suggests that another model is required. The helix-with-loops model, on the other hand, predicts that about 60 per cent of the bases should occur in helical regions of the random poly AGUC, assuming a minimum helix size of four base pairs. This agrees with the experimental observation [38].

The helical content of RNA is even higher in some cases than that found for poly AGUC, and it is tempting to assume that principles which determine the secondary structure of synthetic polynucleotides apply as well to RNA. As a first approximation, then, it can be assumed that loops are also present in the helical regions of RNA (see Fig. 6). Of course, it is very unlikely that RNA contains a random sequence of nucleotides. Instead, since RNA molecules are biologically specific, they must consist of specific nucleotide sequences, and these sequences might possibly favor helical regions devoid of loops; and again they might not. In any case, it is clear from the polynucleotide studies considered that the architecture of RNA is determined for each molecule by its own nucleotide sequence.

Information Transfer

Replication and expression of genetic information are divergent processes, both essential to life, that originate with DNA. While both involve information transfer, the first, DNA → DNA, is a direct transmission; but the other, DNA → protein, entails a complicated translation process involving several RNA intermediaries. These transmission and translation processes occur via molecular mechanisms that depend upon the specific secondary force interaction capabilities of nucleic acids and their constituents. It has been possible to test some hypothetical molecular mechanisms involved in information transfer using synthetic polynucleotides.

Replication of DNA is now known to involve the polymerization of a new polydeoxynucleotide strand complementary to the parent template strand. Changes introduced in the nucleotide sequence of the parent strand and errors made during the replication process itself can be expected to result in an altered progeny DNA strand which, in turn, will be eventually expressed as altered proteins. Such alterations in DNA are believed to be the molecular basis of mutation, that is to say, hereditary change. Genetic fine structure analysis [66,67] has suggested that alteration of a very few base pairs, perhaps only one, in DNA can result

in changed phenotype, so that it is of interest to consider, at the molecular level, those alternations affecting one or more base pairs which might change the information contained in DNA.

Genetic fine-structure analysis has emphasized that some reversible mutations could involve the substitution of non-complementary bases during DNA replication [66,67]. It has been suggested also [37,68] that certain irreversible mutations can, instead, be explained in terms of a deletion or addition of one or more nucleotides in the growing chain. The helix-with-loops model (see Fig. 7) has suggested plausible mecha-

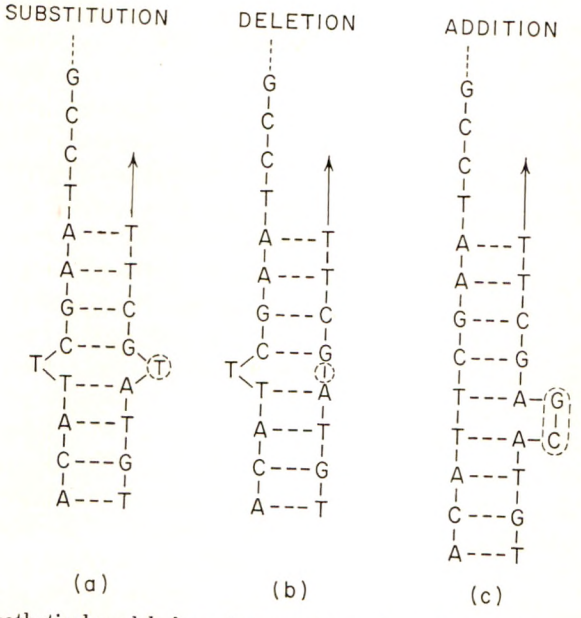


FIG. 8. Hypothetical models for point mutations. In each case, the template chain is the one on the left. Note that while the templates are identical in each case, the sequence in the growing chains varies, depending on the kind of "mistake" made during replication. The "mistake" in the growing chain is indicated by a dotted circle [36].

nisms whereby these three types of spontaneous alteration can occur during DNA replication [37]. In the first of these (see Fig. 8a), both the substituted base and its non-complementary partner are looped out of the growing helix during DNA synthesis so that succeeding regions can pair normally. In this case, there is no difference in chain length between parent and progeny strands.

A deletion alteration, (see Fig. 8b), results in a progeny strand which is shorter than the parent strand depending on the number of nucleotides deleted. Such a deletion might also result from an erroneous incorporation of a non-complementary nucleotide. However, in this case,

the close proximity of a residue in the parent chain, complementary to the erroneous nucleotide, favors loop formation in the parent chain. This kind of deletion can also be imagined to occur when a pre-existing distortion in the parent strand renders some of its residues inaccessible as template sites.

In the last type of alteration (see Fig. 8c), addition of non-complementary residue(s) to the growing chain results in a progeny chain longer than its template. Here, the errors have been removed from the helix, to enable succeeding chain growth to proceed normally.

On further consideration of the erroneous introduction of non-complementary nucleotides into the growing strand, it becomes obvious that the changes in the DNA helix formed are mechanistically identical. Whether the progeny strand is the same length, shorter, or longer than the template, depends on the complementarity relationship between the erroneous nucleotide incorporated in the growing strand and neighboring nucleotides beyond the error in the template strand. Thus, this classification scheme for mutations, based on the loop hypothesis, depends only on the relative lengths of progeny and parent polydeoxynucleotide strands and does not reflect different origins of the primary mutational event. Moreover, it should be emphasized that the same considerations can apply to both induced and spontaneous mutations resulting from substitution, deletion, and addition of nucleotides in the parent genome before DNA replication.

In view of the requirement for RNA, but not DNA, in protein synthesis [13], it seems very likely that the expression of the genetic information involves its direct transfer from DNA to RNA. The principle of base-pair complementarity suggests a mechanism whereby this can occur, and a logical extension of these arguments leads to the prediction that DNA should play a directing role in the synthesis of some types of RNA, and also that there should be a DNA-RNA intermediate. Both these predictions have now been experimentally demonstrated [69–75].

Significant to these developments have been experiments designed to explore the possibilities of interaction between polyribonucleotide and polydeoxyribonucleotide strands. Several questions had to be answered in this connection: (*a*) can complex formation occur; (*b*) if so, is the principle of base-pair complementarity involved, and is the resultant complex of the helical type; (*c*) then, how many strands can occur in such "hybrid" helices, and do these helices have characteristic properties which might make them distinguishable and isolable from their precursors?

Rich [76] has provided answers to some of these questions through a study of the complex formed on mixing poly A with short chains of the chemically synthesized polydeoxynucleotide containing thymine, poly *d*T [77]. Employing the mixing curve technique, he has shown that,

depending on the solvent conditions, it is possible to induce formation of a complex containing one strand of poly A and either one or two strands of poly *d*T, analogous to the situation with poly A and poly U, where both strands are of the ribose type. Further, the melting profile of the complex, even though its strands have different sugars in their backbones, is typical for a completely helical macromolecule.

The formation and properties of a different hybrid made from homopolymer analogues of DNA and RNA has been investigated by Schildkraut, *et al.* [78], using techniques different from those employed by Rich which have subsequently proved of value in the isolation of DNA-RNA hybrid complexes from natural sources. In this case a hybrid helix was formed, containing one strand of poly C and one strand of poly *d*G, by heating and slowly cooling a mixture of poly C and poly (*d*G + *d*C), (the latter DNA-like helix having been synthesized enzymatically) [79]. Analysis of the starting materials and product by density-gradient ultracentrifugation, a physical method which allows separation of macromolecules on the basis of their buoyant density, showed that the hybrid product formed has a buoyant density intermediate between that of poly C and poly (*d*G + *d*C). The completely helical nature of the hybrid complex was made evident by its sharp melting profile, which also showed that this complex is more stable than poly (*d*G + *d*C). It was also observed that, in contrast to its precursors, the hybrid complex is resistant to the hydrolytic action of the enzymes ribonuclease and deoxyribonuclease.

These two sets of observations clearly demonstrate the structural compatibility of polynucleotide chains with somewhat different backbones in a helix maintained by the usual complementary base pairs. With recognition of the unique and distinguishing features of such a hybrid polynucleotide helix, the search for the natural means of information transfer and specific RNA synthesis has been markedly stimulated.

Another aspect of the process of information transfer concerns the chemical steps leading to the synthesis of proteins [13]. Important advances have been made in unravelling these reactions, and particularly the roles played by RNA.

The non-particulate fraction of all cells contains relatively low molecular weight RNA, called *transfer* RNA, which functions to transfer activated amino acids from enzymes that catalyze such activation to ribosomes. The latter are cytoplasmic particles, rich in RNA of much higher molecular weight than transfer RNA, and appear to be the sites of specific protein synthesis. Transfer RNA appears to consist of some twenty different types of molecules, each specific for a different amino acid. In contrast to this specificity for the amino acids, transfer RNA has only slight species specificity. These findings suggest that the function of transfer RNA is to *recognize* amino acids, but probably not to

direct the amino acid sequences of proteins. DNA, also, does not appear to have this directing function. Many studies have shown, instead, that a small fraction of RNA present in the ribosomes is required for amino acid incorporation into protein. This RNA, aptly termed *messenger* RNA [80], has a short survival time in the ribosome, and is not to be confused with the bulk of the ribosomal RNA, which is metabolically relatively inert, and about whose specific function information is totally lacking.

The current hypothesis is, then, that transfer RNA recognizes amino acids, forms complexes with them, and transfers them to specific sites in the ribosome. The act of recognition must depend on secondary-force interactions involving the transfer RNA molecule and the activating enzyme on the surface of which the activated amino acid is bound. The amino acid, now linked to the specific transfer RNA, is transported into the ribosome, where it must be identified at the appropriate template site. It would seem that this identification process does not involve the amino acid directly, but rather depends upon secondary-force interactions between some segment of the transfer RNA molecule and a complementary region of a messenger RNA molecule. This hypothetical outline, which attaches no particular function to the ribosomal RNA, is undoubtedly an oversimplification, but it serves to emphasize that there must be molecular mechanisms of recognition and identification in this process. With respect to the mechanism of identification, it is of interest to recall the experiments described in the previous section which showed that stable interactions can occur between relatively short complementary regions of two polynucleotide chains.

A paramount goal in elucidating the mechanism of protein synthesis is to determine the particular nucleotide sequences in messenger RNA which direct the incorporation of particular amino acids into protein; in other words, the translation of the nucleotide language into the amino acid language. A significant forward step in solving this problem has been made in recent months, and a brief consideration of these epochal findings will serve to illustrate, most forcefully, the theme of this essay concerning the use of synthetic polynucleotides as model systems for studying a variety of problems in molecular biology.

The essence of the recent findings is that a variety of homo- and copolynucleotides can replace messenger RNA in cell-free enzyme systems containing transfer RNA and ribosomes while still catalyzing the incorporation of amino acids into polypeptides (polyamino acid). It was first shown by Nirenberg and Matthaei [81] that when poly U is used as the "messenger," polyphenylalanine is synthesized. This has been interpreted to mean that some multiple of uridylic acid residues code for the amino acid, phenylalanine. This sequence could be UU, UUU, UUUU, or some slightly larger U-containing one. In fact, theoretical arguments

have suggested that the coding ratio, that is the number of nucleotides in a sequence which codes for one amino acid, should be three in any universal code [82]; very recently, in a brilliantly conceived genetic experiment, Crick and collaborators [83] have obtained considerable support for this hypothesis.

Using the assumption that the coding ratio is three for each amino acid, Ochoa and collaborators [84,85] have used a statistical argument to correlate the incorporation of specific amino acids into peptides, synthesized by the enzyme system, with the nucleotide composition of the synthetic polynucleotide "messenger." In this way, they have been able tentatively to assign the nucleotide composition, but not the sequence, of the triplets that code 20 amino acids which occur in proteins.

Their approach has been to synthesize, with polynucleotide phosphorylase, a variety of copolymers containing different base compositions, and to determine which amino acids are incorporated into peptides in the presence of each of these random copolymers. The probability of occurence of different triplets in each of the copolymers has been estimated from base compositions. The same triplet will occur in several copolymers, but with different frequency in each one. It has been possible to relate these frequencies to the relative incorporation of different amino acids with the different copolymers, and thereby deduce the triplet compositions. With this encouraging beginning, it should not be too long before these data are put on a firmer basis, and the exact sequence of nucleotides in each triplet is determined.

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THERMAL SOARING OF BIRDS

By CLARENCE D. CONE, JR.

THE PHENOMENON of natural soaring flight has been the object of theorizing for centuries. The early theories, and even some of the more recent ones, were primarily exercises in metaphysics. With the development of fluid dynamic theory, however, came a somewhat clearer idea of the principles involved. As early as 1883 Lord Rayleigh stated the gross physical conditions necessary for soaring flight. Unfortunately, these statements were far too general to be of any aid in understanding the detailed mechanics of natural soaring.

The presently accepted theory of thermal soaring might be briefly summarized thus: The soaring bird enters a continuous column of relatively warm air which is rising from the ground faster than the bird is sinking (relative to the air) and hence the bird is able to ascend therein to appreciable altitudes. In a restricted sense, this simple explanation is plausible. However, like Rayleigh's soaring theorems, it is but a highly simplified description of what in reality is a most complex phenomenon. The "continuous current" theory as it stands is incapable of explaining many important facts observed in natural soaring flight and actually leads to paradoxical results.

Having been interested for some time in the technical aspects of natural soaring (especially from the viewpoint of gaining knowledge for low-energy flight), it appeared to me that a critical analysis of the subject, interpreting observed facts in the light of fluid dynamic theory, might allow the formulation of a somewhat more accurate and refined theory of the phenomenon. Since general evidence shows (see Appendix II) that thermal soaring is the method most commonly used by soaring birds, I began some time ago a specific study with view to constructing as detailed a theory of thermal soaring as the available experimental and observational data would allow. It is the purpose of this paper to present briefly the more important results of this study which might be of interest to ornithologists.

My personal opportunities for observing soaring birds in flight have been excellent. Most of my observations have been made over a period of years near Savannah, Georgia, although some studies were made in Florida, South Carolina, and Virginia. At Savannah, the observations were primarily on the flight of the Black Vulture (*Coragyps atratus*) and the Turkey Vulture (*Cathartes aura*). Both of these birds occur there in great abundance throughout the year. I have also had occasion to observe the soaring of many other birds such as herons, hawks, and pelicans, and also the incomparable soaring of the Water Turkey (*Anhinga anhinga*).

In addition to my own observations, I have had recourse to E. H.

Hankin's "Animal Flight," certainly one of the most extensive observational records ever compiled on natural soaring. This book is primarily an account of three years of continuous observations on the soaring of Indian vultures as made by Hankin near Agra, India. Due to the enormous abundance and variety of vultures, the observations were made under the most favorable conditions possible. One familiar with soaring must marvel at the detail with which Hankin describes the flight of the Indian birds. Unfortunately, Hankin never attempted the development of a theory of natural soaring despite his vast accumulation of factual data. He was firmly convinced, however, that the continuous column theory was not the answer to thermal soaring.

As a prelude to the discussion of the results of the study, it might be well to emphasize here the obvious but often overlooked fact that a bird in flight is subject to the same laws of aerodynamics as the inanimate airplane. Accordingly, the bird may be analyzed as a specific type of flying machine, quite apart from its biological functions. From a technical standpoint, the only difference between the airplane and the bird is that the latter is considerably more complex. This complexity arises primarily from the fact that the bird's geometry (i.e., the wing shape, area, twist, etc.) is almost infinitely variable, not only for different modes of flight such as soaring and flapping, but for different phases of the same mode such as the upstroke and downstroke in flapping flight. For the soaring mode of flight, however, the study is greatly simplified since the wing geometry is practically constant for a given phase.

On the other hand, the highly specialized structure and mode of flight of the soaring bird are the result of its requirements as a living creature. Over centuries of evolution it has become most perfectly developed for the exploitation of the natural offerings of its environment. As will later be evident, the life habits and requirements of the soaring bird play a critical role in explaining the evolvement of the bird's specific geometry.

A full understanding of the theoretical aspects to be discussed unfortunately necessitates a rather detailed knowledge of aerodynamic and hydrodynamic theory. However, in the hope of giving at least some brief explanation of the most basic aerodynamic principles, an appendix (Appendix I) has been included, defining the simpler aerodynamic concepts. For a more complete discussion, reference should be made to aerodynamics texts. Also, for completeness, an appendix (Appendix II) giving a classification of the types of natural soaring flight is included.

Observations of Thermal Soaring

The value of any theory lies in its ability to explain observed facts, and even more important, to predict the results of future experiments. In the case of our present subject, where the number and complexity of variables are immense, and where carefully controlled experimentation

is impossible, the accuracy of the theory can only be judged by its ability to explain and correlate general field observations. Before beginning the theoretical development, therefore, it is important to describe the characteristic features of thermal soaring and to list some of the more important facts which the theory must endeavor to explain.

Soaring of Vultures: The typical thermal flight of the heavier vultures consists of two phases, circling and gliding. In the circling phase, the bird executes (relative to the stationary ground observer) a series of circles or loops, usually with a continuous gain of altitude. During circling the wings are fully outstretched and the primary feathers are spread out like a fan at the wingtip. The bird is appreciably banked toward the inside of the circle and the flight is usually slow and very steady. If there is a general wind blowing, each successive loop is displaced leeward

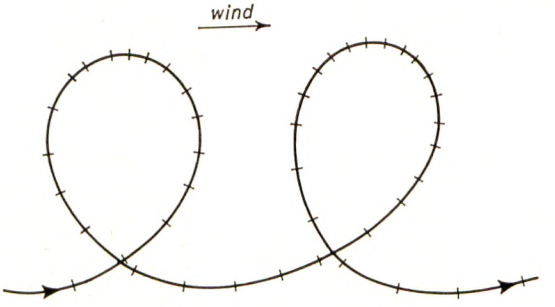


FIG. 1. The flight path of a circling soaring bird. Tick marks refer to equal time intervals (1 sec).

by an amount proportional to the wind strength so that from below the path is as shown in Figure 1. (Hankin obtained such path lines by marking off on a mirror reflecting the sky, the position of the bird at equally spaced time intervals, and then joining the points with a smooth curve.) The bird continues circling until sufficient altitude has been gained, and then with a double-dip of its wings (see Hankin, 1913, for definition of the double-dip and other soaring terminology) it flex-glides away along a straight flight path with considerable increase in speed and usually with an observable loss of altitude. The length of the glide is quite variable. The bird may travel only a short distance and commence circling again, or he may glide completely out of sight. In this manner, by circling and gliding, the bird is able to sustain himself without internal power expenditure and can efficiently cover enormous areas of ground in his continuous search for food.

Other variations of thermal soaring exist, of course, but these are primarily special cases of the circling-gliding phase. For example, in the extreme case, the circling may degenerate into an erratic, continuous

glide through thermal currents provided the thermals are closely spaced and sufficiently numerous (lift-gliding).

General Observations: Under the extremely favorable conditions existing in India, Hankin was able to formulate from his observations on many different vulture types some general principles of thermal soaring (or "sun-soarability" as he called it). Some of the more important of these are listed here. They will later be explained in the light of the theory. The general observations listed apply equally well to soaring flight as observed in this country.

(1) Thermal soaring begins at a definite time each day, varying from day to day, but always depending upon the time of formation of "heat-eddies" on ground surfaces.

(2) The morning hour at which soaring commences varies with the month, becoming progressively earlier from January (9:30 A.M. in Agra) through June (7:00 A.M. in Agra), and then gradually commencing later and later from July through December. (This observation clearly correlates directly with the intensity variation of sunlight during the year.)

(3) The soaring birds always take off precisely in order of their wing loading, with the heaviest bird, the Indian Black Vulture (*Otogyps calvus*, $W/S = 1.23$ lb./ft.^{2}) taking off nearly 1.5 hours after the circling of the lightest bird, the Cheel (*Milvus govinda*, $W/S = 0.55$ lb./ft.^{2}) had commenced. Here W/S is the wing loading of the bird, defined as the ratio of total weight, W, to wing area, S.

(4) The circling of Cheels commences over cities somewhat earlier than it does over open terrain around the cities.

(5) The greater the wing loading of a bird, the greater will be the altitude at which it commonly circles.

(6) Birds which do not regularly soar, such as the Adjutant (*Leptoptilus dubius*, $W/S = 1.54$ lb./ft.^{2}) may be seen soaring only at great altitudes and in the very hottest part of the day, during the summer months only. The bird is incapable of soaring flight during the remainder of the year.

(7) Cranes ($W/S = 1.66$ lb./ft.^{2}) must flap to 500 feet or higher before they can begin sustained circling flight.

(8) Of all forms of soaring (see Appendix II), thermal soaring is by far the most universally used by vultures.

(9) In the Himalaya Mountains, thermal soaring of exactly the same nature as that practiced over the flat plains of Agra is the common type of soaring.

(10) The commencement of circling of a flock of vultures which was initially at rest on the ground usually coincides with a sudden gust of wind on the ground.

(11) The steady wind provides no energy for soaring flight.

(12) The highest degree of soarability occurs when the wind is calm.

(13) The maximum altitude (above the ground) of circling flight as observed for any bird is 6500 feet.

(14) The air is always more soarable at a height than near the ground.

(15) General soarability is a direct function of the presence and intensity of sunshine.

(16) In circling flight, the diameter of the circle increases with the wing loading of the bird. For example, the Cheel makes circles as small as 40 feet in diameter, while the more heavily loaded vultures make circles over 165 feet in diameter.

(17) The spread of the loops in circling flight is proportional to the wind speed, and is in the direction of the wind.

It may be of interest to list here some of the more common soarers observed by Hankin in India, as an example of the great variety of soaring birds which he had the opportunity to study. The numbers listed with each name are the bird's wing span and wing loading in feet, and pounds per square foot, respectively: Cheel (*Milvus govinda*, 4, 0.55); White Scavenger Vulture (*Neophron gingianus*, 5, 0.87); White Backed Vulture (*Pseudogyps bengalensis*, 7, 1.13); Black Vulture (*Otogyps calvus*, 6.5, 1.23); Adjutant (*Leptoptilus dubius*, 10, 1.54), a species of stork; Sarus Crane (*Grus antigone*, ?, 1.66). The details of the thermal soaring of these Indian birds, as described by Hankin, are identical in every respect with those of our own common vultures such as the Black and Turkey Vulture, as observed over the broad flat coastal plains of our southern states. The descriptions are also fully applicable to the soaring of condors.

Theory of Thermal Soaring

Basic Concepts: Before proceeding with technical details, we shall briefly outline the theory resulting from the study. First, we postulate that, instead of a continuous column of air rising from the ground, the thermal as used by soaring birds is actually a closed shell or bubble of air which is rising due to the buoyancy of some of the interior air. The air within the rising thermal shell is not stagnant as in a balloon, but is in circulation about a buoyant vortex ring core. This circulatory flow causes the air to rise upward in the center of the shell and downward on the outside, relative to the core of the shell. A typical cross section of a thermal shell is shown in Figure 2. A soaring bird enters the thermal and begins circling within the upcurrent region. If the upcurrent is stronger than the bird's sinking velocity, the bird will be carried up to a level within the bubble where these two opposing velocities are exactly equal. Here the bird will be in complete equilibrium with the bubble and will be carried along with it as it rises and translates with the wind. We might describe the situation as analogous to an ascending balloon within which the bird is circling, while the air within the balloon circulates

around a closed course. The bird is able to maintain its position within the thermal shell only by virtue of the closed circulation flow that exists within the shell. Otherwise, even though the thermal shell is ascending, the bird could only sink relative to the thermal's core and would ultimately pass out of the shell. Of course, some net altitude gain might be effected even in a noncirculating shell, but sustained circling would not be possible. Thus the bird is actually riding in a continuous column of rising air, only the "column" is closed in that the same air is merely circulated around a closed course. Therefore, once a thermal shell is formed, it is completely free of any connection with the ground. As will

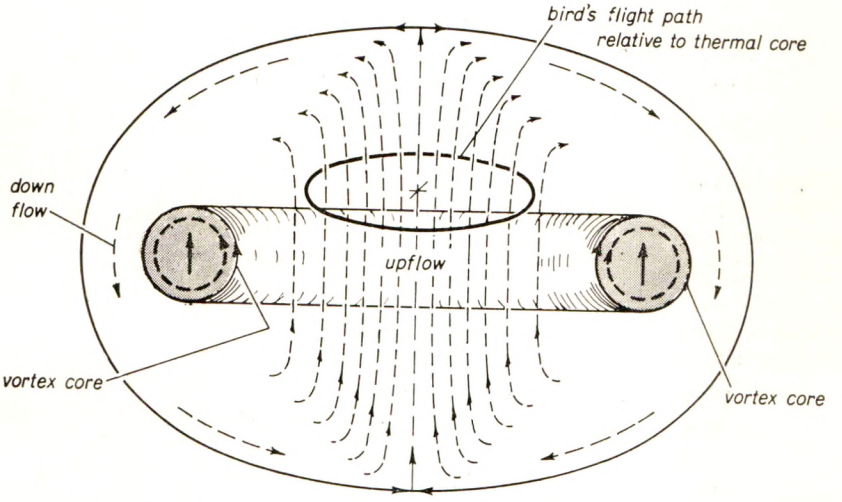


FIG. 2. A cross-sectional view of the air flow within a thermal shell.

be discussed later, this concept of a closed column explains the observed facts in a most satisfactory manner.

In order to remain in the thermal shell the bird must continually circle relative to the core of the shell. In order to execute a turn of sufficiently small radius so as to remain in the upcurrent region, the bird must be capable of very efficient aerodynamic operation. In general, as the radius of turn is decreased, the bird's sinking velocity increases rapidly. Hence it is possible that, if the bird turns in a small enough circle so as to remain in the upcurrent, its sinking velocity has increased so much that it is greater than the maximum upward velocity existing in the thermal. We are thus led to a complex relation between the aerodynamic characteristics of the bird and the size and strength of the thermal. This relation, however, is amenable to quantitive analysis. The results of this analysis yield a most valuable insight into the reasons for the soaring bird's unequaled flight abilities. We shall see that the entire structure and geome-

try of a soaring bird is specifically designed to make maximum use of the particular flow existing in the thermal shell.

Formation and Structure of a Thermal Shell: In order to understand the mechanics of thermal soaring we must first gain an accurate knowledge of the meteorological conditions which are available for the bird's exploitation. This knowledge is critical, for we can scarcely hope to explain the bird's actions unless we know the details of the air currents in which he operates. Hence, we shall now develop the details of such air-flows by tracing the steps and processes involved in the formation of a typical thermal.

As the sun shines upon the ground, the radiant energy is converted into heat and the ground temperature rises. Moisture present in the soil and in vegetation begins to evaporate. The air layer next to the ground becomes warmed by the local convections set up and this layer becomes

TABLE I

Surface Description*	Time and Date	Temperature, °F.
Red brick	1:50 P.M., 3 Sept. 59	112.1
White concrete	1:55 P.M., 3 Sept. 59	118.6
Close-cut grass	2:00 P.M., 3 Sept. 59	84.2
Metal (galvanized)	12:30 P.M., 13 Sept. 59	115.6
Black earth	2:05 P.M., 4 Sept. 59	125.8
White sand	11:55 A.M., 4 Sept. 59	113.7
Asphalt surface†	2:00 P.M., 18 June 60	145.8

* All measurements were made on horizontal surfaces.
† This measurement made at Hampton, Va.; all others at Savannah, Ga.

thicker and warmer as heat and moisture are continually transferred to it from the ground. Were it not for the presence of the air, which carries away the heat generated, the ground surface temperatures would soon approach the boiling point of water. Thus, large quantities of energy in the form of heat are continuously transferred to the air layer near the ground and this is the initial step by which the energy of sunlight becomes transformed to power the soaring flight of birds. Table I lists some typical summer surface temperatures and Figure 3 presents some measurements of the temperature distribution in the lower few feet of a typical warm air layer, as observed by the author. At the time of these measurements some twenty Black Vultures were in circling flight directly overhead. These measurements were made in September near Savannah, Georgia.

As the surface air becomes hotter and moisture-laden, it becomes less dense than the cooler air above it and tends to rise because of its buoyancy. In general, this warm air layer is moving very slowly along the ground in the direction of the general wind, as it grows in thickness. This layer remains in an unstable equilibrium until some disturbance such as a local overheating or a large obstacle causes a protuberance to

develop in the layer (Fig. 4A). Once the equilibrium has thus been upset, the protuberance rapidly expands into a mushroom-shaped body into which the warm air of the layer quickly flows (Fig. 4B). Since other such protuberances are forming simultaneously at various points of the warm air layer, the warm air supply between successive growing protuberances quickly becomes exhausted and the protuberances are pinched off by the cooler, dry air which settles in to replace the warm. A protuberance thus contains a definite mass or body of warm air (Fig. 4C). The cooler air which has settled in starts to warm and the process begins all over

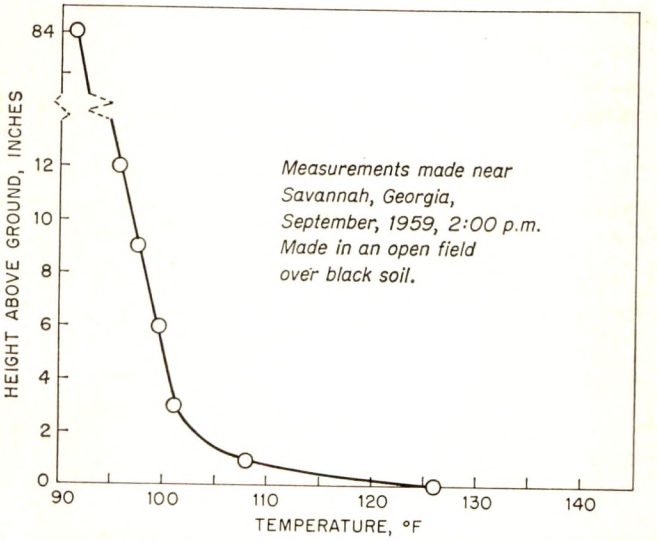


FIG. 3. Experimental temperature distribution in a warm air layer near the ground.

again. Thus, on a clear, hot day protuberances are continuously being generated over the entire land surface, exactly like vapor bubbles rising from the bottom of a pot of gently boiling water.

The pinched-off protuberance or thermal shell, as we shall call it, is acted upon by a buoyancy force and tends to accelerate upwards like a balloon. The thermal shell, however, is quite different from a balloon in that it has no material walls to support it. Due to its motion through the cooler outside air, vorticity is continually created on its surface and this outer wall of rotational fluid rolls up into a core as it is produced (Fig. 4D, E). After a brief development period, the warm air becomes essentially confined to a central core, or vortex ring, which is similar to a common smoke ring. The size of this thermal ring may vary from a few feet to perhaps several hundred feet in major diameter, depending upon the volume of warm air which was involved in the initial protuberance formation.

We have discussed the thermal buoyancy as being caused by the "warmness" of the air. Actually, moisture content can be of as much importance as, or even more than, temperature differences in creating the buoyancy forces, since water vapor is considerably less dense than dry air. On forested terrain, high ground temperatures rarely occur since

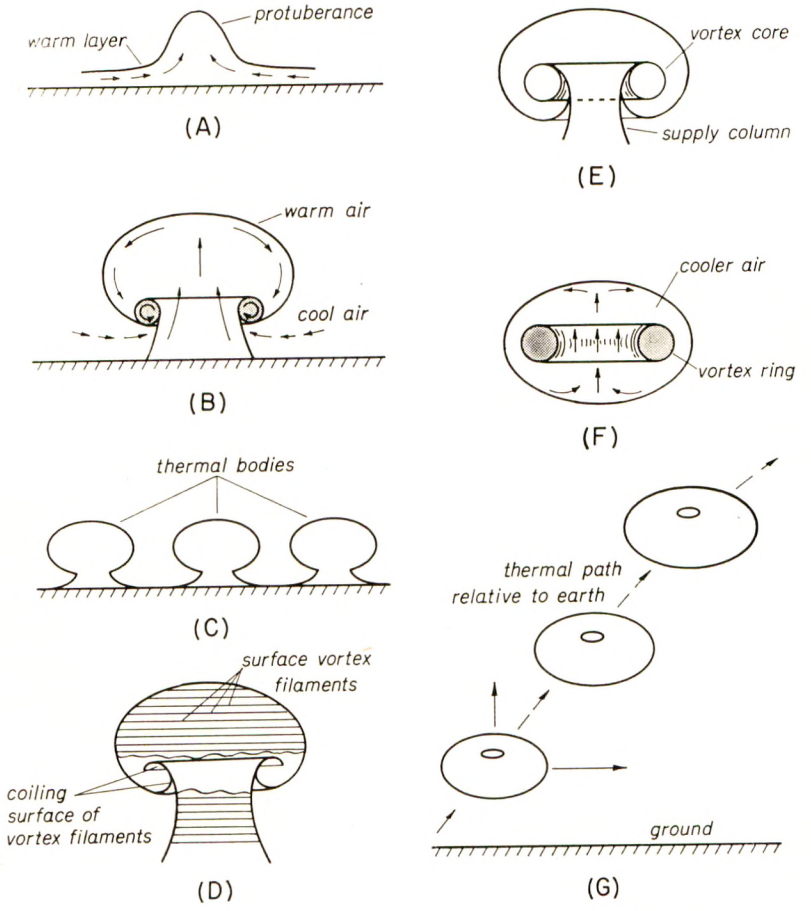


FIG. 4. Various stages in the formation of a thermal shell.

the transpiration of the vegetation maintains a low surface temperature, so water vapor no doubt here plays an important role in thermal production. It should also be noted that it is the temperature and/or moisture *difference* which is important in thermal production, not the absolute values of either.

The mathematical and fluid dynamic considerations necessary to describe the flow associated with a buoyant vortex ring are exceedingly

complex and of course will not be treated here. However, a brief description of the flow pattern about a buoyant vortex ring is necessary. Although we have assumed a simple buoyant vortex ring system as the idealized mathematical model of the thermal shell, it should be pointed out that this is only an approximation, since the warm air of the core becomes more and more diffused as the shell rises. However, the general nature of the flow is well represented by this model. Figure 4F is a cross section of a thermal shell after it has become fully developed. The core of warm air is revolving about its axis and this core is in turn surrounded by cooler air which is circulating around the warm core as shown. The flow is such that the air initially comprising the shell always remains within it although additional air is continuously being added to the shell as it ascends. The entire closed system of circulating air ascends vertically with a speed less than that of the air circulating through the center of the ring. This thermal shell also floats horizontally with the general wind, so that its path relative to the earth is as shown in Figure 4G. For the details of vortex rings and vortex-ring systems the interested reader is referred to such works on theoretical and experimental hydrodynamics as Lamb (1932), Milne-Thomson (1955), Prandtl-Tietjens (1934), and Scorer (1958).

The thermal shell is a self-sustained source of vertical velocities and once it has formed it has no further connection with the ground. It needs no constant supply of fresh warm air to power it, as would a continuous column. As the warm core rises, the work done by its force of buoyancy is used to energize increasing circulational velocities within the thermal shell which accompany the production of vorticity. This production of vorticity is such that the ring expands in major diameter as it rises, so that the thermal shell continuously increases in size with altitude. This increase in diameter causes the ascent rate of the shell to decrease, so the rate of rise decreases with altitude. The increase in thermal diameter causes large amounts of cold air to be added to the shell as it grows, so that the concentrated heat energy of the original protuberance air is transformed to provide a relatively small velocity to an enormous body of air. This existence of circulation velocities within the thermal shell plays a critical role in the present theory, as will soon be shown. It should be mentioned also that, in reality, the air outside the thermal shell for some distance above and below it is also in upward vertical motion. This motion is associated with the so-called "drift mass" of an immersed body, as predicted by hydrodynamic theory. These relatively strong updrafts, which may extend for considerable distances into the clear air above large clouds, are quite familiar to sailplane pilots.

In this remarkable manner the energy of sunlight is converted into a sustained source of vertical velocities which can carry a soaring bird large horizontal distances and to great altitudes. This constitutes the

basic process by which the energy of sunlight is made available to the soaring bird.

The formation and structure of thermal shells as just described is not at all hypothetical. The description is based on the predictions of vortex theory and has been fully verified by experiments in both air and water. My own theoretical treatment of the formation of thermal shells has been fully verified by small scale experiments. Experiments by Scorer also support my theory as to the nature of thermal structure. In addition to the proofs given there, several additional ones may be advanced that a thermal as used by soaring birds consists of a finite body of air rather than a continuous column.

First, a continuous column requires, for flow continuity, a constant inflow velocity over the ground toward the base of the column. Since the warming of the air takes place at the ground, the air supply to the column would result in a continuous ground wind. However, as Hankin also observed, perfect soarability exists without any sustained ground wind, and usually the best soarability is coincident with the complete absence of a sustained wind. Clearly, calm air is most conducive to the production of large and intense thermals which in turn allow the highest degree of soarability. A second proof is to be found in the observation that the starting of a group of soaring birds is often coincident with a sudden gust of wind. This gust is clearly the ground air flow associated with the formation of a thermal shell. (In fact, soarers probably use this gust phenomenon to determine when the air has become soarable in the early morning and as a measure of soarability at other times during the day. Condors and other vultures can routinely be observed testing the airflow direction and strength with extended wings before taking flight in the morning.) Once the thermal body has formed, the ground airflow ceases. There is experienced on the ground only a temporary gust or breeze. The birds, however, continue to circle, rising to great heights with the complete absence of any further ground wind as would be necessary to support a continuous column. Hankin observed this phenomenon with Cheels in India and I have repeatedly observed its occurrence with soaring gulls. A third proof is the fact that the individuals of a group of soaring birds always circle at about the same altitude. They are never spread out over any appreciable altitude range as could easily be the case if a continuous column existed. Also, the observation has been made that birds at a moderate altitude are in sustained circling flight while birds directly below them, near the ground, must flap-circle until they have gained considerable altitude. Clearly, there are no vertical currents near the ground and the birds must rise by flapping until they can reach the thermal shell.

Many other proofs can be advanced such as the formation of the *isolated* cumulus cloud puffs which result from the condensation of

moisture in a thermal body, and the failure of thistledown to rise beneath a soaring vulture (Hankin: 60). It may be concluded that we possess ample theoretical and experimental evidence that thermals in general, as used by soarers, are closed bodies of circulating air, and not continuous columns emanating from hot spots on the ground. Continuous columns of rising air are associated primarily with the formation of enormous air movements such as thunderstorms, which are of little use to soarers. With this knowledge of thermal structure, we can now consider the manner in which the soaring bird makes use of these properties.

Before closing this section, an interesting observation made by Hankin is cited. Cheels are observed regularly to commence flight over the city before beginning over the open country. This is of course because the vertical surfaces of the city buildings are almost as intensely heated just after sunrise as will the horizontal ground be heated at midday. Thus, small but usable thermal shells are available over the cities long before the open ground has even begun to warm.

The Mechanics of Circling Flight: The analysis of the circling phase may be simplified by considering the problem in two parts. First we shall consider the motion of the bird relative to the air within the thermal shell, and then consider the motion of the thermal shell relative to the earth. The motion of the bird relative to the earth is then obtained by vectorially superposing these two simpler motions.

For the moment, considering the thermal core to have no translational velocity (either vertical or horizontal) relative to the earth, the air within the shell will be rising through the center with a relatively high velocity and will be moving down on the outside with a relatively small velocity as follows from continuity requirements. Of course, the thermal shell cannot actually remain stationary relative to the earth unless it is situated within a sufficiently strong down wind such as the flow down a steep mountain side. However, in order to study the air motions within the bubble, it is convenient to suppose that the bubble is stationary. Let us now suppose that a soaring bird enters the thermal body at some point and begins to circle with a radius of turn sufficiently small that he remains in the upcurrent region. The question as to how the bird is able to find and remain in the thermal is not difficult to answer. It undoubtedly finds the thermal shell quite by accident, neglecting the highly doubtful possibility that the bird can "see" hot air masses, i.e., it just happens to pass into a thermal in the course of a glide or flapping flight. In the case where the bird commences circling from rest, it no doubt immediately senses the formation of a thermal by means of the associated ground breeze, as already discussed. The bird probably establishes its flight path within the thermal by sensing the air temperature gradients associated with the buoyancy structure of the core and body and/or it monitors the variation of the resultant aerodynamic force direction compared

to the constant weight direction, i.e., the acceleration experienced as a result of the changing velocity pattern the bird encounters as he "cuts across" the thermal body. As the bird circles in the thermal, he continually sinks relative to the air. However, since the air is moving upward through the center of the core and for some distance above and below the plane of the core (Fig. 2), and since the upward velocity within this region becomes larger as the vertical axis is approached, there will, in general, be some circle radius for which the sinking velocity of the bird is equal to the vertical velocity of the air, and here the bird will remain in vertical equilibrium *with respect to the core* as he circles. That is, he will merely circle in a level plane with respect to the core of the thermal. The thermal upcurrent furnishes the thrust which drives the bird around the circle with constant velocity, since a component of the lift acts in a plane parallel to the plane of the core.

If the bird enters the bottom of the thermal at too low a level, he must flap to rise to the point where the vertical currents are sufficient to support him. Upon reaching this point, he will be in equilibrium; but since the vertical velocities in the thermal's center increase toward the plane of the core, the equilibrium is unstable. The bird will, upon exceeding this lower limit, continue to rise up through the center of the ring as he circles until he reaches an equilibrium plane above the core where the vertical air velocity again balances his sinking velocity. In this top half of the thermal his equilibrium is stable. Should he enter the thermal body at too high a level, he will sink until he attains a stable balance. In this way the bird automatically rises or sinks to an equilibrium position upon entering the thermal body, and from then on he merely circles in a level plane with respect to the thermal shell. Since all forces acting on the bird, except his weight, are of aerodynamic origin, once the bird has established his orbit within the thermal, his motion relative to it will not be changed in the slightest by the fact that the entire thermal body actually has a steady translational velocity relative to the earth, instead of being stationary as was previously assumed.

The motion of the bird relative to the earth is now easily obtained. The bird is circling in a level plane relative to the thermal body. However, the thermal itself is rising and also drifting with the general air movement, exactly like a balloon. Hence the bird's motion relative to the earth will consist of uniform motion around the circumference of a circle which is parallel to the surface of the earth as the circle is translated relative to the earth. Since the bird has no vertical motion relative to the thermal, its gain of altitude will be exactly the same as the rate of rise of the thermal core. If the bird enters the body of the thermal at a low level, it will initially gain altitude somewhat faster than the thermal is rising; but as soon as it reaches its upper equilibrium level, it will begin to rise at the same rate as the thermal.

Under such a circling and translational motion the bird will *appear* to undergo a large gain in altitude on the windward side of the circle, and a much smaller gain on the leeward side. Actually, the bird will gain equal altitude on each leg of the turn. Since his velocity relative to the earth is decreased by an amount equal to the wind velocity on the windward leg, the altitude gained per horizontal distance traveled will be larger than that on the leeward; and this *rate* is what we usually interpret as the altitude gain, rather than the actual distance gained. It may even

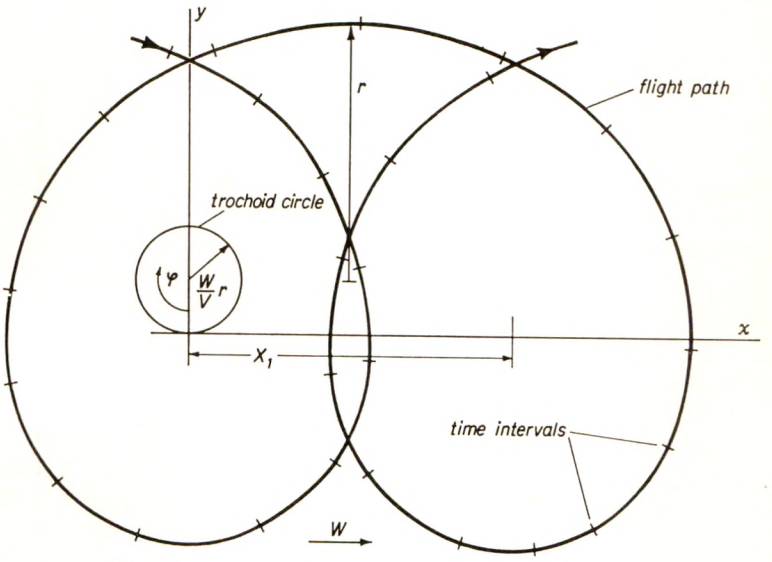


FIG. 5. The trochoidal flight path of a circling Cheel.

appear that the bird loses altitude on the leeward leg if the wind velocity is large. However, if the bird is truly centered in the thermal, this appearance is purely illusory for it is simply proved that equal gains of altitude must occur on each side of the circle.

Mathematically, this type of motion produces a flight path whose projection is a trochoid. Geometrically, a trochoid is the locus of a point on an extended radius of a circle which rolls along the x-axis with constant velocity (Fig. 5). By a simple analysis of the trochoid geometry we obtain the equations of the flight path in terms of the flight variables

$$\frac{x}{r} = \frac{W}{V}\varphi - \sin\varphi \tag{1}$$

$$\frac{y}{r} = \frac{W}{V} - \cos\varphi \tag{2}$$

where x and y are the flight path coordinates as shown, W is the horizontal wind speed relative to the earth, V is the approximate flight velocity

of the bird relative to the air, r is the radius of the circle the bird is making in the thermal, and φ is the angular displacement of the trochoid circle from the point $x = 0$.

These equations allow us to show that the actual observed flight path of a circling bird is a trochoid, and hence is the path predicted by the theory. They also allow us to derive a number of important facts from a few simple measurements. In Figure 5, I have sketched two successive loops from a flight path of a Cheel as recorded by Hankin with the mirror technique. It is clear that the maximum width of the trochoidal flight path is equal to $2r$. The radius of the trochoid circle (small circle) is $(W/V) \cdot r$. Using any convenient unit of measurement, we may obtain the value (x_1/r) for $\varphi = 2\pi$ by direct measurement of the figure. (The x_1 value for one revolution of the trochoid circle is clearly the distance between corresponding points on two successive loops, as shown.) Then the ratio W/V follows directly from equation (1). Knowing now W/V and r, we can plot the flight path using the parametric equations (1) and (2). The path so obtained coincides with the experimental path, thus proving that the bird follows the predicted trochoidal path.

If the flight path has been marked off in known time increments, as in Figure 5, and if the wing span of the bird in the scale of the tracing is known or can be estimated, it is possible to solve for the actual full scale values of r, V, W, and W/V. First we convert the known span (or any other characteristic length) of the bird into the equivalent units of measurement of the tracing. Then we can determine the actual distances represented by the tracing by a simple measurement and multiplication by the scale factor. By counting the total time interval Δt necessary for the trochoid circle to proceed a distance x_1, we have the wind speed $W = (x_1/\Delta t)$, from which the flight speed V may be computed, $V = (V/W) \cdot W$. (In this analysis we have of course assumed that the altitude gained in two successive loops is negligible compared to the dimensions of the loops. Actually, the second loop is slightly above the "plane" of the first loop, if the bird is gaining altitude. Clearly then, in a long series of loops with altitude gain the scale factor of the tracing will decrease. This fact can serve as the basis for calculating the altitude gain.)

As an example of the use of this theory, we may derive the following information from Figure 5, knowing that the path is that of a Cheel with wing span of 4 feet. The bird is making circles of radius 71.4 feet in the thermal, the wind velocity is 4.2 miles per hour, and the air speed of the bird is 20.9 miles per hour. The lift coefficient is approximately 0.53, a rather low value. These calculated values are interesting in that the low wind speed is favorable for the formation of large thermal shells and indeed the very large turn radius of 71.4 feet indicates that the thermal is quite a bit larger than that normally used by the Cheel whose average turn radius is about 35 to 40 feet.

It should be noted here that the motion predicted by the continuous column theory can coincide with the trochoidal path only under the most restricted conditions. In the column theory, to produce a *sustained* trochoidal flight the sinking velocity of the bird must be such that the difference between it and the vertical velocity component of the air within the column is precisely the velocity which, combined with the wind velocity, gives a resultant velocity parallel to the axis of the column—a condition which is clearly impossible unless the bird has a zero sinking velocity. On the other hand, the thermal body theory imposes no such restrictions and, as we have seen, results in an automatic flight regulation requiring only the most simple control by the bird.

The ability of the soaring bird to exploit the entire range of available thermal sizes and strengths is clearly dependent on its ability to execute efficiently turns of very small radius, and a quantitative consideration of this point yields answers to many of the most important questions of thermal soaring. It is obvious that, if a bird can practice sustained circling in thermals of very small size and strength, it will be able to perform even more efficiently in larger and stronger thermals. Hence the smaller the radius of turn a bird can efficiently fly, the greater will be the certainty and duration with which it can soar since the number of usable thermals is greatly increased. It can be proved by a simple but lengthy analysis that the sinking velocity of the circling bird must in general increase as the turn radius becomes smaller. The results of the analysis may be stated in two simple equations. These are the relations for the sinking velocity and the radius of turn of the bird, given as a function of the bird's aerodynamic and structural properties and his angle of bank in the circle. They are:

$$\dot{z} = \left(\frac{W}{S}\frac{2}{\rho[C_D^2 + (C_L\cos\beta)^2]^{1/2}}\right)^{1/2}\frac{C_D}{[C_D^2 + (C_L\cos\beta)^2]^{1/2}}$$

$$r = \frac{W}{S}\frac{2}{\rho g}\frac{C_L\cos^2\beta}{\sin\beta[C_D^2 + (C_L\cos\beta)^2]}$$

where

$\dot{z}$ = sinking velocity
r = radius of the circle
$\frac{W}{S}$ = wing loading (weight per unit of wing area)
C_L = lift coefficient (a measure of the lift force)
C_D = drag coefficient (a measure of the drag force)
g = acceleration due to gravity
β = angle of bank in the circle
ρ = air density

(See Appendix I)

An analysis of these equations leads to the following very important results:

(1) Since the vertical velocities within the thermal increase as the vertical axis is approached in a given plane above or below the core, to

reach an equilibrium position the bird must adjust his radius of turn to a sufficiently small value such that he can circle where the air current is strong enough to sustain him. In particular, if the thermal is small, he must have a small radius of turn to remain within the confines of the thermal body. However, if the bank angle β is increased so as to reduce the radius of turn, the equations show that the sinking velocity will increase. Hence, although the bird may be able to make a turn of very small radius, he may find upon reaching the stronger currents of the thermal that his sinking velocity has so increased that it exceeds the vertical circulation velocity there, and he will descend through the thermal and ultimately pass out of it. That is, he cannot remain in equilibium within the thermal if his radius of turn is too small. It is clear then that a minimum thermal size and/or strength exists for each type of soaring bird below which the bird cannot attain equilibrium anywhere in the thermal.

Due to the velocity pattern existing within the thermal shells, there exists, in general, a maximum and a minimum radius or turn for equilibrium flight within the thermal. In between these limits lies a range of radii for which the bird may fly in equilibrium. This supposes of course that the thermal is of sufficient size and strength to permit sustained circling for the given type bird. For a given bird a thermal may be too small to be within his turn capability, or it may be of insufficient strength anywhere to match his sinking velocity. This is very important, for the thermal size and strength dictate which birds may use it. Conversely, the aerodynamic properties of the bird determine the minimum thermal strength and size which can be used for sustained soaring. For instance, Hankin noticed that Cheels circled long before the heavier birds. As will be pointed out in the next section, wing loading critically affects both the minimum radius of turn and the sinking velocity of a bird, and hence the light Cheel can easily use the small weak thermals of the early morning, while the heavier birds must await the larger, stronger thermals that come with the more intense heating as the morning progresses. Hankin also noted that the heavier birds always circled at a higher altitude than Cheels. The reason for this is also evident in the light of the foregoing discussion. The heavier birds require a larger radius of turn and also stronger currents. Hence they must intercept and use the thermals at higher levels where the thermals have grown to such a sufficient size and intensity that the larger birds can remain in equilibrium within them. To reach such thermals they must often flap to considerable altitudes.

For a thermal of sufficient size and intensity the establishment of equilibrium is practically automatic. The bird doesn't have to adjust continuously its angle of bank until it finds an equilibrium position. So long as it maintains a radius of turn sufficiently small to remain in

the thermal upcurrent, it will automatically rise or sink to an equilibrium position. Should it increase or decrease its radius of turn within the thermal limits, it will merely sink or rise to a new equilibrium position. It is of little matter just where, in the thermal, this equilibrium is established, since, as we have already seen, the bird can only rise at the same rate as the thermal core is rising. Thus the establishment of equilibrium appears to be a relatively simple matter, and the bird may fly any of a range of radii, so long as the maximum and minimum for the given thermal are not exceeded.

Once the equilibrium is reached, the entire circling flight phase also becomes automatic. Provided the thermal is truly axisymmetrical, the bird will automatically maintain its correct angle of bank and will make circle after circle without the necessity of any wing adjustments. Slight tail motions may be needed to maintain trim if the thermal is turbulent or slightly unsymmetrical. Thus we account for the observed regularity and absence of directive movements so characteristic of the circling flight of soaring birds.

(2) The second important result given by the equations is that of the effects of wing loading, W/S. It is obvious that the lower the wing loading the lower will be both the sinking velocity and the radius of turn. Hence, low wing loadings are mandatory for the most efficient thermal soaring. The smaller W/S is, the smaller and weaker can be the thermals that the bird can utilize. This is probably the most important reason why soaring birds can remain aloft much more easily and can soar with so much more certainty than man-made sailplanes. The bird, even with the excess weight of his auxiliary flapping muscles, still has a loading far below that of the best sailplanes. Thus, even though the aerodynamic efficiencies of bird and sailplane are roughly comparable (Raspet, 1950), the lower wing loading of the bird allows it to tap the energies of thermals which the sailplane cannot begin to exploit, even if the pilot could find them. The manner in which land soaring birds attain this low wing loading and still maintain high aerodynamic efficiency is a most interesting and valuable lesson, and will be discussed in detail shortly.

(3) The equations show that in circling flight we want the ratio $C_L^{1.5}/C_D$ to be a maximum. This is equivalent to saying that, for a given lift force, the drag force should be very small. But what is perhaps even more significant, the equations show that for maximum soaring ability as large a value of C_L as possible should be available, which means that $C_L^{1.5}/C_D$ should be a maximum when C_L is a maximum. For normal aircraft wings, C_D increases more rapidly than C_L so that the ratio $C_L^{1.5}/C_D$ reaches a maximum at a rather low value of C_L which is not good for thermal exploitation. As mentioned in Appendix I, the greater part of the drag at high lift values is due to induced drag, and this

drag can be greatly reduced by increasing the aspect ratio of the wing (the ratio of span to chord). However, it is not quite so simple to make practical use of this fact. As the span is increased, the bending moment at the joint of the wing with the body becomes large, and the bone structure necessary to provide sufficient strength increases considerably. The added weight that is necessary for the higher aspect ratio wing increases W/S, so that if we continue to increase the aspect ratio we reach a point where the decrease in drag due to high aspect ratio is completely nullified by the increased drag due to the increase in W/S, and further increases in aspect ratio will actually cause increased sinking velocities. The land-soaring bird has solved this problem in a most ingenious manner by the use of its large primaries or pinions. We shall cover this subject in detail in the next section.

To summarize briefly the results of this section, we have seen how a soaring bird enters a thermal and almost automatically reaches an equilibrium. Circling within the thermal, he is carried upward with the thermal as it drifts with the wind, and we have seen how the predicted motion fits the observed flight patterns. Lastly, we were able by the quantitative analysis of circling flight to develop equations which explain several other important observations. We will now use the theory to explain the function of the large slotted tips so characteristic of land soaring birds and to indicate the reason for the differences in structure in land and sea soarers.

Aerodynamic-structural Characteristics of Soaring Birds: As a wing increases lift by an increase in angle of attack, its drag increases even more rapidly. One part of the drag is due to the friction between the air and the wing; this is relatively small and unaffected by lift. It can be reduced by careful smoothing and streamlining of surfaces. The other part is called the induced drag and for most wings it depends very much on the lift, becoming very large at high lift coefficient values. This induced drag is associated with the variation of the lift force across the span of the wing. The decrease in lift as one proceeds from the maximum at the center of the wing to zero at the tip causes a series of very small vortices to be shed from the entire trailing edge of the wing, each vortex having a strength proportional to the rate at which the lift decreases at the spanwise position where the vortex is shed. This sheet of vortices is continuously generated by the bird as he passes through the air, and the kinetic energy per second put into these vortices essentially equals the power needed by the bird to maintain flight. The smaller this required power, the greater the ease and certainty with which the bird can soar. If now, for a given total lift on the wing, we make the wing span very large and the chord small (while maintaining the area constant), we see that the *rate* at which the lift decreases across the span is decreased. Hence, the vortices leaving the trailing edge become weaker as the wing aspect

ratio is increased. This results in lower induced drag at high lift coefficients, so that aspect ratio can be used to increase the value of $C_L^{1.5}/C_D$ at large values of C_L. (Values of C_L above 1.0 may be considered quite large for normal wings.) This method of reducing drag is used in sailplanes, where the wings are long and narrow. However, as pointed out in the last section, there is a practical limit to the use of aspect ratio as a drag reduction device.

A second method is also available for reducing wing induced drag at high C_L values. This involves the use of endplates, or large discs, which are attached to the tips of the wing (Figure 6). These plates keep the lift from becoming zero at the wing tip and thus reduce the lift variation across the span so that the induced drag is less.

If we now compare two of the best soaring birds, the albatross and

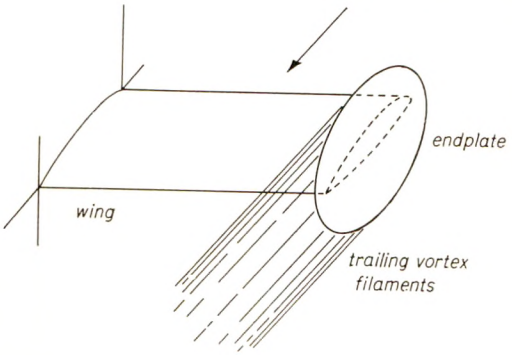


FIG. 6. Endplate on a conventional wing to reduce the induced drag.

the condor, in the light of the above discussion, one fact stands out. The albatross meets the requirements for being a superb soarer; he has an aspect ratio of nearly 15. This is a very high value. The condor on the other hand should hardly be able to soar at all. His aspect ratio is only about 6, yet for his size he exhibits an unexcelled soaring ability. The paradox is evident: How can a low aspect ratio wing possess high aspect ratio efficiency? The question is even more acute since the circling flight of the land bird requires values of C_L as high as 1.6 where the induced drag becomes extremely large. The answer clearly lies with the endplate mechanism and a critical aerodynamic analysis of the function of the characteristic fanlike wing-tip of the condor provides the answer to the riddle.

It should be noted that the slotted wing-tip is a common but unique structure of land soaring birds possessing low aspect ratio wings. It is apparent that the slotted tip is intimately associated with induced drag. Sea birds have found the high aspect ratio wing quite suitable to their way of life while the land soarers have not found it quite so advan-

tageous a device. Instead, they have for very definite reasons developed an ingenious mechanism for getting high effective aspect ratios without either the fragility or weight penalty so characteristic of high aspect ratio wings. We will now consider the aerodynamics of the slotted tips and then suggest why they are so prominent in land birds and absent in true sea soarers.

Since my experimental work has been done with wings of Black Vultures (*Coragyps atratus*), I shall discuss the geometry of this particular bird. The discussion, however, applies equally well to all land soarers. The effective wing-tip consists of seven long slender primaries which, considered as individual air foils, have very large aspect ratios (A = about 14). In the circling phase of flight these pinions are always held fully extended and present somewhat the appearance of a fan as they radiate out from the main wing. This spreading out separates the pinions from one another and leaves large gaps or slots between successive feathers. The pinions are also spread out *vertically* due to their bending or curving upward under the air load they carry. The leading pinion bends upward strongly, with its tip almost vertical. The second pinion also has a strong curvature, but less than the first, and so on until the sixth and seventh pinions actually curve downward in flight. The first three pinions are all heavily loaded; the fourth through the seventh are not nearly so curved. A good idea of the three dimensional disposition of the pinions in flight can be obtained by fully spreading the fingers on the hand and then raising the thumb as high as possible while twisting the hand so that the little finger is turned down. In this way each succeeding feather lies below the plane of the one in front of it so that each pinion is essentially operating as an individual airfoil. Flight photographs of the pinion spread of condors appear in plates 9 and 11 in Koford (1953) and of vultures in Storer (1948: 70). (In these photographs the pinions are only moderately loaded.) In flight, the curvature of these pinions is automatic. That is, they assume their proper curvature automatically due to their elastic deformation under the air loading and not because of direct control by the bird. I have shown this by testing a rigid vulture-wing in a wind tunnel. At angles of attack below stall and at speeds equal to the usual flight speed of the bird (20–25 miles per hour) the pinions automatically spread out vertically in *exactly* the same pattern as that photographed in soaring birds. Different degrees of pinion spread and curvature can be obtained by varying the wind speed while holding the angle of attack of the main wing constant or by varying the angle of attack while holding the air speed constant. In actual flight, of course, the angle of attack uniquely determines the equilibrium air speed of the bird, so that this natural combination we might call the "design" flight conditions. Each pinion has a carefully tailored structure to give a specific elasticity gradient and twist so that it will attain a precise

curvature for the specific angle of attack being flown. When not under an air load the pinions all curve strongly downward at the tips. The leading pinion has an appreciable geometrical twist near its tip so that it presents a rather large angle of attack to the airstream, and hence, in flight, the heavily lifting tip produces a sharp curvature of the pinion. The second pinion is also twisted but to a lesser extent. The third pinion has only a slight twist, while the others have none. However, in flight, the fourth pinion still has some span curvature just due to the elasticity of the feather. In flight the tips of the first four pinions have been bent from the strong downward curvature to an upward curvature. The force necessary to do this is appreciable and for large birds like the condor the force may amount to a pound or so for bending a single pinion. At lower lifts the feather curvature automatically decreases and the wing assumes a more conventional tip shape. As the pinions bend or curve under the air load, stresses are set up in the vanes of the feather which cause it to become more and more cambered as it bends. Such cambered airfoils are capable of producing much more lift than flat sections, and this explains why the pinions become so highly curved at low flight speeds. Here we have the ultimate in aerodynamic design: the use of aeroelasticity for automatic geometry control.

Now when the bird is operating at a high lift coefficient, as in circling, if the wing-tip were one continuous piece, the lift would go rapidly to zero at the tip thus producing a strong vortex sheet behind it. However, the tip is actually broken into seven smaller, high aspect ratio vanes which are strongly curved upward and inward as discussed. In order that these vanes remain curved in this manner a component of the air-load must act in a spanwise direction, and this means that the pinions must be throwing air outward, away from the center of the wing. This outward flow prevents air from sweeping in from the tip over the upper surface of the main wing, as it would otherwise do, and thus prevents the formation of a trailing vortex sheet behind the main wing. The total wing circulation is of course shed in very weak vortex sheets from the high aspect ratio pinions, but due to the spreading of the vorticity over a large area instead of confining it to a single tip vortex, the kinetic energy of the flow field is appreciably reduced as is the associated induced drag. In effect, therefore, the pinions can considerably reduce the over-all intensity of the trailing vortices in exactly the same manner as would a large endplate. In fact, each pinion may be considered as a small endplate, eliminating its portion of the induced drag.

An alternative explanation of the drag reduction mechanism of the pinioned wing is to consider it as a *multiplane system* in which the planes (the pinions) merge to form the main wing. If we apply Munk's results (1921) on multiplanes, it is easily proved that the pinioned wing must reduce the induced drag over that of a single wing. Newman (1958) has

concluded that the pinions do not reduce the induced drag, as a result of a simple analysis of tandem airfoils to which he compares the bird wing. It follows, of course, directly from Munk's Stagger Theorem that merely splitting a flat airfoil into several separate foils arranged one directly behind the other has no effect at all on the total induced drag. However, the vertical arrangement or gap of the individual foils has a critical effect which Newman failed to consider: a multiplane system with gaps always has less induced drag than a monoplane of equal span and lift.

To test this theory I surveyed the airflow about and behind the vulture wing previously mentioned with a thin cotton thread mounted on the end of a long wire, using a high angle of attack. The airspeed was 20 miles per hour, the normal circling speed for Coragyps. (Unfortunately it was not possible to make direct measurements of the forces on the wing due to the lack of a balance.) Nowhere in the tip region or behind the wing could a developed vortex flow be found so long as the pinions were allowed to maintain their natural positions. Any distortion of the leading pinions' configuration, such as depressing the leading pinion, immediately produced a large trailing vortex. As I previously mentioned, the vortices actually come from the wing in a sheet form, but they quickly roll up into one concentrated vortex which may easily be detected with the streamer. The stronger the vortex sheet is the more rapidly it rolls up, so this may be used as a measure of the vortex intensity. As just stated, no developed vortex motion could be located anywhere behind the pinions, indicating that the trailing vorticity is quite diffuse. The streamer did indicate a small upwash outboard of the wing-tip, however, showing that some induced velocities were still present.

It appears that here we have found in nature an ultimate exploitation of the endplate principle. The slotted tip, so characteristic of land soarers, is thus a drag reduction device which automatically allows the bird to obtain high values of $C_L{}^{15}/C_D$ at high values of C_L, without the disadvantages of the high aspect ratio wing or large endplates. This is truly a remarkable achievement from both the aerodynamic and structural viewpoints, since it is accomplished by automatic aeroelastic geometry control. The emulation of this principle by man in his sailcraft offers an interesting and rewarding challenge.

We have seen that the land soarers in general use the slotted tip for induced drag reduction while the sea birds such as the albatross and gull use the high aspect ratio wing. The question arises as to why the different wing forms have evolved. The answer is perhaps obvious but it presents such a beautiful picture of the perfection of adaptation in nature that I shall consider it in detail. The vulture must possess excellent soaring ability for it must spend many hours on the wing in search of food. Yet it must possess the ability of the flapping bird also for it must alight and take off from thick undergrowth and rugged terrain. It

does not require much reflection to predict the fate of a bird with wings like the albatross required to operate under such conditions. The albatross soars over smooth water and takes off from long, level beaches. Here, he has no worry of obstructions and hence operational conditions are perfect for his long slender wings. However, on some islands where the albatross nests, even small bushes pose a serious threat and birds have often been found with their huge wings hopelessly entangled in small shrubs, thus testifying to the dangers that await high aspect ratio wings on forested terrain. Hence the operational conditions attending the vulture's mode of life demand a low aspect ratio wing.

Another fundamental factor is involved in the relative structural weights of the two birds. The vulture lives by tearing flesh from dead animal carcasses. To do this requires a rugged skeletal frame and strong muscles of head, neck, and legs. The bird must be relatively large to perform this mission. In addition, rapid take-off from a closed place requires considerable flapping muscle. Even if the vulture had no worry from entanglements, the luxury of a high aspect ratio wing with its weight penalty could scarcely be afforded. With the pinions, the smaller aspect ratio allows a very light wing structure since the pinion weight is negligible. The albatross on the other hand needs little flapping muscle, only light skeletal and muscle systems, and has scarcely any legs or feet. Thus the albatross can, in view of these light-weight components, afford the heavier high aspect ratio wing. These reasonings are borne out by actual measurements. For example, I have found the structural weight of the dried wings of the Black Vulture and Laysan Albatross to be respectively 0.119 and 0.242 lb/ft^2, showing that the low aspect ratio wing is indeed more efficient structurally. In all probability the "wet" weights of the albatross wing would be a much higher percentage than the vulture's because of the greater muscle and tendon system required by such a wing.

A most perfect example of this relative weight factor is obtained by comparison of the Turkey Vulture with the Magnificent Frigate-bird (*Fregata magnificens*). Both birds weigh 3½ pounds. However, the Turkey Vulture has a span of only 70 inches while the Frigate-bird has a span of 90 inches. The relative aspect ratios are not known exactly but are probably of the same ratio as the condor and the albatross. The wing loading of the Turkey Vulture is about 0.86 lb/ft^2; that of the Frigate-bird is unknown but may be calculated at 0.87 lb/ft^2 for an assumed aspect ratio of 14. There is little doubt that much of the weight of the Frigate-bird lies in its wings, since it has scarcely any legs or feet at all. The Turkey Vulture has relatively heavy legs and body for its survival mission and therefore must possess a light wing structure. It would be a most interesting study actually to dissect and compare the relative weights of the various component parts of these two birds in

detail to see if the prediction of relative weights arising from aerodynamic considerations are borne out. If it may be suggested, this would form the basis for a most valuable graduate thesis and it is hoped that such a study will be undertaken.

It thus appears that, in order to obtain the high aerodynamic efficiency and low wing loading so critical to circling flight within the thermals, the land birds have made use of the weight savings and other advantages afforded by pinions, while the sea birds have developed long, slender wings. Some birds which soar over both land and sea, such as the pelican, have a combination of both very high aspect ratio and slotted-tips. These birds practice both dynamic and thermal soaring.

The question arises as to why do sea birds not make use of the slotted-tip since it is advantageous from a weight standpoint. The obvious answer is of course that the high aspect ratio wing is quite well-suited to the dynamic soaring practiced by sea birds, while the slotted-tip wing is best suited to circling flight. In thermal soaring, very high lift coefficients must be attained to pull the bird around circles of small radii. Here the large chord of the low aspect ratio wing of the vulture raises the local Reynolds number (Appendix I) to a sufficiently high value that the wing boundary layer is mostly turbulent and the wing does not stall until very large angles of attack are reached. This need for a large chord by land soarers is especially acute. As the wing angle of attack is increased, the flight speed is decreased so that, at high angles of attack, a large chord is mandatory to offset the loss in velocity and thus attain a sufficiently large Reynolds number so that the wing will not stall. The small chord of the high aspect ratio wing leads to low Reynolds numbers for moderate flight velocities, with a laminar boundary layer. Clearly, high lift coefficients cannot possibly be attained with such a wing, unless some outside influence can be made to force turbulence in the boundary layer. In the dynamic soaring of sea birds it is doubtful if high lift coefficients are ever needed. Since we are very close to the natural transition Reynolds number in the flight speed range of soaring birds, these differences in chord length can have a pronounced effect in determining the type of wing which *must* be used for each type of soaring. The laminar boundary layer associated with the high aspect ratio wing of the sea bird leads to a considerable reduction in the skin friction drag and since the induced drag is low (i.e., C_{D_i} is small) the over-all drag of the bird can be very low indeed.

Unfortunately we are here getting into more detailed aerodynamics, which is not our immediate purpose. It will be sufficient to say that the local Reynolds number effect on maximum lift coefficient can quite satisfactorily explain the aspect ratio differences between land and sea soaring birds.

Finally, we may mention that if a thermal is large enough in diameter,

or if the bird's wing loading is low enough, a bird with a high aspect ratio wing without slotted-tips can make very good use of thermals. Gulls do it routinely; but more heavily loaded birds would require immense thermals. However, it is doubtful if land soarers could practice dynamic soaring over water to any useful degree. From the observational data available it appears that land birds simply have too much drag for the limited energy available in ocean wind shear layers. They must remain over land where their highly developed geometry allows them full use of the small but highly concentrated energy regions within thermal bodies. Likewise, the ultra-refined soaring birds of the sea must remain within the broad but weak energy regions of the shear layer where their low drag can be used to best advantage.

Concluding Remarks

It has been the intention of the foregoing discussion to present at least the essential features of the author's conclusions as to the nature and mechanics of the thermal soaring of birds. Due to the extensive scope of the subject it has been necessary to omit almost completely the technical and mathematical aspects of the meteorological and aerodynamic developments, as well as the many supporting facts. While there is little doubt that thermal soaring as discussed herein is the principal mode of flight of land soaring birds, it is by no means the only mode and a great deal of work must be done before we can clearly define the complete theory of natural soaring flight in all its phases. In particular, the subjects of dynamic soaring in gusty weather and of flex-gliding and lift-gliding aerodynamics are of considerable importance and should be most fully investigated.

The prime features of the theory are the concepts of the closed thermal body with the accompanying internal circulation, and of the function of the slotted wing-tip in reducing induced drag. These two factors, combined with the aerodynamics of flight within such a thermal body, have been able to explain quite satisfactorily the observed details of thermal soaring and have offered a number of suggestions as to the reasons for the structural differences in land and sea soaring birds.

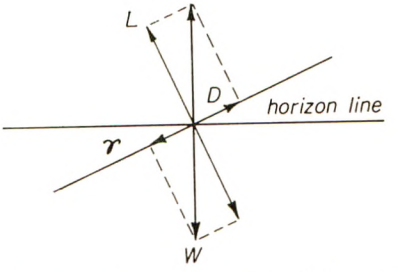
Most important of all, however, the theory suggests many new areas and possibilities for experimental investigation of natural soaring and it is hoped that such research by both aerodynamicists and ornithologists will be initiated and carried out. When the quantitative theory of natural flight has been sufficiently developed, we shall be in a position to attack the fascinating and ultimately important problem of man-powered flight, with full certainty of success.

APPENDIX I

Soaring Aerodynamics

Aerodynamic Forces: As a bird flies through the air, it experiences a resultant

force due to the air pressure distribution over its wings and body. The component of this force normal to the flight direction is called the lift, while the component opposite to the flight direction is called the drag. (Note that lift need not always act normal to the horizon.) If the bird is flying a straight course at a constant speed, the sum of all forces acting on it must be zero. Hence it is necessary that the resultant aerodynamic force be equal to the bird's weight. When the forces are resolved along the flight path, it is necessary that a thrust force exist equal to the bird's drag. For a gliding bird moving through still air, there is no aerodynamic thrust force. Hence, the bird must glide down toward the earth at such an angle that the resultant aerodynamic force exactly balances the weight,



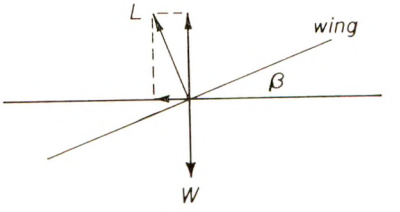


FIG. I-1. Force system on a gliding bird. FIG. I-2. Force system on a circling bird.

as shown in Figure I-1. The gliding angle γ is determined strictly by the ratio of lift to drag, $\tan \gamma = D/L$.

The lift and drag forces are determined by the relations

$$L = C_L \cdot \tfrac{1}{2}\rho V^2 \cdot S \qquad \text{and} \qquad D = C_D \cdot \tfrac{1}{2}\rho V^2 \cdot S$$

where

L = lift force
D = drag force
C_L = lift coefficient
C_D = drag coefficient
ρ = air mass density
V = relative air velocity
S = wing area (plan projection)

The coefficients are constants but depend for their value on the shape of the bird and on the geometry and angle of attack of its wing. The angle of attack is determined by the condition that the sum of all the force moments about any axis parallel to the wing span equal zero.

Soaring Flight: In the glide at constant velocity V, the force component balancing the drag is $W \sin \gamma$. Since the bird has a velocity V in the direction of the force, the weight W is doing work against the drag at the rate $WV \sin \gamma$. Since the vertical component of the bird's velocity (the sinking velocity) is $V \sin \gamma$, the bird is losing potential energy at the rate $WV \sin \gamma$, and we see that the loss of altitude is furnishing the energy to power the glide. Suppose now that the bird were gliding down through an enormous box of air, but that this box was rising vertically (relative to the earth) with the speed $V \sin \gamma$. Then, although it would still be sinking relative to the air at the rate $V \sin \gamma$, the bird would be flying parallel to the surface of the earth with a velocity $V \cos \gamma$. If the box of air were moving upward with a speed greater than $V \sin \gamma$, the bird would

actually be gaining altitude with respect to the earth. In this case the power for flight comes from the energy of the rising air. This simple picture gives the basic principle of static soaring flight. (See Appendix II.)

Induced Drag: Since the sinking velocity $V \sin \gamma$ should be of minimum value, so that even weak vertical air motions can sustain the bird, it is necessary that the drag be as small as possible. At the high C_L values needed for circling flight, the primary drag source is the induced drag. Induced drag is dependent upon the rate at which the lift force varies across the span of the wing. The primary geometric factor influencing this variation is the wing aspect ratio A, given by $A = b^2/S$, where b is the total wing span from tip to tip. The greater the value of A for a given wing plan form and lift, the smaller will be the induced drag.

Turns: If a bird wishes to make a turn, it is necessary that a component of the aerodynamic force act toward the center of the turn to provide the necessary centripetal acceleration. This is accomplished by banking the wing through some angle β as shown in Figure I-2. In this way a component of the lift acts to pull the bird around the turn. Since the lift is no longer acting in a vertical plane, the lift force must be greater in a turn if it is to balance the weight force.

Wing Stall: Very near the surface of a wing (or body) the air is retarded due to the frictive resistance. This very thin layer is called the boundary layer and it has a pronounced effect on the angle of attack at which the wing stalls. Aerodynamic theory shows that for a certain critical value of the Reynolds number $R = (Vc)/\nu$ (where V is the flight speed, c is the wing chord at a spanwise point, and ν is the kinetic viscosity of air) the boundary layer changes from a smooth laminar flow to a turbulent one. The turbulent layer, since it has more energy in it, is able to remain attached to the surface at much higher angles of attack than can the laminar boundary layer. Most birds have flight speeds and wing chords such that the Reynolds number is well below critical and hence high angles of attack cannot be attained without stall. Hence, the necessity for having a turbulent boundary layer is critical for wings which must attain high lift coefficients, and to attain such flow the chord of the wing or the flight speed must be large. In circling flight, at small radii of turn, a large velocity is precluded so a large chord and relatively rough wing surface are needed if high lift coefficients are to be attained.

This brief discussion is intended only to point out the principles pertinent to soaring flight. The necessary details of flight mechanics are readily available in aerodynamics texts.

APPENDIX II

Classification of Soaring Flight

Birdflight in which the wings remain stationary or rigid may be divided into two types. In *gliding* flight the bird merely glides through the air with a continuous loss of altitude, and ultimately comes to earth. In *soaring* flight the bird is able to fly a level course or even gain altitude by using the energy of the vertically moving air. We are here interested only in the soaring phase.

The principles of fluid dynamics allow us to state quite simply the two atmospheric conditions necessary for a bird to remain in the air without flapping its wings. One of the following must be true for sustained soaring:

(1) the air must have a vertical component of velocity, or otherwise

(2) the motion of the air must not be of uniform speed.

These simple facts were first stated by Lord Rayleigh in 1883. They lead to the conclusion that a bird cannot soar in still air or in a wind of constant horizontal velocity.

These two requirements form the basis for the classification of soaring flight. Soaring which is based on the utilization of the vertical motions of the air is called *static soaring*. It is almost exclusively the form used by land soaring birds. Soaring using the non-uniformities of the wind is termed *dynamic soaring*. This form is occasionally practiced by land birds in gusty or stormy weather, but the available evidence indicates that this form is of much less importance than static soaring to land birds. For sea soarers such as the albatross, however, dynamic soaring is probably used almost exclusively. We are primarily concerned in this paper with the soaring of land birds, so we shall discuss only static soaring.

Static soaring may be subclassified according to the manner in which the vertical air velocity is produced. These are (1) declivity winds and (2) thermals.

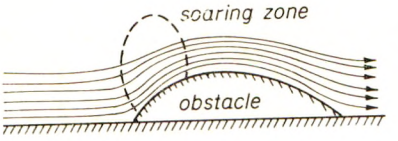
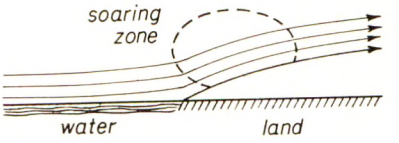


FIG. II-1. Declivity currents formed by an obstacle (hill).

FIG. II-2. Declivity currents formed by a land boundary layer.



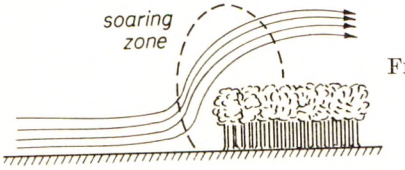


FIG. II-3. Declivity currents formed by trees.

Declivity currents arise when the wind moving parallel to the earth encounters an obstacle, such as a hill (Fig. II-1). The air is forced upward as it approaches and passes over the obstacle thus producing a vertical velocity component that can be utilized by the birds. Declivity winds are also produced by ground friction. When wind blowing over a large body of water suddenly meets the shore, the increased friction between the air and the ground causes a retardation of the lower air strata and a boundary layer forms. This retarded layer has the same effect as a solid obstacle and produces declivity winds (Fig. II-2). The same effect occurs when wind moving over relatively smooth land suddenly encounters rough or forested terrain. Gulls may commonly be observed making use of the shore declivities.

It is obvious that in order to have declivities of sufficient strength to support soaring we must have strong surface winds and/or very large obstacles. In addition, the location of these obstacles is fixed, so that soaring is necessarily restricted to the immediate vicinity of the obstacle. Declivity winds are thus so restricted that they are in general of small value to soaring birds, whose primary reason for flight is to cover large areas of land in search of food. Only in the special case of the beach soaring of gulls and other shore birds, does declivity

soaring assume some importance. The mechanics of soaring continuously over a given area in a constant declivity wind are quite simple and have long been exploited for endurance purposes by sailplanes. However, due to the relative unimportance of this form of soaring for natural flight we shall omit its discussion.

Thermals occur when surface layers of air become so warmed and/or moisture-laden by contact with the sun-heated earth that they are less dense than the layers above them and tend to overturn, forming rising bodies or bubbles of air. The formation of thermals depends only on the presence of sufficient sunshine and thus their distribution is universal over all land areas. Due to their wide and continuous distribution, thermals alone appear to offer the sustaining means necessary for practically useful soaring flight. The year round availability of strong thermals is the reason why the highest degree of soaring is observed in temperate and tropic regions. It is because of the availability of thermals that soaring is possible over broad flat plains in a form equal or superior to that observed in mountainous country where declivities are always available. (It is interesting to note that, even in mountainous country, thermal soaring is quite as widely used as is declivity soaring—hills are excellent thermal producers.)

This availability of thermals is the condition upon which the very existence of the large soaring birds such as the vultures depends. This is the only atmospheric condition which will permit them to travel so effortlessly over such wide areas in search of food, and they have adapted themselves to exploit it to the fullest extent. The quantitative records kept by Hankin (1913) clearly reveal the critical importance of "sun soarability." Only in very adverse weather are gust-soaring and flap-gliding used by Indian vultures. These forms of flight both require considerable exertion by the bird, and, considering the great percentage of the day which the bird spends in flight, it is clear that he could not long meet the energy demands of these types of flights.

Hence we may reasonably conclude that of all the forms of soaring flight, thermal soaring is by far the most important for natural flight.

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TOBERMORITE GEL— THE HEART OF CONCRETE

By STEPHEN BRUNAUER

WHEN PORTLAND cement is mixed with a limited amount of water, the resulting paste sets, i.e., it becomes reasonably firm in a few hours. The paste then continues hardening for months or even years. This hardened paste is the matrix in which the aggregate is embedded in concrete—the grains of sand, the pieces of gravel or crushed stone are held together by the cementing action of hardened paste. Hardened paste and aggregate together form the artificial rock called concrete. Most of the important engineering properties of concrete, such as strength, dimensional stability, and permeability to water, are determined primarily by the properties of the hardened portland cement paste matrix [1].

Portland cement is a mixture of several compounds, among which the most important ones are two calcium silicates: tricalcium silicate, Ca_3SiO_5, and β-dicalcium silicate, β-Ca_2SiO_4. These two silicates constitute about 75% of a portland cement by weight. In the hydration reaction, the two silicates produce similar calcium silicate hydrates and different amounts of calcium hydroxide. The calcium silicate hydrate, because of its similarity to the natural mineral tobermorite [2], has been called tobermorite gel, and this gel is the most important constituent of hardened portland cement paste and, consequently, also of concrete. Tobermorite gel plays a vital role in determining the rheological properties of fresh portland cement paste, which properties in turn determine the consistency and workability of fresh concrete. Tobermorite gel plays a dominant role in the setting and hardening of the paste and in determining the strength and dimensional stability of hardened paste and concrete. It is, therefore, no exaggeration to say that tobermorite gel is the heart of concrete.

Hydration of the Calcium Silicates

1. Pastes of tricalcium silicate and dicalcium silicate set and harden, qualitatively, in the same manner as do pastes of portland cements. (There are four crystalline varieties of calcium silicate, but only one occurs in portland cements, the β-form. Whenever reference is made to dicalcium silicate, the β-form is to be understood.)

If one mixes tricalcium silicate with a limited amount (e.g., 0.7 of its own weight) of water at room temperature, and allows the mixture to stand, the reaction goes to completion in about a year. Examination of the hardened paste [3] leads to the following stoichiometry of the reaction

$$2Ca_3SiO_5 + 6H_2O = Ca_3Si_2O_7{\cdot}3H_2O + 3Ca(OH)_2 \qquad (1)$$

The tricalcium silicate was ground to the fineness of portland cement, which has an average grain size of the order of 10μ. This is a very fine powder from the point of view of a chemical engineer, yet the surface of the unhydrated material is negligible compared to the surface of the hydrated material. The specific surface area of the calcium silicate hydrate produced in the hydration is of the order of 300 m^2/g, which is about 1000 times the specific surface of the tricalcium silicate. The calcium hydroxide appears in relatively large crystals, making a negligible contribution to the surface of the hardened paste. The surface of the calcium silicate hydrate will be discussed in detail later.

The calcium silicate hydrate produced in the hydration of tricalcium silicate (and, as will be seen, of dicalcium silicate also) is a gel. It is called tobermorite gel or tobermorite (G).

2. There were two difficulties encountered in the determination of the stoichiometry of the hydration of the above reaction: one was in the determination of the calcium hydroxide produced in the reaction, the other in the determination of the combined water content of the tobermorite gel.

Uncombined calcium oxide and calcium hydroxide can be quantitatively extracted by means of a number of organic solvents or solvent mixtures. The method found most suitable by us was proposed by Franke [4], and it employs a mixture of acetoacetic ester and isobutyl alcohol. The trouble was, however, that whether we used the Franke solvent or any other, a part of the lime was extracted also from the calcium silicate hydrate.

It seemed, therefore, that the calcium hydroxide should be determined *in situ*. A method of quantitative analysis by means of X-ray diffraction, developed by Copeland and Bragg [5], appeared well-suited for this purpose. It was tried, but it led to variable results, averaging about 85% of the value indicated by equation (1). This meant, we thought, that either the CaO/SiO_2 ratio in the calcium silicate hydrate was greater than 1.5 or some of the calcium hydroxide was adsorbed on the tremendous surface of the tobermorite gel. X-ray analysis determines only the amount of crystalline calcium hydroxide.

We returned, therefore, to the chemical method of extraction. It was true that the Franke solvent extracted some of the lime from the tobermorite, but the rate of extraction was much slower than the rate of extraction of the uncombined calcium hydroxide. We utilized this difference between the rates of extraction to determine the lime contents of both hydration products, tobermorite (G) and calcium hydroxide. Actually, we developed three different modifications of the Franke method [6,7], and each led to the result shown in equation (1). We found also that the non-crystalline calcium hydroxide was not adsorbed on the surface of the tobermorite. The adsorption of calcium hydroxide was

slight, doubtless because, in the competition between calcium hydroxide and water for the surface, water prevailed. The non-crystalline calcium hydroxide, i.e., that undetected by X-ray analysis, was amorphous or very poorly crystallized material [8].

3. The second difficulty was encountered in the determination of the combined water content of tobermorite (G). The large surface of the gel adsorbed large amounts of water. The question was how much of the water in the hydrate was chemically combined water and how much was adsorbed water.

A complete physical separation of the two types of water by methods known at present was impossible. If one equilibrates the hydration products at a water vapor pressure of 5×10^{-4} mm, which is the vapor pressure of ice at $-78°C$, the tobermorite contains just a little more than two molecules of water. Probably less than 1% of this is adsorbed water. If one equilibrates at a water vapor pressure of 8×10^{-3} mm, which is the equilibrium pressure of $Mg(ClO_4)_2 \cdot 2H_2O$ and $Mg(ClO_4)_2 \cdot 4H_2O$, the tobermorite contains 2.8 molecules of water per molecule. Adsorption experiments revealed that 0.3 moles of water per mole of tobermorite was adsorbed water, and 2.5 moles of water was chemically combined. We have called the first method of drying dry-ice drying or D-drying, the second, perchlorate drying or P-drying.

Tobermorite (G) is a colloid and, like other colloidal hydrates, it loses water from its structure continuously when equilibrated at lower and lower vapor pressures of water. The free energy of binding of the adsorbed water to the surface is variable, as in most adsorbent-adsorbate systems. Although the average free energy of binding of the combined water in the structure is greater than the average free energy of binding of the adsorbed water to the surface, the most firmly bound adsorbed water is held more strongly than the most loosely bound combined water. As a result, at lower vapor pressures some of the combined water is removed, while some of the adsorbed water is held by the hydrate.

At higher vapor pressures, tobermorite (G) would contain more than 2.5 moles of combined water and more than 0.3 moles of adsorbed water. Such experiments have not been performed to date, so the three molecules of water indicated in equation (1) are somewhat conjectural. Bernal [9] proposed the structural formula $Ca_2[SiO_2(OH)_2]_2[Ca(OH)_2]$, which is equivalent to three molecules of water in tobermorite (G). Our experiments are consistent with Bernal's formula, except that $CaO \cdot H_2O$ should be written in place of $Ca(OH)_2$. After P-drying, the formula is $Ca_2[SiO_2(OH)_2]_2CaO \cdot \frac{1}{2}H_2O$, and after D-drying the formula becomes $Ca_2[SiO_2(OH)_2]_2 \cdot CaO$.

4. Dicalcium silicate reacts with water much more slowly than tricalcium silicate. The ions are densely packed in Ca_2SiO_4, whereas the structure of Ca_3SiO_5 has "holes" in it [10], which may explain why the

latter is more easily attacked by water. Also, Ca_3SiO_5 is thermodynamically unstable with respect to Ca_2SiO_4 plus CaO [8].

When dicalcium silicate is mixed with 0.7 of its own weight of water at room temperature and the mixture is allowed to stand, the reaction does not go to completion even in several years. Examination of a more than four years old hardened paste, which still contained about 15% unhydrated Ca_2SiO_4, has indicated the following stoichiometry of the reaction

$$2Ca_2SiO_4 + 4H_2O = Ca_{3.3}Si_2O_{7.3}\cdot 3.3H_2O + 0.7Ca(OH)_2 \qquad (2)$$

The tobermorite gel in this paste is somewhat different chemically from the one that appears in equation (1); for example, the molar CaO/SiO_2 ratio in this gel is 1.65 as against 1.50. Nevertheless, the two gels are very similar; their X-ray diffraction patterns are the same, and electron-microscopical examination reveals the same morphology. The specific surface areas are somewhat different, as will be discussed later.

Tobermorite (G) is, thus, not a single compound, nor is it merely the two compounds shown in equations (1) and (2). It is a series of hydrates of continuously varying compositions. The lowest CaO/SiO_2 ratio observed by us for a stable tobermorite (G) was 1.39, the highest was 1.75 [11]. In the early stages of the hydration reactions, even lower ratios were observed for Ca_2SiO_4 and even higher ratios for Ca_3SiO_5. The word "stable" does not intend to imply thermodynamic stability. It is probable that only one member of the series is stable thermodynamically, that with the highest CaO/SiO_2 ratio.

The Tobermorite Group

1. The tobermorite (G) series of hydrates constitutes a subgroup in the group of hydrates called tobermorites. The natural mineral tobermorite itself is, strictly speaking, not a single hydrate but a subgroup, because samples found in different localities have slightly different chemical compositions and X-ray diffraction patterns [12].

All tobermorites, natural or synthetic, are layer crystals. The natural mineral that was found in Northern Ireland had the formula $Ca_4(Si_6O_{18}H_2)\cdot Ca\cdot 4H_2O$. Its crystal structure was determined by Megaw and Kelsey [13], who found that the layers were actually triple sheets, with some similarities to vermiculite. Taylor and Howison [14] described the structure as follows.

> "The central part of each layer, equivalent to the octahedral layer in a clay mineral, could be described as a distorted calcium hydroxide sheet divested of all its hydrogen atoms. This sheet is flanked on both sides by parallel rows of wollastonite-type chains, which are kinked in a plane perpendicular to that of the sheets. Only two-thirds of the tetrahedra in each chain are linked directly to the central CaO_2 sheet by sharing of oxygen atoms. The other third, which are held away from the CaO_2 sheet, will be called "bridging" tetrahedra. The composite 2:1 sheets so

far described, have the composition $Ca_4Si_6O_{18}$. Between them are the remaining or interlayer calcium atoms, and water molecules. The distribution of the hydrogen is uncertain. There may be more SiOH than is implied by the formula $Ca_4(Si_6O_{18}H_2) \cdot Ca \cdot 4H_2O$, with corresponding replacement of interlayer water by hydroxyl.

"To a good approximation, tobermorite from the above localities is orthorhombic with $a - 11.23$, $b - 7.37$, $c - 22.6$ Å, $Z = 4$. The crystals are laths with length b (parallel to the chains) and cleavage (001) (the plane of the layers); a and b are both strongly pseudo-halved, and for many purposes a pseudo-cell with $Z = 1$ may be used. This pseudo-cell is body centered. The longest observed basal spacing is therefore 002 ($d = 11.3$ Å), and corresponds to the thickness of a single 2:1 layer."

The molar CaO/SiO_2 ratio in this tobermorite is 0.83. Probably this is the lowest limit for any tobermorite; the upper limit found in our laboratory was 1.75, but others reported even higher values [15,16].

2. When a tricalcium silicate paste is prepared, the water becomes saturated with calcium hydroxide within minutes. The same happens when a dicalcium silicate paste is prepared, except more slowly. The final hydration products, as we have seen, are calcium hydroxide and a member of the tobermorite (G) subgroup. If, however, the silicates are hydrated in a large excess of water, so that the concentration of the lime in the solution is below saturation, the calcium silicate hydrate that forms is not a member of the tobermorite (G) series and, of course, no calcium hydroxide is produced in the reaction. The CaO/SiO_2 ratio in the calcium silicate hydrate depends on the final concentration of lime in the solution. The lower limit of the ratio (at a low lime concentration) is somewhere around 0.8, the upper limit is in the vicinity of 1.5 (at or near lime saturation).

The same series of hydrates can be produced by other reactions, the most widely used among which is the reaction between lime and silica in water. Taylor, who investigated these hydrates most thoroughly, has named the series calcium silicate hydrate (I) or, briefly, CSH (I) [17]. Other names have been used by others (including myself), but in this paper the designation CSH (I) will be used for this tobermorite subgroup. The subgroups tobermorite (G) and CSH (I) differ from each other not only in chemical composition but in morphology, degree of crystallinity and some other properties as well.

The CSH (I) hydrate system was studied by many investigators. In a review paper, in 1947 Steinour [18] reported the results of sixteen investigations. Although there is a vague general agreement between most of these results, they differ considerably in detail. The differences were probably caused, at least in part, by lack of adequate experimental techniques and lack of complete equilibration but, as will be seen later, there are some more subtle reasons also.

Since 1947 other investigations have been published, the results of two of which are reproduced in Figure 1. The molar CaO/SiO_2 in the tobermorite is plotted against the lime concentration in the solution.

The curve of Taylor [17] and the curve of Kalousek [19] are in broad general agreement, but they do not agree in detail. For example, both curves show that there is a sharp rise in the CaO/SiO_2 ratio of the solid at or near lime saturation, but Taylor's curve indicates an upper limit of 1.5 for the CSH (I) series, whereas Kalousek drew his curve to indicate the upper limit of 1.33. The tobermorites corresponding to the sharply rising portion of the curve do not belong to the CSH (I) subgroup. They belong either to the tobermorite (G) subgroup or to another subgroup to be discussed later.

The highest CaO/SiO_2 ratio for a CSH (I) hydrate obtained in this

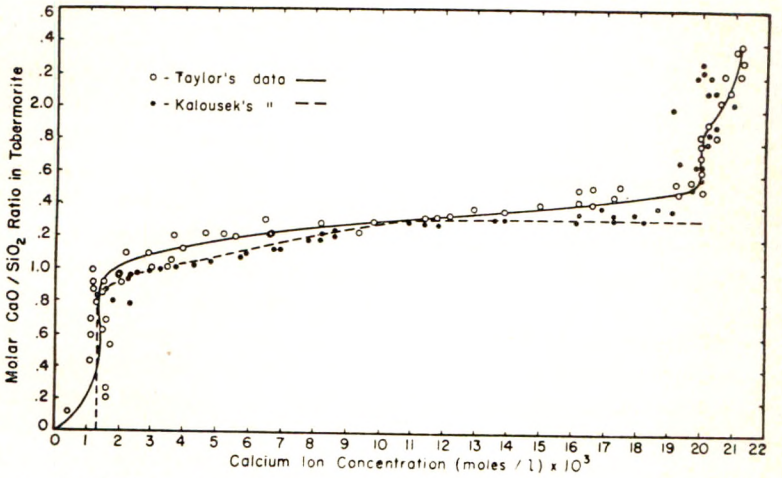


FIG. 1. Variation of the molar CaO/SiO_2 ratio in tobermorites with the calcium ion concentration in the solution.

laboratory was 1.44, but others obtained higher values; for example, Grudemo [20] reported 1.56. The lowest value for the tobermorite (G) series, as stated earlier, was 1.39. It seems, therefore, that the ratios overlap for the two subgroups. This, however, is not certain. It is possible, as Kalousek has pointed out, that the highest lime preparations of CSH (I) contain some tobermorite (G), or the lowest lime preparations of tobermorite (G) contain some CSH (I), or both.

3. The increase in the CaO/SiO_2 ratios of the tobermorites is accompanied by an equimolar increase in the H_2O/SiO_2 ratios [8]. It is a remarkable fact that while the CaO/SiO_2 and H_2O/SiO_2 ratios in the hydrates of the CSH (I) subgroup, from one end of the series to the other, almost double, the dimensions of the unit cell and the density remain practically constant.

When water is removed from tobermorites, the a and b dimensions of the unit cell, i.e., the dimensions within the 2:1 layers described before,

show a very slight shrinkage, but the shrinkage is so small that these dimensions can be considered constant. The *c* dimension, however, i.e., the distance between the layers, shows a considerable shrinkage upon removal of water [17]. Removal of lime from the structure results in an increase in the *c* dimension [20]. Simultaneous removal of a molecule of lime and a molecule of water leaves the *c* dimension practically unchanged [21]. Thus, large changes in composition cause almost no change in the unit cell dimensions.

Taylor and Howison [14] measured the densities of CSH (I) hydrates ranging in CaO/SiO_2 ratio from 0.81 to 1.50, and found no systematic trend to increase with ratio. They have also advanced a hypothesis for the explanation of constant density together with constant unit cell dimension. They have suggested that as the CaO/SiO_2 ratio increases, calcium replaces silicon in the lattice. The "bridging" SiO_4 tetrahedra, which are not attached to the central CaO_2 sheet, are gradually removed or, more correctly, only SiO_2 is removed because two of the oxygen ions in each tetrahedron are common to adjacent groups. The calcium ion enters the lattice, but not directly into the place of SiO_2, i.e., not into the layer, but between layers. Because the calcium ion has two positive charges and SiO_2 is neutral, to preserve charge balance two hydrogen ions must also be removed. The over-all replacement is SiO_2H_2 by Ca. Such a replacement would have a relatively small effect on the cell weight. It would also explain the very slow equilibration between CSH (I) hydrates and calcium hydroxide solutions. If increasing the lime content would involve merely uptake of lime between the layers, one would not expect such a slow equilibration. Finally, the hypothesis predicts that the upper limit of the CaO/SiO_2 ratio would be 1.75, corresponding to the removal of all "bridging" tetrahedra. Although the upper limit of the ratio for the CSH (I) subgroup is around 1.5, the upper limit for the tobermorite (G) subgroup is 1.75, as was stated before, and the upper limit for another subgroup, CSH (II), which will be discussed later, is also 1.75. It is possible that either tobermorite (G) or CSH (II) or both of these subgroups form a continuous series with CSH (I).

Brunauer and Greenberg [8] have modified the Taylor-Howison hypothesis with the suggestion that SiO_2H_2 is replaced not by Ca alone, but by Ca plus a water molecule. Such a substitution would lead to a far better constancy of the cell weight than the Taylor-Howison substitution; it would explain the actually found compositions of tobermorites better; and it would also explain the constancy of the *c* spacing, mentioned earlier. The modified Taylor-Howison hypothesis accounts quite well for the compositions, structures, densities, and some other properties of the hydrates of the CSH (I) subgroup, possibly even of the entire tobermorite group.

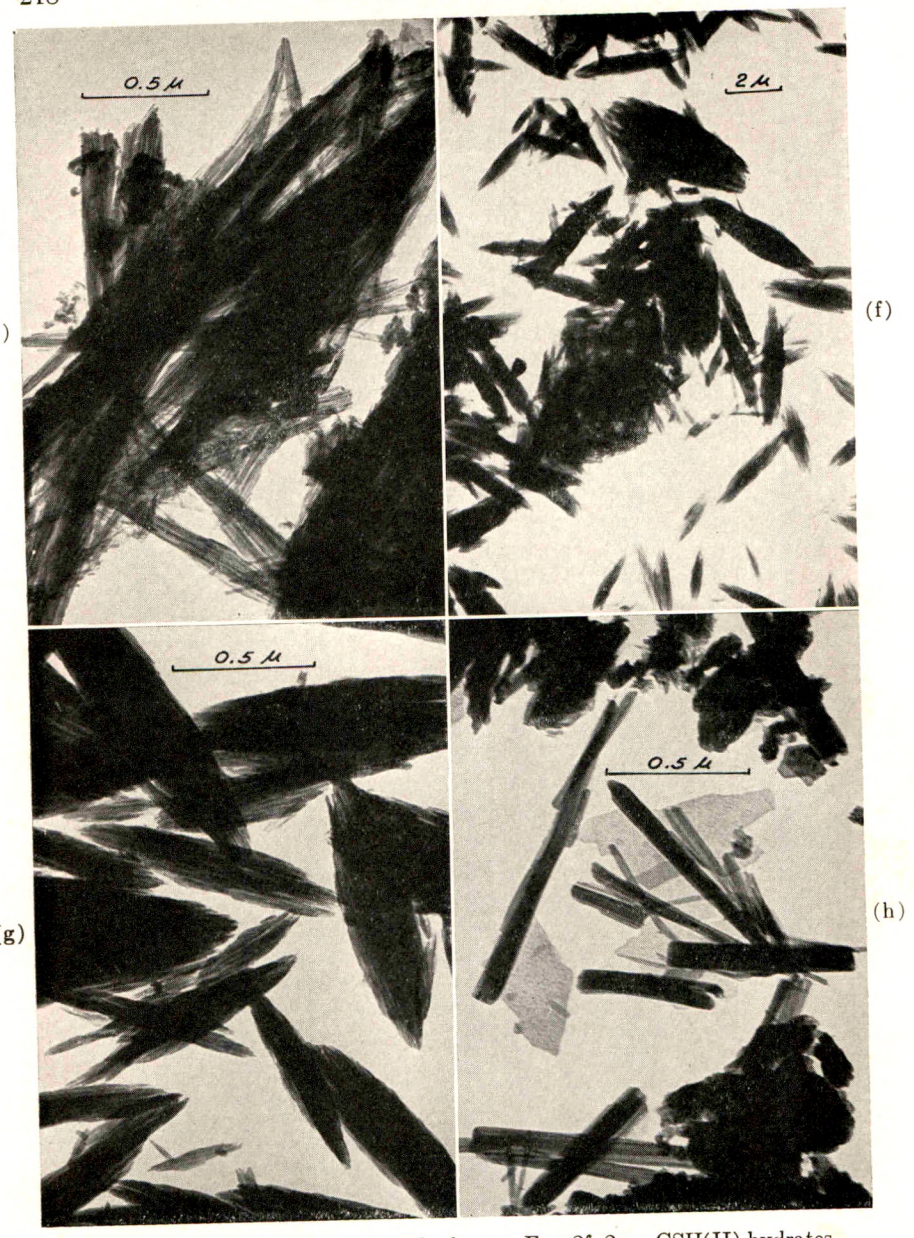
4. While it is true that CSH (I) hydrates of widely different compo-



(a) (b) (c) (d)

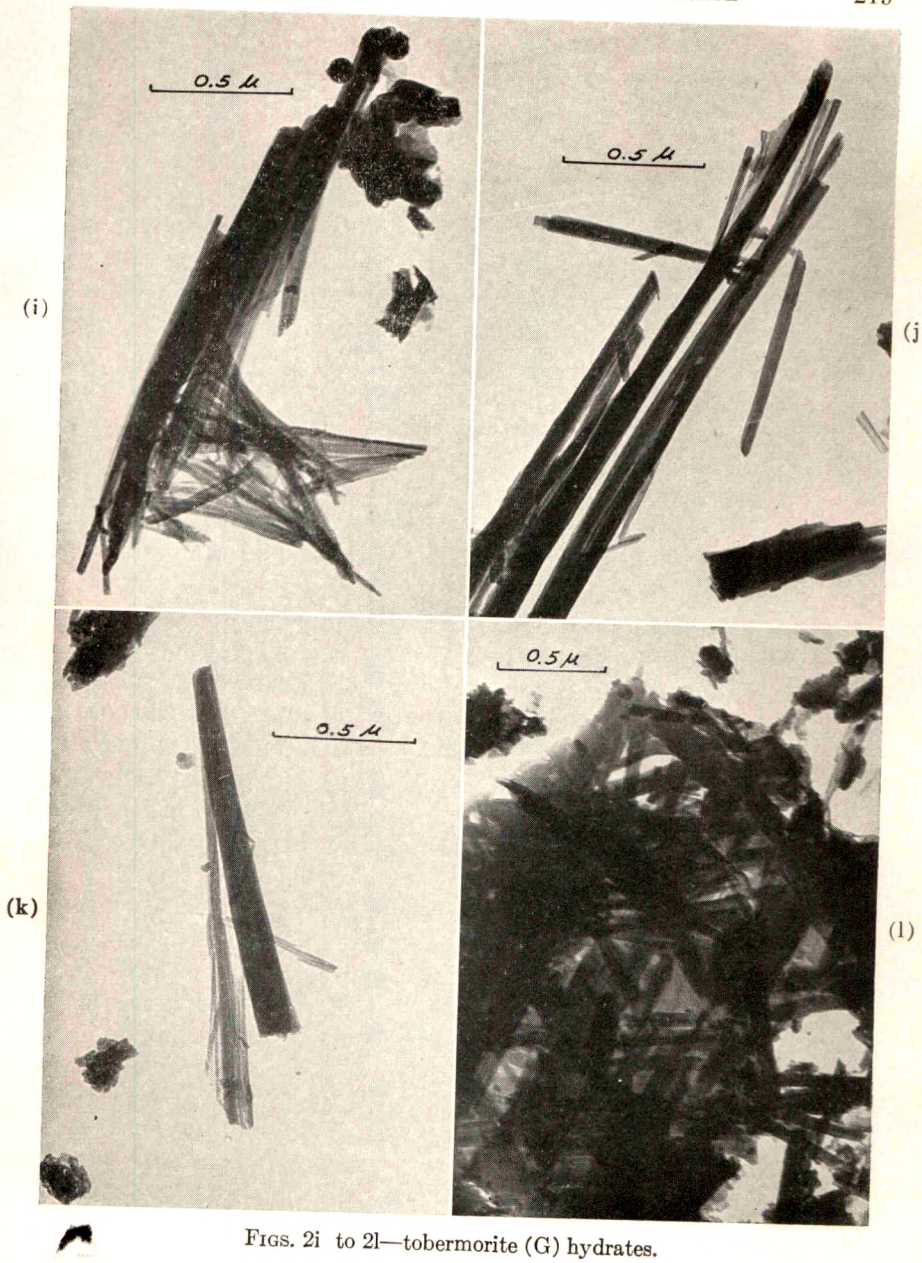
FIGS. 2a to 2d—CSH(I) hydrates.

sitions can exhibit almost identical X-ray diffraction patterns, it is also true that tobermorites of identical compositions can exhibit widely different X-ray patterns. The hydrates first of all differ in degree of crystallinity. Heller and Taylor [12] list 46 diffraction lines for the natural



FIGS. 2e and 2h—FIG. 2e—CSH(I) hydrate. FIG. 2f.-2g.—CSH(II) hydrates.
FIG. 2h—tobermorite (G) hydrates

mineral from Ballycraigy; a hydrothermal preparation from lime and silica may give almost as many diffraction lines, but a room temperature preparation by the same reaction may give less than a dozen diffraction lines. In addition, the lines may appear at slightly different d-spacings,



FIGS. 2i to 2l—tobermorite (G) hydrates.

and their relative intensities may show great differences. Actually, no two investigators report identical X-ray patterns for tobermorite preparations as yet; nor can the same investigator obtain identical patterns if he uses different starting materials, or if he employs slightly different experimental conditions.

Besides structural variations, the CSH (I) hydrates exhibit a wide range of specific surface areas. The samples prepared in our laboratory had surface areas between 135 and 380 m^2/g. Such differences in structure and surface area are, doubtless, responsible for the differences between the two curves of Figure 1 and for the greater differences obtained by earlier investigators [18]. Two preparations of a hydrate of a given composition with differences in structure and specific surface area would have different free energy contents and solubilities. Thus, it may happen, as Figure 1 shows, that Taylor's tobermorite, with a CaO/SiO_2 ratio of 1.1, is in equilibrium with a calcium ion concentration of about 3 millimoles per liter, whereas Kalousek's is in equilibrium with a concentration of about 6 millimoles per liter.

5. Figures 2a to 2e are electron micrographs of CSH (I) hydrates prepared by the reaction of lime and silica gel. The reaction products were suspended in an organic liquid and the suspension was subjected to ultrasonic vibrations for a short time to disperse the particles. Figure 2a is a micrograph of a hydrate with CaO/SiO_2 ratio of 0.98—all particles are very thin crumpled sheets, frequently showing rolling up at the edges. As the CaO/SiO_2 ratio in the CSH (I) hydrate increases, one observes fibrous structures. Figures 2b to 2d are micrographs of a hydrate with CaO/SiO_2 ratio of 1.29. Particles of different morphologies can be detected: Figure 2b shows crumpled sheets, Figures 2c and 2d show the types of fibrous structure that we shall observe later in the hydration products of Ca_3SiO_5 and Ca_2SiO_4. Figure 2e is a micrograph of a hydrate with CaO/SiO_5 ratio of 1.44; here the particles are predominantly fibers. The fibers may possibly be rolled up sheets, but there is no clear evidence of this in the micrographs of CSH (I) hydrates.

The magnification is indicated in each micrograph. The length of the line is 0.5μ, except in Figure 2f, in which it is 2μ.

6. Figures 2f and 2g show electron micrographs of dicalcium silicate hydrated in excess water, with a water to solid ratio of 9.0. The slurry was placed into a polyethylene bottle, and the bottle was rotated on a wheel for more than two years. Almost all of the preparation was dispersed by ultrasonic vibrations, and all dispersed material consisted of such cigar-shaped particles. The particles show some resemblance to those seen in Figure 2d.

The hydrate that exhibits the cigar-shaped particles is a member of another tobermorite subgroup, the CSH (II) subgroup. The CSH (I) subgroup is intermediate in properties between the natural minerals and the CSH (II) subgroup; the CSH (II) subgroup is intermediate in properties between the CSH (I) subgroup and the tobermorite (G) subgroup. Hydrates of the CSH (II) subgroup were first reported by Taylor [17], and Grudemo obtained electron micrographs for Taylor's hydrates similar to those shown in Figures 2f and 2g [20].

The X-ray diffraction patterns of CSH (II) hydrates are quite similar to those of the CSH (I) hydrates, though there are some differences [12]. Both subgroups exhibit many diffraction lines, whereas tobermorite (G) hydrates always show only three diffraction lines. On the other hand, in composition CSH (II) resembles tobermorite (G); the CaO/SiO_2 ratios obtained in this laboratory for CSH (II) hydrates range from 1.50 to 1.75.

The Tobermorite (G) Subgroup

1. The tobermorite (G) hydrates are very poorly crystallized. Their X-ray diffraction patterns consist of three lines: a very strong and very broad reflection, with a maximum at 3.05 Å, and two much weaker broad lines at 2.79 and 1.82 Å. These spacings were obtained on samples dried at a vapor pressure of 5×10^{-4} mm ("D-drying"). Heller and Taylor [22] found that these spacings have indices of (110), (200), and (020), respectively, and they reported the same spacings at 3.07, 2.81, and 1.83 Å for less strongly dried CSH (I) samples prepared hydrothermally.

The three lines of tobermorite (G) hydrates correspond to three of the strongest lines of all tobermorites. All three are (hk0) spacings, i.e., spacings within the layers. The (020) spacing represents half the distance between neighboring Si atoms in the chains of the SiO_4 tetrahedra of the 2:1 layers described earlier. Twice this distance, 3.64 Å, is the *b* length of the orthorhombic pseudocell. Twice the (200) spacing, 5.59 Å, is the *a* length of the pseudocell. However, tobermorite (G) exhibits no basal spacing, so the *c* length can be obtained only by indirect means.

The density of D-dried tobermorite (G) preparations, having the formula $Ca_3Si_2O_7 \cdot 2H_2O$, was found to be 2.86 g/cc [21]. From the molecular weight and the density, one obtains the volume of a molecule. Knowing the *a* and *b* lengths, the *c* length can be calculated for a pseudocell containing one molecule, and it is 9.3 Å. Numerous investigators found this value in X-ray patterns of very strongly dried CSH (I) hydrates; it appears to be the distance of closest approach of two layers.

The tobermorite (G) preparations discussed above, when dried at 8×10^{-3} mm pressure ("P-drying"), have the formula $Ca_3Si_2O_7 \cdot 2 \cdot 8H_2O$ and the density of 2.73 g/cc. If one assumes that the 0.3 mole of adsorbed water in this hydrate has the density of liquid water, then the density of $Ca_3Si_2O_7 \cdot 2 \cdot 5H_2O$ is 2.76 g/cc. This gives an interlayer distance of 9.9 Å. The extrapolated density of the $Ca_3Si_2O_7 \cdot 3H_2O$ of equation (1) is 2.70 g/cc, which gives a *c* spacing of 10.3 Å. Although the tobermorite (G) hydrates do not exhibit basal spacings, the closely allied CSH (II) hydrates do, and the reported values range between 9.8 and 10.5 Å (12)(24).

Even the three broad diffraction lines of the tobermorite (G) hydrates do not represent the lowest limit in the degree of crystallinity of tober-

morites. When tricalcium silicate is hydrated in a small steel ball-mill, an unstable tobermorite is obtained which exhibits only a single broad diffraction band with the maximum in the vicinity of 3 Å [23].

Gard, Howison and Taylor [24] have advanced a hypothesis for the structures of high-lime tobermorites (tobermorite (G) and CSH (II)), according to which the basic layer is not the triple (2:1) layer of Megaw and Kelsey [13] but a double (1:1) layer, one of which is a lime, the other is a silica layer. Our results disagree with this hypothesis. However, Gard, Howison, and Taylor rather suggested than developed their model, so it is possible that a more complete development of the hypothesis may change this conclusion.

2. From the considerations of the previous section we can conclude that in a tobermorite (G) hydrate of composition $Ca_3Si_2O_7 \cdot 3H_2O$, one molecule of water is between the layers. As the vapor pressure of water is reduced, the interlayer water is lost continuously. On P-drying, about half of this water is lost, on D-drying, all of it. The calculated interlayer distance after D-drying is 9.3 Å. It was found in this laboratory [21] that the hydrate dried to this stage, even after soaking in water for a day, did not swell. Megaw and Kelsey [13] proposed that in a strongly dried tobermorite, showing a c spacing of 9.6 Å, the silicate chains in one layer fitted into the grooves between those in the next adjacent layer. This would explain the strong attraction between adjacent layers, leading to the exclusion of water. Tobermorite (G), subjected to P-drying, showed some swelling upon immersion in water, but the swelling was slow and slight.

After D-drying, the molecule contains only the hydroxylic water attached to the SiO_4 tetrahedra in the layers, and this water is much more firmly held than the interlayer water. When the above D-dried sample, having the formula $Ca_3Si_2O_7 \cdot 2H_2O$, was further dried over P_2O_5 for four weeks to constant weight, only 5% of the water was lost. Thus, at a vapor pressure of the order of $1/1000$ of that used in D-drying, very little of the layer water was lost.

It will be seen in the next section that tobermorite (G) is very finely divided in the c direction. This dimension of the crystallites is colloidal—it even verges on the molecular. On the other hand, the dimension of the particles in the b direction, i.e., along the silicate chains, is quite large, it is of the order of a micron or several microns. Thus, speaking with some license, tobermorite (G) is a colloid in the c direction but not a colloid in the b direction. It is interesting that interlayer water is lost continuously, as water is usually lost by colloids, but the water content of the silicate chains remains almost constant over a wide range of vapor pressures.

3. The electron micrographs of tobermorite (G) hydrates show, predominantly, long straight fibers, rather like the ones shown in Figure 2c.

Figure 2h, obtained from tricalcium silicate hydrated for eight years, shows such fibers, as well as broken fragments of calcium hydroxide crystallites. It also shows some undispersed matter.

Figure 2i, obtained from the same paste, indicates that the fibers are actually *rolled up sheets*, and this is seen more clearly in Figures 2j and 2k, obtained from dicalcium silicate pastes. At places where the sheets are unrolled, one can see that their thickness is of the order of magnitude of a unit cell. The fibers are long, most of them longer than 1μ.

Figure 2l shows an aggregation of fibers, obtained from a tricalcium silicate paste. The ultrasonic vibrations, in this case, have only separated the fibers slightly but did not disperse them. The aggregation looks like a pile of matches, with the alternate rows crossing each other.

4. The adhesion of the tobermorite (G) particles to each other and to other bodies is responsible for the strengths of hardened pastes of tricalcium and dicalcium silicates; it is also the most important factor in the strengths of hardened portland cement pastes and concrete.

The compressive strengths of hardened portland cement pastes exceed their tensile strengths by an order of magnitude. The compressive strengths of hardened calcium silicate pastes are about the same as those of hardened portland cement pastes. Although no data have been published on the tensile strengths of calcium silicate pastes, it seems likely that they are lower than those of portland cement pastes because calcium silicate pastes are dispersed easily by ultrasonic vibrations, whereas portland cement pastes are not.

The great discrepancy between compressive and tensile strengths can be understood on the basis of the structure indicated by Figure 2l. To produce failure in compression, one must work against the forces within the tobermorite crystallites, i.e., against valence forces—though, of course, one is aided by the imperfections within the crystallites. On the other hand, to produce failure in tension, one merely has to overcome the adhesion between the tobermorite particles, so one works against van der Waals forces.

5. The hardened pastes of calcium silicates and portland cements are porous bodies, and changes in relative humidity cause movement of water into or out of the pore system. Entry of water results in swelling and removal of water in shrinkage of the paste. When concrete loses water, it shrinks, and this may produce cracking. Among all improvements in concrete that research may bring about, the one most desired by the construction industry is the elimination—or, at least, a great diminution—of shrinkage. Tobermorite gel is the most important factor in the shrinkage of concrete, just as it is the most important factor in the strength of concrete.

Figure 3 represents three different mechanisms by which tobermorite (G) may swell upon entry of water (or shrink upon removal of water).

On the left side, the tobermorite is considered as a sheet, prior to rolling up into a fiber. It will be shown later that all stable tobermorite sheets are either two or three molecular layers thick. Water can enter between these layers, as was discussed before, with the result that the distance between the layers increases. Thus, entry of water produces a swelling of the sheet.

In the middle of Figure 3, tobermorite is considered as a fiber, and the cross section of a two-layer tobermorite is shown. Water can enter between the layers at X, which is the mechanism already discussed, but it can also enter at Y into the convolutions of the fiber. Water, in this case,

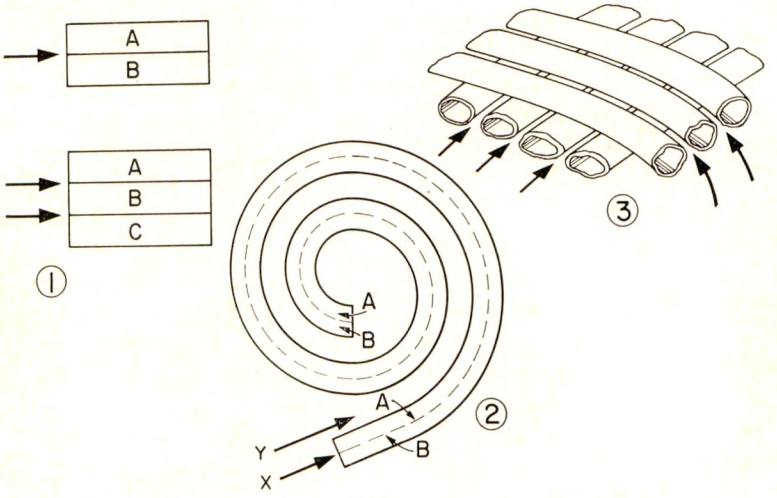


FIG. 3. Three mechanisms of expansion of tobermorite gel.

is adsorbed on the surface of the tobermorite in several layers, and this results in the swelling of the fiber.

On the right side of Figure 3, tobermorite is considered as an aggregation of fibers. This is an idealized drawing of the aggregation of fibers shown in Figure 21. Water can enter between the fibers and separate the fibers from each other. This results in a swelling of the aggregation of fibers.

Even the above three mechanisms do not exhaust all possibilities of swelling and shrinkage. Water can enter not only into the tiny pore spaces between the individual fibers but also into the much larger pore spaces that exist between aggregations of fibers. The pores between the gel particles are called gel pores, those between aggregations of gel particles are called capillary pores. Entry of water into the capillary pores also produces swelling of the hardened paste; in fact, at high relative humidities, this is probably the most important factor in dimensional changes.

The Surface of Tobermorite (G)

1. Powers and Brownyard [25] were the first to measure the specific surface areas of hardened portland cement pastes, using the B.E.T. method [26] with water vapor as the adsorbate. They were able to demonstrate that surface area and porosity of the paste play decisive roles in determining the most important engineering properties of paste: strength, dimensional changes, permeability to water, and freezing of water in the pore system. Subsequent work in this laboratory has shown that about 75% of the colloidal fraction of hardened portland cement paste (the so-called "cement gel") is tobermorite gel, and about 80% of the specific surface of hardened portland cement paste is tobermorite gel surface.

Work on the pore size distribution of hardened pastes of portland cements and calcium silicates is still in its early stages; it will not be reported, therefore, in the present paper. However, much work has been done on the surfaces of these pastes. It has been established that the specific surface areas and the compositions of the tobermorite gel in pastes of portland cements are the same as in pastes of the calcium silicates.

2. If the rolling of the tobermorite (G) sheets does not create spaces into which water cannot penetrate, the area measured by water adsorption for the fibers should be the same as that of the sheets prior to rolling. This area could be calculated from crystal structure data, if the thickness of the sheets were known.

Because the thickness is very small, compared to the two other dimensions of the sheet, practically all of the surface area resides in the two sides of the sheet. The a and b dimensions of the orthorhombic pseudocell of tobermorite (G) are 5.59 and 3.64 Å, respectively. If one places one molecule of composition $Ca_3Si_2O_7 \cdot 2H_2O$ (D-dried tobermorite (G)) into the pseudocell and assumes that the sheet has the thickness of a single pseudocell, calculation gives a specific surface area of 755 m^2/g. If the sheet has the thickness of two pseudocells (two molecular layers), the specific surface area is 377 m^2/g, and if the thickness is three molecular layers, it is 252 m^2/g. In these calculations, the areas along the edges of the sheets have been neglected.

In a series of experiments [27], in which we determined the surface energy of tobermorite (G), we made fourteen preparations, using different starting materials (Ca_3SiO_5 and Ca_2SiO_4), different water to solid ratios (0.7 and 9.0), and three different methods of preparation. The maximum surface area obtained was 376 m^2/g, the minimum was 245 m^2/g. From this we concluded that all tobermorite (G) particles were either two or three molecular layers thick. At the time of the writing of this paper, more than 100 tobermorite preparations have been examined. In the early stages of preparation of the calcium silicates, we detected single-layer tober-

ites, as well as tobermorites that were more than three layers thick. In these early stages of hydration, the composition of the tobermorite went through changes, as I stated earlier. However, by the time the tobermorite reached its final and stable composition, the surface area also became stabilized, and in every instance it indicated two and three-layer tobermorites. The surface areas of the pastes obtained from the hydration of Ca_3SiO_5 indicated a predominance of two-layer tobermorites, those from Ca_2SiO_4, three-layer tobermorites.

3. Recent work [11] has established a relationship between the composition and the specific surface area of tobermorite (G). The specific surface areas of stable tobermorites were plotted against the CaO/SiO_2 ratios, and a straight line was obtained. The equation of the least squares straight line, based on 44 pastes, was

$$A = 865.0 \pm 42.9 - (354.7 \pm 39.9)(CaO/SiO_2) \qquad (3)$$

where A is the specific surface area in m^2/g, and (CaO/SiO_2) is the molar CaO/SiO_2 ratio. This empirical equation indicates that the more lime there is in the tobermorite the lower the specific surface, which in turn indicates that increase in the lime content results in a larger fraction of three-layer and a smaller fraction of two-layer sheets. It has been suggested that lime has the ability to cement neighboring crystallites of tobermorite together [24]. Equation (3) indicates that lime can cement the layers of tobermorite, within the crystallite, to each other.

The slope of the line shows that a unit increase in the molar CaO/SiO_2 ratio produces a decrease of 354.7 m^2/g in the surface area. The decrease per molecule of tobermorite (G) is 19.4 $Å^2$, if the calculation is made for the average composition, i.e., for a CaO/SiO_2 ratio of 1.55. The area contributed by a pseudocell is $a\,b$, which is 20.35 $Å^2$. The 19.4 $Å^2$ value calculated differs from this by less than 5 per cent, whereas the 19.4 $Å^2$ value itself has a standard error of 11 per cent, as equation (3) shows. Thus, it appears that a unit increase in the molar CaO/SiO_2 ratio i accompanied by the disappearance of a unit surface, the surface contrib uted by a pseudocell.

The above calculations are based on a model in which the pseudoc always contains one molecule of tobermorite (G), regardless of comp sition, and the molecule always contains 2 atoms of silicon. This mea that the weight of the pseudocell increases as the CaO/SiO_2 ratio creases; for example, an increase in the CaO/SiO_2 ratio from 1.50 to means the addition of 0.10 molecule of CaO and 0.10 molecule of to the pseudocell. This, naturally, should be understood in the ave sense, i.e., one molecule of lime and one molecule of water is add one pseudocell out of ten.

The model leads to the structures of two and three-layer tober sheets shown in Figure 4. The CaO/SiO_2 ratios of the two and

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layer sheets are 1.39 and 1.72, respectively, and the lowest and highest value obtained in this laboratory were 1.39 and 1.75. The specific surface areas calculated on the basis of this model for the two and three-layer sheets are 387 and 240 m^2/g, respectively, and the highest and lowest values obtained were 391 and 244 m^2/g. Equation (3) leads to specific surface areas of 372 and 255 m^2/g for CaO/SiO_2 ratios of 1.39 and 1.72, respectively; these values differ by 3.9 and 6.2 per cent from the calculated values, which differences are well within the standard error of the curve.

The modified Taylor-Howison model [8] gives a good description of the CSH (I) subgroup of tobermorites, as was discussed earlier. That model leads to a constant pseudocell weight, in contrast with the model discussed above. If the Taylor-Howison model is valid also for the tobermorite (G) subgroup, the structures of the two and three-layer sheets are somewhat different from those shown in Figure 4. The differences are slight for the three-layer sheet: the surface contains 0.75 molecules

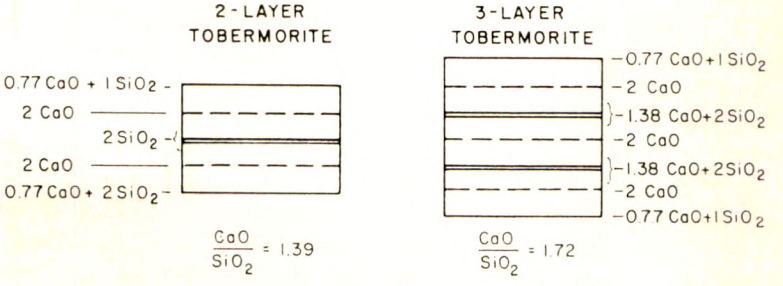


FIG. 4. Two and three-layer tobermorites.

of CaO instead of 0.77, and the CaO content of the layers between the pseudocells is 1.50 molecules instead of 1.38. The differences are greater for the two-layer sheet: the surface contains 0.60 molecules of CaO and 1.14 molecules of SiO_2 instead of 0.77 and 1.00 as in Figure 4, and the layers between the pseudocells contain 1.21 molecules of CaO and 2.29 molecules of SiO_2 instead of no lime and 2.00 molecules of SiO_2 as in Figure 4. Experiments are in progress to ascertain which of the two models is correct.

4. Earlier I stated that we used fourteen calcium silicate pastes for the determination of the surface energy of tobermorite (G). Surface energy is an abbreviation for surface total energy, and it is defined as the difference between the total energy of a molecule of a given substance in the surface layer and the total energy of the same molecule inside the body of that substance. If samples of a substance are obtained in different states of subdivision, and the heats of solution of these samples are plotted against their specific surface areas, the plot is a straight line, the

slope of which is the surface energy. Such a plot is shown in Figure 5 for the tobermorite (G) preparations. The slope of the least squares straight line gives a surface energy of 386 ± 20 ergs/cm^2.

Prior to this work, we determined the surface energies of calcium hydroxide [28] and hydrous amorphous silica [29], and obtained the values of 1180 and 129 ergs/cm^2. One would expect that the surface energy of tobermorite (G) is between these values for two reasons. Speaking with some license, the surface of the calcium silicate hydrate is a sort of "chemical mixture" between the surfaces of calcium hydroxide and

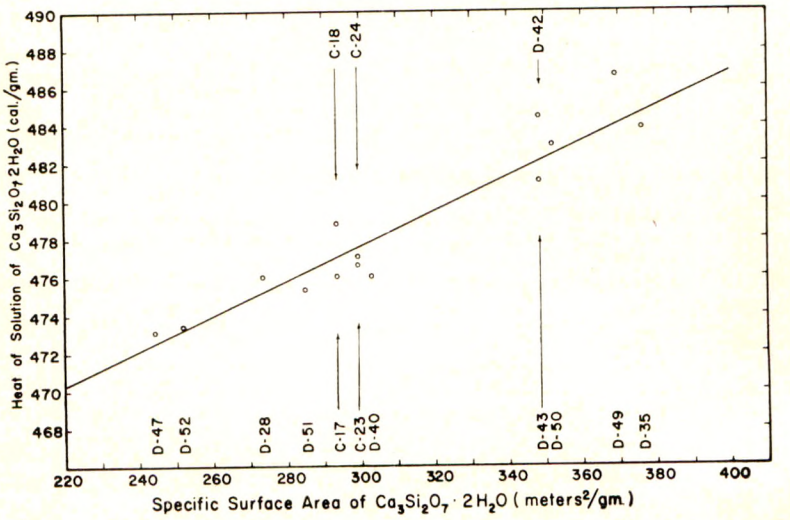


FIG. 5. Variation of the heat of solution of $Ca_3Si_2O_7 \cdot 2H_2O$ with specific surface area.

hydrous silica. In addition, the calcium hydroxide used in the surface energy determination contained almost perfect crystals, the hydrous silica was amorphous, and the calcium silicate hydrate was intermediate between the two, it was poorly crystallized. The surface energy of tobermorite (G) is almost exactly the geometric mean between the surface energies of calcium hydroxide and hydrous amorphous silica.

The point in Figure 5 which falls farthest away from the straight line is the one obtained for the preparation designated as D-49, and it is 2.6 cal/g too high. In all probability, this is a random error, but let us assume that it is not. Let us assume first that all of this difference is caused by some variation in the body structure, such as, for example, differences in crystal defects. The 2.6 cal/g amounts only to 0.58% of the heat of solution. Let us assume next that none of this difference is caused by variation in the body structure, but all of it is caused by variation in the surface structure. This amounts only to 7.6% of the surface energy of

tobermorite (G). These figures refer to the preparation showing the maximum deviation. If the calculations were made for the average deviation, which is 1.1 cal/g, the results would be less than half of the values given. One can conclude, therefore, that both the body structure and the surface structure of tobermorite (G) are highly reproducible.

ACKNOWLEDGMENT

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A SURVEY OF EARLY AMERICAN PREHISTORY

By H. M. WORMINGTON

IT HAS LONG been recognized that man did not originate in the Western Hemisphere, and that it is in the Old World that the ancestors of the American Indian must be sought. It is, however, only within the last thirty-five years that there has been general agreement that man reached the New World during the Pleistocene period. Not until 1926 were objects indubitably made by man found in clear association with articulated bones of extinct animals. In that year, at a site near Folsom, New Mexico, a finely flaked projectile point was found lying between the ribs of a bison of an extinct species (Cook, 1927; Figgins, 1927). Since then tremendous progress has been made in the study of the first Americans.

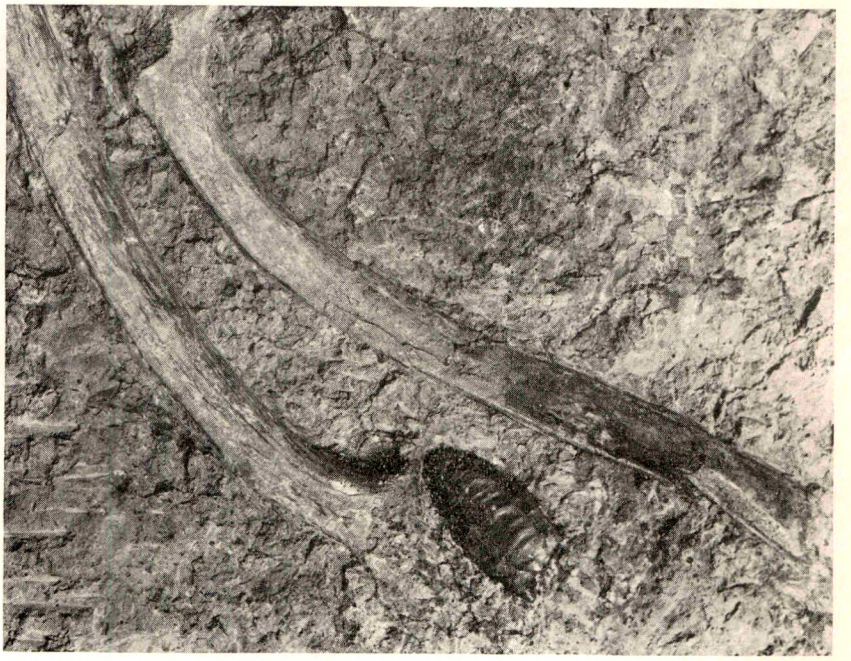
There are still many questions for which we have no answers, but as more data become available regarding the pre-ceramic cultures of South America as well as North America, as we learn more about the prehistory of Siberia, and as more radiocarbon dates are obtained, it becomes possible to see the gradual emergence of a general pattern. One thing that has become increasingly clear is that problems cannot be solved by thinking in terms of a single continent, or even of one hemisphere.

Evidence for an Asiatic origin of America's native population is provided by the appearance of the American Indians, for they closely resemble Asiatics of the Mongoloid race. It is a reasonable assumption that, when man first entered the Western Hemisphere, watercraft capable of traversing large bodies of water was not available, and that entry must have been by way of Bering Strait. Since Miocene times, Asia and North America have constituted a single land mass only separated at brief intervals by submergence at the Strait. In the past there have been a number of periods when the continents have been linked, not by a small bridge, but by a broad plain, some thousand miles in width, that presented no major physical barriers.

We cannot say with any degree of certainty when men first entered the New World, but various lines of evidence show that it must have been many millennia ago. There can be no doubt that the first migration occurred considerably more than 11,000 years ago, for by approximately 9000 B.C. men had reached the very tip of South America and by at least 8000 B.C. different cultural traditions, well-adapted to regional environmental conditions, were fully established in different parts of the Americas. To complete such a trek and for such variations to arise must have taken time, and a date for the entry of the ancestors of these people of the general magnitude of some 15,000 to 20,000 years ago would seem reasonable to the majority of New World prehistorians.

There are tantalizing clues that suggest a much greater antiquity for New World man, but none of these putatively early finds has won universal acceptance. Two localities that have been dated by the radiocarbon method are of particular interest. At Tule Springs, Nevada, a discoidal scraper was found in a deposit that yielded charcoal and bones of Pleistocene animals (Harrington and Simpson, 1961). A radiocarbon assay of charcoal mixed with earth indicated an age in excess of 28,000 years ago (26,000 B.C.). Additional work at this locality would be highly desirable.

At a site near Lewisville, Texas, twenty-one hearths, fossil bones, charred hackberry seeds, and a crude chopper and a scraper gave evi-



Courtesy of the Denver Museum of Natural History.

FIG. 1. Folsom point and associated bison ribs imbedded in matrix, Type Station, Folsom, New Mexico.

dence of human occupation (Crook and Harris, 1957). Several assays of charcoal from a hearth provided an age determination in excess of 37,000 years ago (35,000 B.C.). This date would have been more readily accepted had not a Clovis fluted point been found imbedded in the burned red clay of the hearth. Evidence from other sites indicates that such points came into use at a much later period, and most archaeologists are convinced that a date in excess of 35,000 B.C. cannot be valid for the point. There is some reason to believe that the introduction of the point was a hoax, somewhat in the Piltdown tradition, perpetrated by an

individual unconnected with the excavations, which were conducted by persons of complete integrity. This is extremely unfortunate, for the radiocarbon assay may very well date the hearth from which the sample was taken, yet the presence of the fluted point has cast certain doubts on the site.

There are also finds in Mexico, notably at Tequixquiac and in the Valley of Puebla, where faunal associations suggest great age, but further investigations are required. In the Altiplano of Bolivia there are reports of localities on high terraces above dry lakes attributed to the Pleistocene that have produced surface assemblages of crudely flaked uniface artifacts. It has been suggested that these may be of Lower Palaeolithic age. These may be of considerable age but not necessarily of such antiquity as this, and intensive geological studies would be desirable. New evidence obtained from a site in Wyoming indicates that estimates of age based on typological considerations must be approached with great caution since hand-axe-like forms and flake tools strongly reminiscent of Mousterian forms were being produced in the United States in the 8th millenium B.C. In western Europe the last of the Mousterian people lived about 35,000 to 40,000 years ago (Bordes, 1961).

Should there ultimately be proof that man has been in the New World for more than 35,000 years, we must admit that we know nothing of his ancestors in eastern Siberia, for there no sites of such age have been found. In so vast an area, however, it is always possible that sites older than any now known will be uncovered at some future date.

There are also questions that arise about the great gap between these very early dates and the later dates which are firmly established, with no evidence of occupation in the intermediate period. Perhaps, here too, it is simply a question of not having found the evidence. One might also consider the possibility that only a very small population movement was involved in the first penetration, and that it had little influence on later developments, or even that the original group became extinct. This would accord with the belief of many physical anthropologists that, despite certain variations, there is a basic homogeneity, both phenotypically and genotypically, among American Indians; that only modern man contributed to the population, and that genetic changes are essentially the product of selection and drift in a huge cul-de-sac that provided every variety of environment (Stewart, 1960).

In any case, when we begin to consider the American aborigines who were well-established in many parts of the New World by 9000 or 10,000 B.C., we are on relatively firm ground. At an earlier date there were present in eastern Siberia people accustomed to periglacial conditions and life on the tundra, capable of hunting big game, and equipped to travel through the Arctic. We have so little information about the physical types of early people on both sides of the Strait that little can be done

along this line of inquiry at the present time, and we are almost entirely dependent on studies of lithic materials.

Many archaeologists have made the statement that there is a surprising and disappointing lack of parallels between the Siberian Palaeolithic and Paleo-Indian cultures of the New World. The writer does not believe that this is quite true. There are a number of reasons for this impression. First, we have looked for virtual duplication of highly evolved New World types of projectile points instead of considering the prototypes from which they might have developed. We have also been unduly impressed by claims that early New World projectile points were pressure flaked, and this technique did not come into use in Siberia until late. More recent studies suggest that the earliest American types were produced by well-controlled percussion and that pressure flaking was a later indigenous development. Also, we have failed to consider that a trait that was quite unimportant in the parent area—in this case, bifacial flaking in eastern Siberia—could, in the course of time, gain increased importance in another area, as this trait did in the New World. Furthermore, we have tended to place far too great an emphasis on this particular trait due to the fact that in many cases we have had only a very limited knowledge of Paleo-Indian tools other than projectile points, because of the finding of kill sites rather than habitation sites.

In seeking to appraise developments in the Western Hemisphere during the period that began in the 9th to 10th millenium B.C., and trying to determine what happened during the period that preceded it, we must consider what may have been available for export from Siberia at an earlier date. Recent investigations in the Cave of Ust Kanskaiya in the Charysh River area of the Altai have demonstrated the presence in Western Siberia of a very early complex that included forms reminiscent of Middle Palaeolithic types, others of Upper Palaeolithic aspect, and bifacial flaking (Rudenko, 1960). These elements presumably spread to Eastern Siberia where they contributed to the Palaeolithic of that area. There, the Palaeolithic is essentially a hybrid culture for it also contains some East Gravettian elements and shows evidence of influences from the chopper-chopping tool tradition of eastern Asia.

Not until much later did bifacial flaking become important in the lithic industries of eastern Siberia, but it was present during what is called the Palaeolithic I stage in that area, as is indicated by the presence of large bifacially flaked laurel leaves in the Irkutsk Military Hospital site, attributed to this stage, and regarded as older than the sites of Malta and Buryet, which are tentatively dated at around 20,000 B.C. Also, although the Verkhholenskaiya Gora site is considered the type station for the post-glacial Stage III of the eastern Siberian Palaeolithic, it is possible that the bifacially flaked leaf-shaped points and some scrapers and broad blades found there may be considerably older. The site was

not clearly stratified and many of the implements were surface finds. Some specimens may be of the same age as the rhinoceros bones found at the site.

In the Maritime Provinces of the Soviet Far East, leaf-shaped points or knives are common and there are chopping tools of a distinctive type. These points are extraordinarily like the doubly pointed forms that are found in early pre-ceramic complexes from Alaska to South America. Guess dates suggest an age of no more than 6000 to 8000 B.C. These estimates may be modified when radiocarbon dates become available.

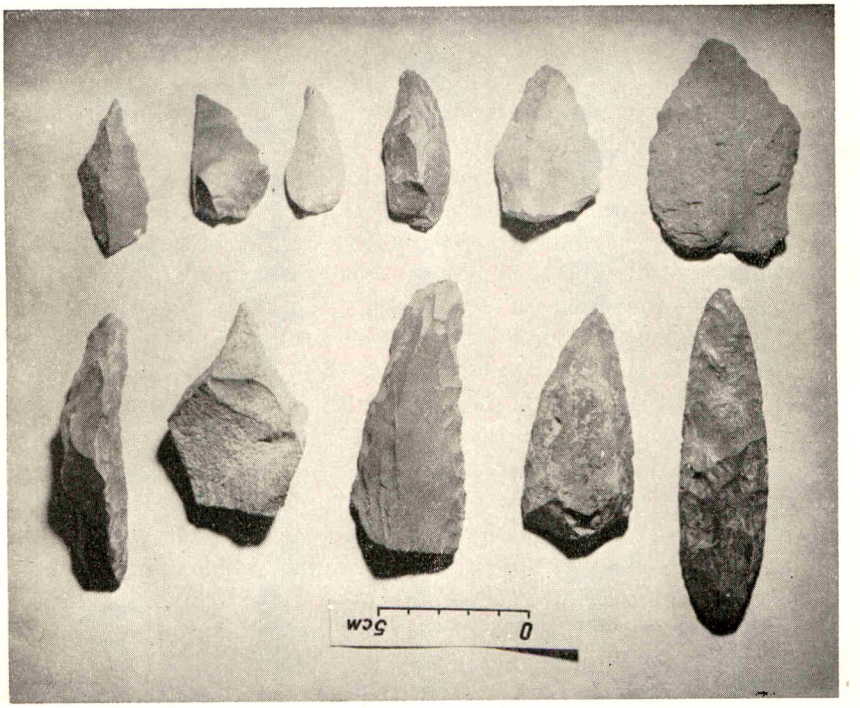


FIG. 2. Artifacts from Ust-Kanskaia Cave near Biisk in western Siberia.

It is also possible that there are earlier sites, containing the prototypes of these, which have not yet been found. The leaf-shaped form persisted here for a long time, for it is also found in sites with ceramics.

On the basis of the Siberian evidence it would appear that the early migrants could have had chopping tools, flake implements that resembled those of the Mousterian culture, keeled and discoidal scrapers, somewhat crude blades, and some bifacially flaked leaf-shaped forms, although the latter would have been of minimal importance in the assemblage. The evidence from early American sites indicates that these very elements were present, although the bifacially flaked implements became

much more important there, and the forms were greatly elaborated.

Percussion-flaked bifacial leaf-shaped points and types that represent a relatively minor modification of this form have an extraordinarily wide distribution in the New World and a very great time range. Among the earliest are Sandia points, found in a cave of that name (Hibben, 1941) in New Mexico. They differ largely from the probable Siberian prototype by the removal of a few flakes to form a single shoulder. A somewhat similar type, but more highly modified with slight stemming on both sides and a fairly straight base, has been found at the Hell Gap site in Wyoming in an horizon with a radiocarbon date of 8890 B.C. ± 550 (Agogino, 1961). Interestingly enough this horizon also yielded crude hand axes, and discoidal scrapers and flake implements that suggest the use of Mousterian techniques. The excavation of this site has not been

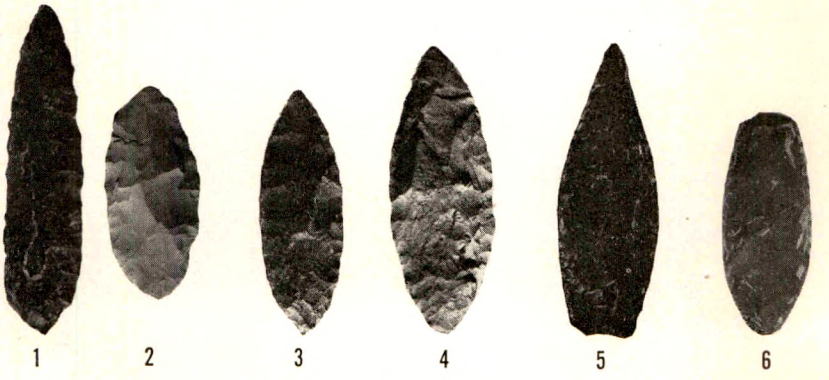
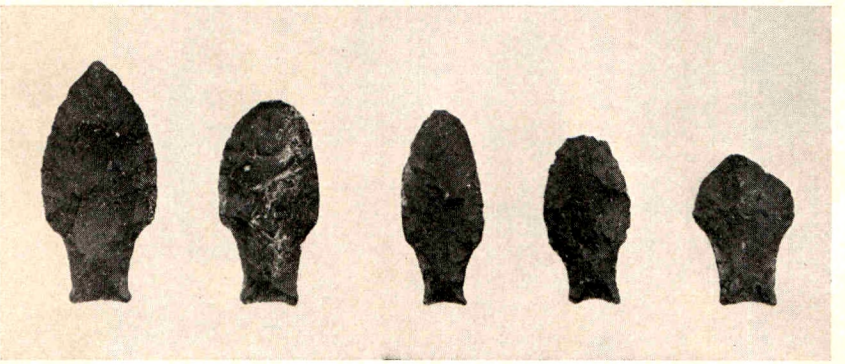


FIG. 3. Leaf-shaped points from Siberia and America. 1 and 2 Osipovka Site, near Khabarovsk, U.S.S.R. (Photograph courtesy of Institute of Archaeology, Leningrad), 3 and 4 from DjRi3 Site, Fraser Canyon, British Columbia. (Photograph courtesy of University of British Columbia), 5 and 6 from site near Santa Isabel Iztapan, Mexico. (Photograph courtesy of Direccion de Prehistoria, Mexico, D.F.)

completed and no statistical evaluation is possible, but it is already evident that uniface implements greatly outnumber the bifacially flaked points.

Farther to the south, bi-pointed leaf-shaped points have been found in a number of early sites, and in South America they are the predominant form in pre-ceramic contexts. There are certain differences in proportion, thickness, type of flaking, etc., but they do seem to represent variations on a single basic theme. Two leaf-shape points were found in clear association with mammoth in the Valley of Mexico (Aveleyra, 1955) and, in a cave in Tamaulipas, similar specimens were found in an horizon with a radiocarbon date of 7315 B.C. (MacNeish, 1958). A still earlier horizon produced pebble tools very like those of Siberia and other parts of eastern Asia.

At the site of La Vela de Coro in western Venezuela, artifacts have been found together with bones of mastodon, glyptodon, and other extinct animals with a radiocarbon date of about 14,000 B.C. In the better known El Jobo culture of the same area, which is regarded as more recent but still of considerable antiquity, are many biface leaf-shaped points and knives, as well as keeled scrapers, very like Siberian types, scrapers reminiscent of Mousterian forms, crude blades, and hand-axe-bifaces, some of which are of considerable size (Cruxent, 1956). Laurel leaf points have also been found in pre-ceramic complexes in Peru, Argentina, Bolivia, and Uraguay. If we are correct in assuming that the leaf form was the prototype of all early New World types, there must be some of these in South America that predate the ninth millennium B.C. The stemmed points found in the lowest horizon in Fells Cave in Patagonia, which has a radiocarbon date of 8760 B.C. ± 300, represent a fairly evolved type and the ancestral form must be older (Bird, 1938).



Courtesy of the American Museum of Natural History.

FIG. 4. Projectile points from the lowest level of Fell's Cave, Patagonia.

There is another area where leaf-shaped points and modifications of this form are exceedingly common—northwestern Canada and the Pacific Northwest area of the United States. Among the best known localities is the Five Mile Rapids Site on the Columbia River in Oregon where the bi-pointed forms were in exclusive use at the time of the earliest occupation (Cressman, 1960). Radiocarbon dates suggest a beginning date of the general magnitude of 9000 B.C. Similar forms have been found at the DjRi 3 Site in the Frazer Canyon area of British Columbia in an horizon dated at 7000 B.C. (Borden, 1957).

In North America was developed one very distinctive technique, that of fluting. This is the grooving or channeling of a point through the removal of longitudinal flakes from one or both faces. There are some fluted Sandias and it may be from these that there developed the concave-

based, lanceolate, fluted points which bear the name Clovis. Clovis points have been found in association with mammoth in the Plains and in the southwestern United States. Geological evidence and radiocarbon dates suggest an age between 7000 and 11,000 B.C. (Haury, 1959).

The fluted tradition became very widespread. Many fluted points have been found in the eastern United States, but not in association with extinct fauna, and there is only one eastern fluted point site with radiocarbon dates, the Bull Brook site in Massachusetts (Byers, 1954). These indicate occupation about 7000 B.C. However, studies of distribution as related to paleogeography suggest an essentially comparable antiquity for fluted points in the east and in the west (Mason, 1962).

This technique also spread to the south. Scattered finds of fluted points, similar to Clovis, have been reported from Mexico and Central

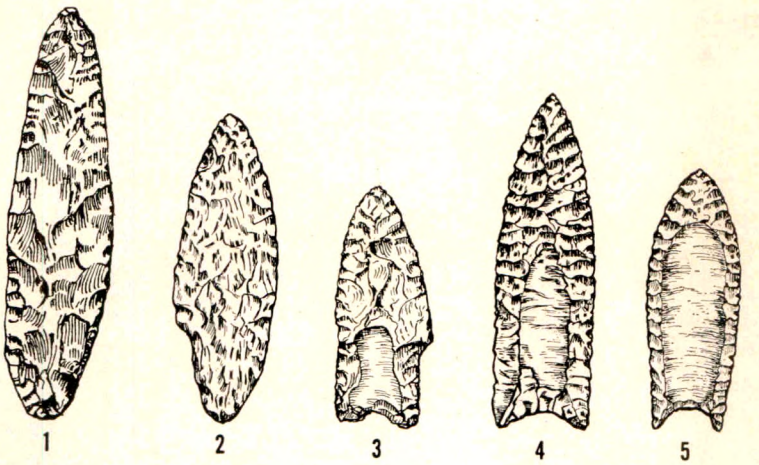
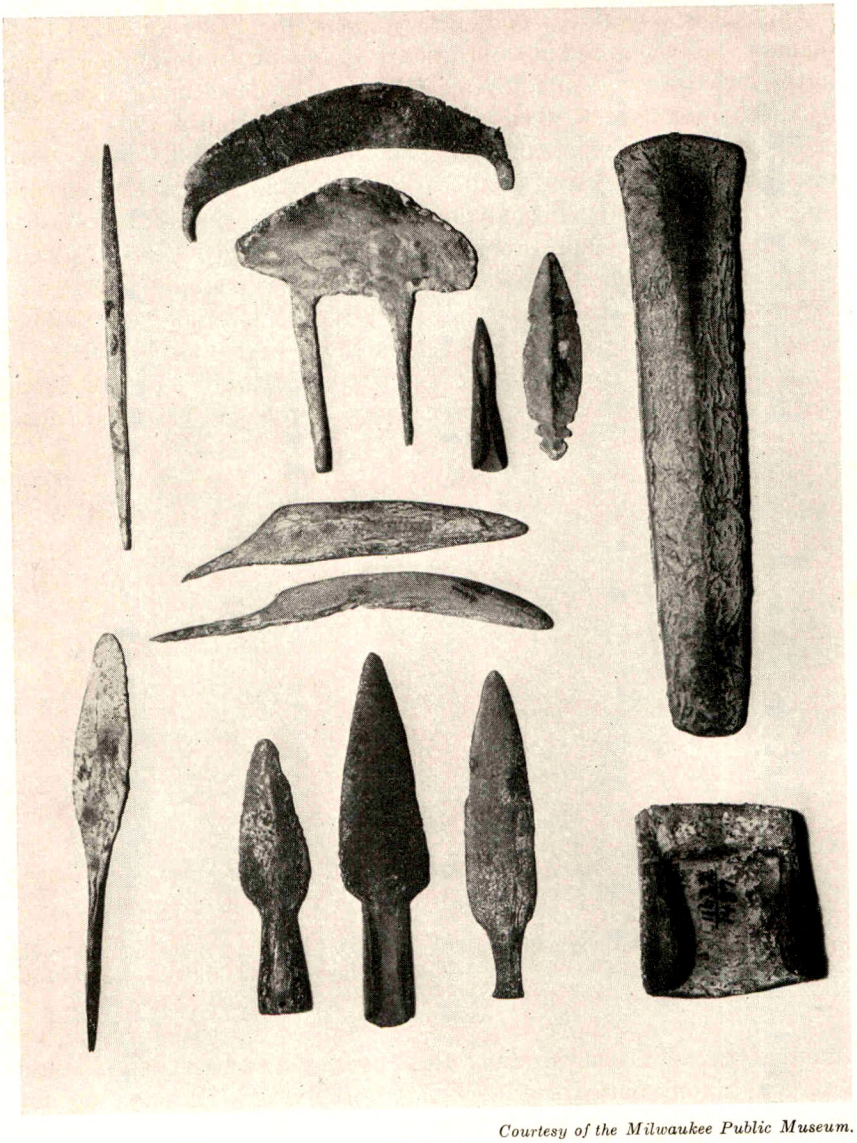


FIG. 5. Postulated sequence of projectile point types. 1. Laurel leaf from Siberia. After Rudenko 1960, Plate 18, page 122. 2. Sandia point. 3. Fluted Sandia point. 4. Clovis fluted point. 5. Folsom point.

America. In Bolivia, in localities that also produce well-shaped laurel leaf points, some fluted specimens have been recovered. They do not at all resemble Clovis points or the later fluted Folsom point, but they do show the wide distribution of the fluting technique. Some interesting finds of fluted points have been made near Quito, Ecuador (Bell, 1960). Some of these points resemble North American types, but most are stemmed, with the same shape as those from the lowest level at Fell's Cave in Patagonia, although they are definitely fluted. Here we apparently have a blending of a northern and a southern tradition.

The successors of the mammoth hunters of the Plains and the Southwest of the United States were primarily hunters of Pleistocene bison. The best known are the makers of Folsom points, which are essentially



Courtesy of the Milwaukee Public Museum.

FIG. 6. Copper implements of the Old Copper Culture, Wisconsin.

smaller and more refined versions of Clovis points. It has been said repeatedly that Paleo-Indian points were pressure flaked, but it now appears that percussion was the basic technique, although there is some evidence of a pressure retouch on the edges of Folsom points. Many of the associated tools were made on flakes and there is some suggestion of the use of an essentially Levalloiso-Mousterian technique, but there are also some blade implements. Radiocarbon dates indicate an age of

about 8000 or 9000 B.C. for the Folsom culture. Later hunters of now extinct bison in the Plains did use a pressure flaking technique in producing a variety of unfluted lanceolate point types. It would appear that the technique of pressure flaking developed independently in the New World.

To the west of the Rocky Mountains are found a number of complexes that have been grouped under the term Desert Culture (Jennings and Norbeck, 1955). There was some hunting, but there appears to have been more utilization of small game than in the Plains. Projectile points were commonly stemmed or notched and must represent indigenous development. Choppers and keeled and domed scrapers, which are virtually identical to choppers and scrapers found in Siberian Palaeolithic sites, were very common tools. Presumably these continued to be important in this area where the rigors of the environment necessitated a dependence on plant as well as animal foods. Choppers would be useful in the taking and preparation of vegetal foods and the scrapers (sometimes called scraper planes) could be used in the preparation of plant fibers. In areas where there was less dependence on gathering, these types tended to diminish in importance. The role of plants in the economy is also indicated by the presence of milling stones. Carbon 14 dates from Danger Cave in Utah, where occupation began in the ninth millenium B.C., indicate that grinding stones were in use by about 8000 B.C. and perhaps earlier (Jennings, 1957). This apparently is a product of independent invention.

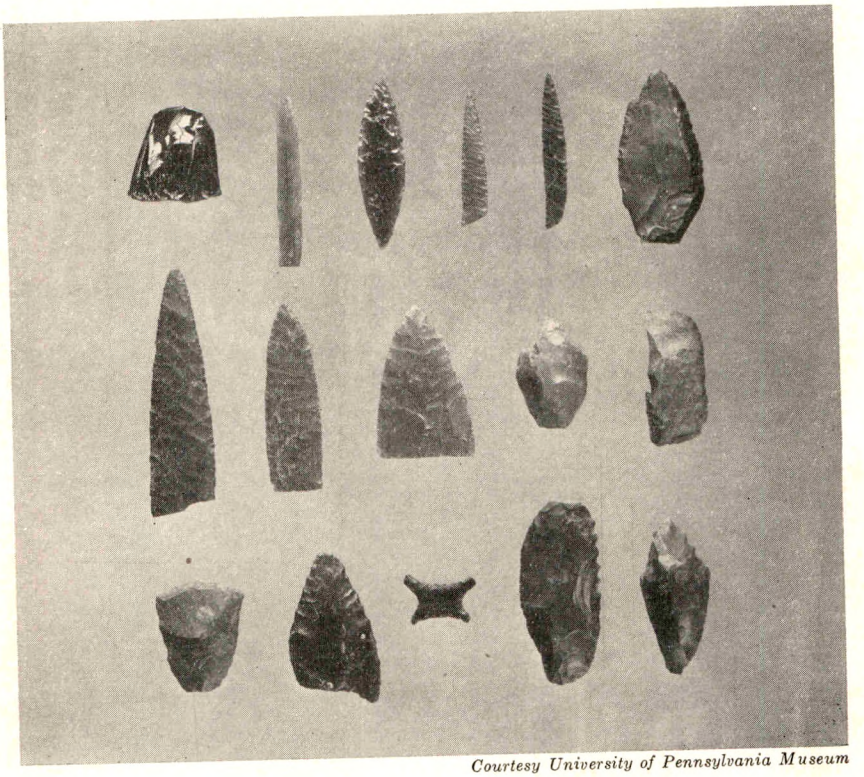
Basketry was being made here by about 7000 B.C. By the same time, woven sandals and fine twined basketry were being produced in Oregon (Cressman, 1951). The techniques employed were so highly developed that these cannot possibly be among the earliest specimens produced.

In the eastern United States have been found a variety of complexes commonly referred to an "Archaic" stage of development. Although there is very little stratigraphic evidence, it is generally assumed that the first developmental stage was characterized by fluted points, and the next stage was the Archaic. During the early Archaic, scrapers, axes and hoes were produced by flaking, and polished and ground stone implements characterize the late Archaic. Throughout, there was an emphasis on wood-working tools. Radiocarbon dates indicate Archaic occupation as early as 7000 or 8000 B.C. in Alabama, Illinois, and Missouri.

One of the most interesting Archaic manifestations is found in the Great Lakes area. About 5000 B.C. there were Indians there who mined copper and, by processes of cold hammering and annealing, produced beautiful tools and implements of metal (Ritzenthaler, Editor, 1957). Copper was not used in Siberia until a much later period, and this must be purely the result of independent invention in America.

Regrettably, Arctic regions, through which migrants from Asia would have had to pass, have yielded no very early sites. Among the earliest complexes is one from the Firth River in the MacKenzie drainage, which is called British Mountain (MacNeish, 1956, 1959). A tentative estimate of age is about 7000 B.C. Among the artifacts found were crude chopping tools made from pebbles, scrapers made from flakes, a very few coarse blades, burins, uniface projectile points, and ovoid biface knives or points.

A different tradition is represented by the Denbigh Flint Complex,



Courtesy University of Pennsylvania Museum

FIG. 7. Artifacts of the Denbigh Flint Complex, Alaska.

first found at the Iyatayet site on the Seward Peninsula in northern Alaska, which was occupied about 3000 or 4000 B.C. (Giddings, 1951). A few fairly large projectile points resembled North American Paleo-Indian types, but most of the artifacts were extremely small. There were many microblades and polyhedral cores from which they were struck, burins and tiny tools made from burin spalls, and side and end blades with delicate pressure flaking. Striking resemblances may be observed when comparisons are made with materials from Siberian Neolithic sites,

although these do contain pottery, which is not present in the Alaskan assemblage.

Apart from industries, such as Denbigh, which show later Old World influences, it would seem reasonable to suggest that, over a long period of time, from a very simple base such as is represented by the earliest Siberian Paleolithic, different traditions developed independently in the New World, conditioned in a large measure by environmental factors. Many questions remain, but with further work and continuing cooperation between prehistorians in both hemispheres, many answers will be found.

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PERSPECTIVES

EUROPEAN SCIENTISTS AT WORK, 1961

By SIMON MARCSON

August 1: Lower Regent Street in London is filled with historical memories. As I walk along it to the national headquarters of DSIR (Department of Scientific and Industrial Research), I am so obviously engaged in rubbernecking that I acquire a companion who turns out to be a guide with the morning off. He needs very little prompting to point out to me the monuments and buildings and the historical anecdotes which accompany each.

The DSIR building, headquarters for the fifteen laboratories which it administers in the different parts of the United Kingdom, is crowded and in need of a paint job. As a matter of fact, DSIR thinks so too, and is on the verge of moving. Sir Harry Melville, its Director, is in Canada, and he has arranged for me to see DSIR's Deputy Chief, Dr. B. K. Blount and his colleagues. DSIR is a government agency which was established in 1916 to develop and organize scientific research especially for application to industry. Its concern is with the application of science in industrial processes ranging from forest products to the baking of bread. It does not concern itself with the application of science in problems of defense, atomic energy, medicine, and agriculture.

It does all this in three ways: makes grants to promising research in universities and colleges, and helps them maintain an adequate supply of scientists and engineers for industry, government, and universities. It conducts research to meet the needs of government departments and the society at large. It encourages industry to do research and apply scientific knowledge, chiefly by helping to form cooperative research associations and assists them with grants proportionate to the sums raised by industry. In addition, DSIR communicates the results of new research from industry to industry, and suggests topics for future investigation.

The work of DSIR is guided by an Executive Research Council, appointed by the Ministry for Science. Each member of the Council serves part time at a fee for a specified term of years. The Permanent Secretary of DSIR is the chief officer of a staff of more than 5000, and all other staff members are civil servants.

The total budget of DSIR at the end of 1959 was over $20 million. About one-sixth of the total DSIR staff were classified as Scientific Officers. The largest number of staff people were employed in the three largest stations, the National Physical Laboratory, the Building Research Station, and the Road Research Laboratory. Each laboratory has

its own research director with his own advisory research board of scientists and industrialists appointed by the Research Council. The National Physical Laboratory at Teddington has had and still has its committee appointed by the Royal Society, and the Minister of Science accepts it as a Committee of the Department.

The Permanent Secretary, the Deputy Secretary, and three top advisers meet monthly with the laboratory research directors. Once or twice a year DSIR headquarters staff travel to the various laboratories and have dinner with the research staff. One or more senior members of DSIR's headquarters staff serve on the steering committee of a laboratory. However, the laboratory director is definitely in charge of his laboratory and the DSIR liaison members take his advice. DSIR headquarters centralizes the administration of personnel practices for all the laboratories. These practices, after all, don't have very much leeway since civil service rules constitute their fundamental guide lines. Each laboratory does its own recruiting. Offers of appointment, however, are issued only from DSIR headquarters. Civil service status confers on the laboratory staff member's annual increments within a grade classification. If he is promoted, his annual increment may change. As he moves along and achieves higher grades, his salary is as good as any in industry. In general, the members of the staff feel that their career prospects are better in government than in industry.

Turnover is slight among the DSIR scientists. The opportunities for advancement are good, and for senior men include that of rising to the position of research director. For the younger men, the pension system seems to be an important factor in attaching them to the DSIR laboratory. In any case, DSIR is in a position to attract bright young people and hold them. Very few leave for university positions although university positions rate high among university graduates. Industrial positions, generally, rank low in desirability because they do not seem to pay much attention to the promotion of scientists to important positions.

After lunch at the Athenaeum Club with my hosts, where we drank out of pewter tankards, I was taken to Waterloo Station where I caught the train to Teddington. At Teddington a chauffeured car was waiting to drive me to the National Physical Laboratory. NPL is the oldest of DSIR's laboratories and was in existence long before the creation of DSIR. Its huddled array of old and new brick laboratory buildings are devoted to activities somewhat similar to those of our Bureau of Standards and involve the work of some 1300 staff members of which about 180 have doctorates in science.

The Royal Society which controls its scientific policy appoints NPL's general board. The Board's membership is drawn from industry, government, university, public, and DSIR. While the laboratory's executive

committee appoints the men who head the laboratory's major scientific divisions, and are given the title of Superintendents, the Board must approve such appointments.

The laboratory recognizes scientific distinction by promoting a scientist to a Special Merit appointment. This is the equivalent of a full professorship in a university. Such openings, however, are rare. As a matter of fact, the administrative ladder provides more opportunities of this type. In order to achieve a Special Merit appointment, the scientist has to have great distinction and be a Fellow of the Royal Society. NPL receives about one merit appointment a year and now has twelve scientists in Special Merit positions.

The superintendent is chosen both on the basis of his scientific record, and because he shows promise of being an able manager. He holds monthly staff meetings where the members may bring up anything they wish. The superintendent depends on the group leaders' reports for both the performance of staff members and for what they reveal about the capacities of the supervisory staff. These statements and the merit review reports form the basis of recommendations for promotion. The decisions on promotions are, however, made by DSIR headquarters. The highest attainable award is that of Queen's Honors. DSIR recommends each year four to six scientists and about two to three such awards are given each year to DSIR scientists.

The degree of autonomy a man has in research depends on his position in the laboratory. Senior Principal Scientists have more independence in their work than men in lesser grades. Freedom in research at the NPL laboratories means, however, that he is "free" to pursue his work in the "direction" of the laboratory interests, and not outside of it.

August 3: The Philips buildings tower over Eindhoven but do not hide the existence of its other distinguishing characteristics—an unusual art museum and a budding university. Philips not only towers physically over Eindhoven but for the most part it is Eindhoven since it employs 35,000 of its population. The employment practices of Philips have been for many years influenced by its expert psychology department which specializes in the testing and placement of applicants.

Professor M. H. K. van der Graaf leads Philips psychology department and has the responsibility of testing applicants for lower-grade functions. It also gives expert assistance in cases where the psychological aspect of industrial problems reveals itself as important. The "Psychological Department," as it is known, belongs to the Department for Personnel and Industrial Relations. As a psychological unit it has a staff function and keeps in close touch with the Personnel Department's recruitment and selection units, the Staff Planning and Training Department, the Education Department, the Socio-Economic Department, and the personnel officers of separate line departments. The psychology unit is

also concerned with problems in the management development field. One of the members of the psychology department, Dr. E. Cools, is particularly interested in conducting research on the administration of research laboratories. Professor van der Graaf, while he is a full-time regular member of Philips, teaches at a University. Philips policy is to permit its staff members to carry out such assignments on company time. In all, about fifty staff members do this and their professorial titles are carried over and used in the company.

The world-wide Philips company is managed by a 12-man administrative committee. Among its members is one of the distinguished scientists serving on the staff of the research laboratories. The research laboratories are, therefore, well represented since it numbers only 300 professionals in a total company labor force of 200,000. The Board of Management adopts an annual lump sum budget on the recommendation of the laboratories. Laboratory management allocates this budget in terms of its needs, and is free to make its own decisions as to choice of research areas. There is, however, a long run expectation on the part of the board of management as to results. Nevertheless, the research laboratory's sense of autonomy has fashioned a high degree of responsibility on the part of the laboratory to the company. The laboratory's management proudly stresses its achievement of an academic environment.

Philips Research Laboratories draw a sharp distinction between themselves and the development groups in the product divisions. While relations between the two are important and amicable, the research laboratories insist on defining themselves as devoted to research and not development. The laboratories even go as far as to refuse to accept any government research projects. They fear both the possible dilution of their research activities, and the bureaucratic controls that might emerge with government contracts.

The Deputy-Director, Henri Bienfait, to whom I talked at some length, believes firmly that effective research requires an environment that is not distracted by the immediate needs of industry. To accomplish this objective he believes that the establishment of an environment approaching that of the university is desirable. To further this objective two important steps are taken. Firstly, leading laboratory scientists are given the same status and salary as group leaders, thus encouraging outstanding scientists to remain in research. Group leaders' salaries approach that of head engineers, but are not as high as that of management. Their policy in this instance is that they want their people to know that salaries are higher in management. On the other hand, outstanding scientists are given time off from their working hours to teach in universities, and so add to their income and their professional status. Secondly, new incoming university recruits to the laboratories are permitted to start in fundamental research. Laboratory research manage-

ment reasons that since the recruit's prior background has been university research, his present inclination will be towards fundamental research. After three or four years in fundamental research they then review with each individual his entire potential career. This results, frequently, in the emergence of an interest in developmental research and transfers ensue. Transfers of this type amount to an average flow of 4% per year. The result is that most of the directors and their deputies of product development centers are "alumni" of the research laboratories.

The research laboratories which are composed of physicists, chemists, mathematicians, biologists, and electrical engineers (total 331) have a high ratio of technical staff to professional staff (1700 to 331) and a low ratio of administrative staff to professional staff. The small number of administrative personnel in the laboratories is a matter of pride to them, but is partly explainable in that various corporate departments conduct the relevant activities. The Philips Laboratories recruit about thirty topnotch people each year. They usually manage to obtain those they go after notwithstanding the competition they receive from other companies and universities. The Netherlands universities are competitive in starting salaries but cannot maintain a competitive position salarywise. The Philips Laboratories also have a number of distinguished scientists on their staff which is attractive to new recruits.

Dutch legislation has made it mandatory that every concern must have an employee group. Philips Research Laboratories has a group of ten scientists who meet with research management. These meetings serve to provide an established system of communication between the scientists as employees and their management. Such groups are present in all of the major divisions of Philips and are endorsed by both management and staff. The Dutch rational approach to employee relations seemed to express to me the calm orderliness I found in the other segments of life in Holland.

August 8: As I approached the grounds of the French Petroleum Institute in Malmaison it seemed as if I was entering a large beautiful private estate. This first impression was not too wrong. The grounds had been a private estate adjoining Josephine Bonaparte's house, and her guard house serves now as the Institute's guest house. Distinguished guests now dine on the famous china and dinner service once used by Josephine Bonaparte. The lawns, ponds, trees, and flowering shrubs of the former estate have been retained and the new laboratory buildings fitted in among them. The total effect is that of entering a delightful park containing unexpected surprises.

Mr. E. Fischer, the Secretary-General of the Institute, in the absence of the Director-General, Dr. R. Navarre, received me and explained the Institute's origins. The war's aftermath brought with it the need

to re-establish the oil industry with extensive and new tasks. In order to do this, the various existing organizations such as the National Higher School of Petroleum, the National School of Internal Combustion Motors, and Bonnier's National Research Station were amalgamated into the French Petroleum Institute. The Institute's operating budget depends on a fee which is part of the price structure of petroleum products, and collected by the French Government.

The Minister of Industry and Commerce nominates the laboratory's general manager who is then appointed by the French Government. He in turn appoints his staff. However, he is advised by a board appointed by the Minister of Finance to which the Minister of Industry and Commerce appoints industrialists, delegates of government agencies interested in petroleum matters, and outstanding scientists. The fiscal activities of the institute are supervised by a state comptroller. Any research work carried out at the request of industry is paid for by the particular company to the Institute.

Mr. Fischer explained to me that the Institute's objectives were of three types:

(a) The initiation or execution of research which furthers the development of scientific knowledge and of industrial techniques;
(b) The development of engineers and foremen capable of participating in the formation, diffusion, and effective application of new ideas;
(c) The documentation of new scientific knowledge and industrial techniques of interest to the national economy.

To carry out these objectives the French Petroleum Institute not only has laboratories which carry out research in sedimentology, geology, drilling and refining chemistry, but it operates a school for engineers and technicians.

The National Advanced School of Petroleum and Motors is a training ground for engineers, foremen, and less advanced technicians. The school's curriculum includes not only lectures, but practical work and individual research projects which are carried out in the laboratories of the various technical departments. On-the-job training is also provided for with the cooperation of industry. Refresher courses by outstanding experts are organized periodically for industry's top technicians. These students working towards degrees arrange their curricula with related universities. Students working towards a diploma receive it from the Ministry of Fuels. French students admitted to the school receive scholarships only slightly inferior to the starting salary of an engineer. Some of the students have been employed by companies prior to their entry to the school, and as a result prosecute their studies as active employees.

The school's activities are carried out in its own modern four-story

classroom and laboratory building. It fits into the institute's park-like grounds as another campus-type building, giving ready access to the institute's rich facilities. On the beautiful August day that I visited the school the students were on vacation, but it seemed as if all 200 of them were everywhere as I toured the well-equipped building.

The Institute's personnel numbers 1200 of which 350 are scientists and engineers. Some of the professional people come into the Institute's laboratories from universities, even from universities in Belgium, others from industry. The professional personnel includes chemists, physicists, geologists, mathematicians, and a wide variety of engineers. Seventy per cent of the professional staff people engaged in research have Ph.D.'s, while those in development work are mainly engineers.

Engineering salaries start at about $300 a month. The highest non-administrative research salary is about $700 a month while the top administrative salary in research is about $1000 per month. The starting salary increases at the rate of about six to ten per cent per year for about six to eight years. After that the salary curve flattens out. These salary curves are similar to those in the petroleum industry, and any adjustments to them are worked out in cooperation with the petroleum industry.

After the laboratory member has served for about six to ten years he can be nominated to the position of scientist. This position earns him a salary similar to that of Head of a Department, but permits him to stay on in research without any administrative duties. The laboratory finds this an effective technique for holding on to people.

The smallest work unit consists of a chief and two engineers. Every engineer has one or two technicians assigned to him. Every member of the laboratory's professional staff keeps a time card on which he notes the time he has given to each project he is at work on, and the time he has given to documentation. Since there is no great enthusiasm for these weekly time cards their accuracy is generally regarded as an approximation to the facts.

Members of the research staff are allocated about 10% to 15% of their time to work on their own ideas. If the work a research man carries out in this way looks promising, then he proposes it to his supervisor. This is then followed by a meeting which includes the laboratory director and an assignment ensues. The major problem which occurs in this area is the difficulty of organizing the time of research people with many ideas.

In Retrospect: Many characteristics of the American industrial and government laboratory are readily observable in these three laboratories. I would in retrospect, however, point out one major difference of far-reaching consequences. This difference I would define as one of status. In general, I would say that status is much more emphasized in European

laboratories than in their American counterparts. By and large, scientists have more status than their American associates. On the other hand, European engineers have less status than American engineers.

I know these are dangerous generalizations but I think their consequences are discernible. The greater status of the European scientist gives him more non-monetary fringe benefits, and perhaps, relatively speaking, more economic benefits and a greater sense of professional security. The greater status of the American engineer while making a more mobile and fluid entity at the same time serves to enhance the possibilities of the cooperation of research scientists and product engineers.

HERE AND THERE

When Chen Ning Yang, with his colleague, Tsung Dao Lee, suggested, in 1957, that, "in layman's words, the right- and left-handed basic constituents of matter behaved differently, and when experiment confirmed that the laws of nature do distinguish between a system and its mirror image, our knowledge of fundamental physical principles took one of the sudden turns which have characterized its history." In these words, Dr. Yang's publishers, Princeton University Press, present his new book *Elementary Particles* (68 pages; $2.75), a short history of some discoveries in atomic physics. In his discussion of some of the problems of symmetry involved in physical systems, the author also records that:

> "It is amusing to notice that symmetries that result under a combined transformation are well-known in the decorative arts. In Figure 39 we reproduce (on the following page) a remarkable work of the Dutch artist, M. C. Escher. Notice that the picture is not identical to its mirror image. But if one switches the white and black colors of the mirror image, then identity is restored."
>
> Table 4 of the book, also reproduced, page 252, draws our attention to the xis and the sigmas among the charged particles with lifetimes of the order of 10^{-10} second, in comparison with the lifetime of Sigma Xi, now seventy-five years.

These extracts are published with the permission of the publisher, of the author, and of the artist.

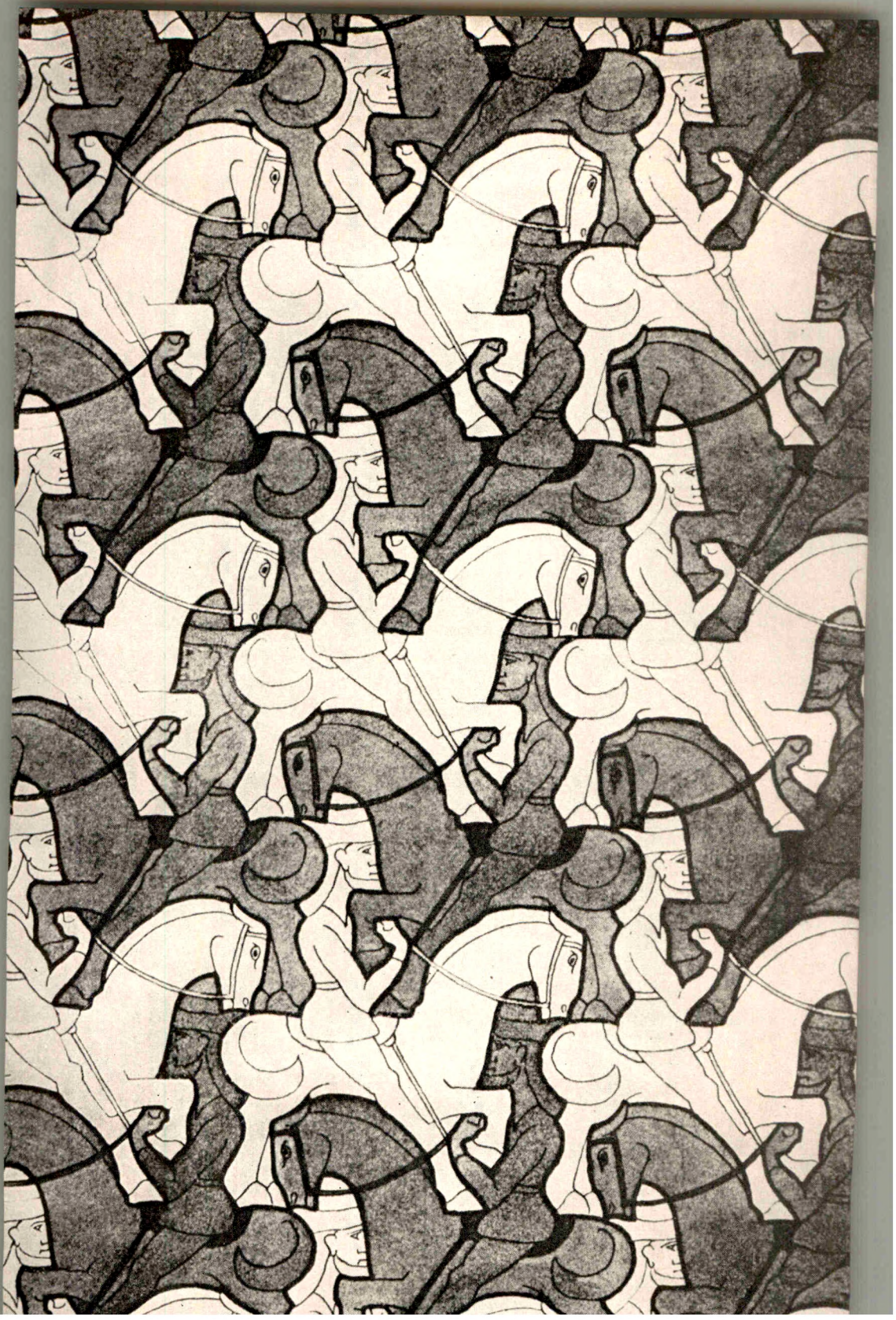


TABLE 4

Particle		Electric[1] charge	Mass in Mev[2]	Spin[3]	Strangeness	Mean lifetime in seconds	Common disintegration products	Antiparticle[5]
Baryons								
Ξ^-	(xi minus)	$-e$	1319	½	−2	2×10^{-10}	$\pi^- + \Lambda$	$\bar{\Xi}^+$ (antixi plus)
Ξ^0	(xi zero)	0	~1311	½	−2	$\sim 2 \times 10^{-10}$	$\pi^0 + \Lambda$	$\bar{\Xi}^0$ (antixi zero)
Σ^-	(sigma minus)	$-e$	1196	½	−1	1.6×10^{-10}	$\pi^- + n$	$\bar{\Sigma}^+$ (antisigma plus)
Σ^0	(sigma zero)	0	1192	½	−1	$\approx 10^{-20}$	$\gamma + \Lambda$	$\bar{\Sigma}^0$ (antisigma zero)
Σ^+	(sigma plus)	$+e$	1190	½	−1	0.8×10^{-10}	$\pi^+ + n$ or $\pi^0 + p$	$\bar{\Sigma}^-$ (antisigma minus)
Λ	(lambda)	0	1115	½	−1	2.5×10^{-10}	$\pi^- + p$ or $\pi^0 + n$	$\bar{\Lambda}$ (antilambda)
n	(neutron)	0	940	½	0	1.0×10^{3}	$e^- + \bar{\nu} + p$	$\bar{n}$ (antineutron)
p	(proton)	$+e$	938	½	0	stable	——	$\bar{p}$ (antiproton)
Bosons								
K^0	(K zero)	0	498	0	+1	(see footnote 4)	(see footnote 4)	$\bar{K}^0$ (anti-K zero)
K^+	(K plus)	$+e$	494	0	+1	1.2×10^{-8}	$\mu^+ + \nu$, $\pi^+ + \pi^0$, or others	K^- (K minus)
π^+	(pi plus)	$+e$	140	0	0	2.6×10^{-8}	$\mu^+ + \nu$	π^- (pi minus)
π^0	(pi zero)	0	135	0	0	$<10^{-15}$	$\gamma + \gamma$	itself
γ	(photon)	0	0	1	0	stable	——	itself
Leptons								
μ^-	(mu minus)	$-e$	106	½	undefined	2.26×10^{-6}	$e^- + \nu + \bar{\nu}$	μ^+ (mu plus)
e^-	(electron)	$-e$	0.511	½	undefined	stable	——	e^+ (positron)
ν	(neutrino)	0	0	½	undefined	stable	——	$\bar{\nu}$ (antineutrino)

1. $e = 4.8 \times 10^{-10}$ e. s. u.
2. Mev = million electron volts = 1.6×10^{-6} erg.
3. Spin = angular momentum in units of $\hbar$, $\hbar = 1.05 \times 10^{-27}$ erg sec.
4. K^0 and $\bar{K}^0$ share the same kind of disintegration products: $\pi^+ + \pi^-$, $\pi^0 + \pi^0$, $\pi^+ + \mu^- + \bar{\nu}$ and others. They both have *two* mean lifetimes = 1×10^{-10} sec and 6×10^{-8} sec. Each of the other particles in the table has only one mean lifetime.
5. A particle and its antiparticle have the same masses, spins and lifetimes. They have equal but opposite electric charges, equal and opposite strangenesses. The disintegration products of an antiparticle are the antiparticles of the disintegration products of the particle. For example, $\bar{\Xi}^+ \rightarrow \pi^+ + \bar{\Lambda}$. Compare, however, footnote 4.

THE SOCIETY OF THE SIGMA XI

REPORT OF THE TREASURER FOR 1961

SCHEDULE 1. BALANCE SHEET AS OF DECEMBER 31, 1961, WITH 1960 FIGURES FOR COMPARISON

	1961	1960
ASSETS		
Bank Balances:		
Union & New Haven Trust Co. (Checking Account)	$ 47,456.71	$ 39,744.52
Connecticut Savings Bank	10,000.00	20,000.00
New Haven Savings Bank	5,208.31	18,069.29
First Federal Savings & Loan Assn.	10,000.00	10,000.00
National Savings Bank	10,000.00	10,000.00
Society for Savings (Hartford)	10,000.00	10,000.00
Union & New Haven Trust Co. (Berg Fund)	3,063.47	2,984.39
Investments (Schedule 4)	222,352.69	194,662.29
Total Assets	$318,081.18	$305,460.49
LIABILITIES		
Semi-Centennial Fund	15,050.00	15,050.00
Berg Memorial Fund	3,063.47	2,984.39
William Procter Memorial Fund	22,624.55	22,624.55
Ada P. McCormick Fund		
January 1, 1961—$2,443.53		2,443.53
Interest 105.80	2,549.33	
The Hsien Wu and Daisy Yen Wu Fund	5,120.00	
Research Fund (Schedule 3)	73,647.22	72,053.20
Securities Reserve		
January 1, 1961 $ 741.95		
1961 Loss 372.25	369.70	741.95
Reserve for Royalties to Authors		
Reserve on January 1, 1961 682.84		682.84
Received in 1961 650.87	1,333.71	
Reserve for Prepaid Assessments		
Reserve on January 1, 1961 10,217.50		10,217.50
Net Increase for 1961 2,356.00	12,573.50	
Capital Reserve, January 1, 1961	129,544.23	129,544.23
Reserve for Life Membership	100.00	100.00
Surplus:		
Balance, January 1, 1961 49,018.30		49,018.30
Operating Gain, 1961 3,087.17	52,105.47	
Total Liabilities	$318,081.18	$305,460.49

SCHEDULE 2. OPERATING STATEMENT, JANUARY 1 TO DECEMBER 31, 1961

	1961	1960	*Budget* 1961
RECEIPTS			
Chapter Assessments	$ 92,931.00	$ 90,747.80	$ 90,000.00
Club Assessments	9,159.50	7,955.00	8,000.00
Membership at Large $45,455.87			
To Reserve for Prepayments 2,356.00	43,099.87	37,080.02	40,000.00
Initiation Fees	11,428.20	11,156.50	10,000.00
Charters (Net)	210.00	150.00	...
Sales of Insignia (Net)	1,383.40	636.44	1,000.00
Subscriptions to AMERICAN SCIENTIST	16,715.94	17,905.53	16,000.00
From RESA for AMERICAN SCIENTIST	7,000.00	7,000.00	7,000.00
Advertising (Net)	43,893.36	42,032.13	35,000.00
Sales of *Science in Progress* (Net)	358.37	500.18	...
Royalties, *Science in Progress*	650.87	...	...

Honoraria for Lecturers	10,320.00	7,680.00	9,000.00
Interest and Dividends (Net)	11,694.76	10,792.85	6,000.00
Initiates' Booklets	525.43	377.42	...
Miscellaneous	259.49	14.33	100.00
Life Membership	...	100.00	...
Total Receipts	$249,630.19	$234,128.20	$222,100.00
DISBURSEMENTS			
Headquarters:			
Salaries	$ 36,052.32	$ 31,290.77	$ 32,400.00
Supplies and Expenses	4,083.30	4,267.20	4,000.00
Postage and Mailing	1,978.77	1,551.50	2,000.00
Rent	2,448.00	2,448.00	3,000.00
AMERICAN SCIENTIST:			
Salaries	9,752.00	9,500.00	12,000.00
Printing and Mailing	127,734.78	114,600.66	120,000.00
Office Expenses	1,277.64	1,338.05	1,200.00
Honoraria	1,000.00	675.00	1,500.00
Rent	1,260.00	1,260.00	1,260.00
Lecture Program:			
Honoraria	10,650.00	7,635.00	12,000.00
Travel	7,773.59	4,488.29	7,000.00
Clerical	3,155.05	3,394.60	3,600.00
Science in Progress	192.50	...	750.00
Royalties (to Reserve for Authors)	650.87	...	...
Officers' Travel and Expense	5,212.43	5,142.92	4,500.00
Membership Program	7,662.26	6,973.91	7,500.00
Science Service	300.00	300.00	300.00
Bank Charges	22.48	111.67	200.00
To Reserve for Life Memberships	...	100.00	...
New Equipment	$ 1,673.60	$ 2,925.99	$ 1,000.00
Interest to Berg Fund	79.08	55.23	...
Interest to Research Fund	602.00	602.00	600.00
Wu Fund, Interest to Research	138.62	...	...
Transferred to Research Fund	15,000.00	25,000.00	5,000.00
Social Security	1,414.42	1,041.48	900.00
Manuals of Procedure	1,456.86	934.14	800.00
Initiates' Booklet	...	...	200.00
IBM Machine Rental	768.00	792.00	800.00
Connecticut Medical Service	295.75	137.95	350.00
Interest to McCormick Fund	105.80	103.00	...
Miscellaneous	801.46	737.72	500.00
Remodeling Office	2,648.33	...	3,000.00
IBM Conversion	...	1,000.50	...
Certificates	353.11	1,655.97	(100.00)
Total Disbursements	$246,543.02	$230,063.55	$226,260.00
Operating Gain	3,087.17	4,064.65	(4,160.00)
	$249,630.19	$234,128.20	$222,100.00

SCHEDULE 3. RESEARCH FUND OPERATIONS

Balance, January 1, 1961		$72,053.20
Contributions, Membership-at-Large & Chapters	$33,672.68	
Transferred from RESA	1,500.00	
Interest on Semi-Centennial Fund	602.00	
Interest on Wu Fund	138.62	
Transferred from 1961 Operating Account	15,000.00	
	$50,913.30	
Less Grants for Year	49,319.28	
Increase in Fund for Year	1,594.02	1,594.02
Balance, December 31, 1961		$73,647.22

SCHEDULE 4. INVESTMENTS

(As of December 31, 1961. Carried at Cost)

Bonds:			
$ 5,000	American Tel. and Tel. Co., 3⅜%, 1973	$ 5,229.38	
$10,000	American Tel. and Tel. Co., 4¾%, 1998	10,466.95	
$ 5,000	American Tobacco Co., 3¼%, 1977	5,079.99	
$ 5,000	Borden Co., 2⅞%, 1981	4,992.09	
$ 5,000	Boston Edison Co., 2¾%, 1970	5,090.23	
$ 5,000	Consumers Power Co., 3⅛%, 1981	5,114.19	
$ 7,000	Copperweld Steel Co., 5%, 1979	7,406.00	
$ 6,000	Pan American World Airways, 4⅞%, 1979	5,538.25	
$ 6,000	Sinclair Oil Corp., 4⅜%, 1986	5,883.25	
$ 3,000	Southern Bell Tel. and Tel. Co., 3%, 1979	3,273.75	
$15,000	U.S. Treasury, 5%, 1964	15,000.00	
$15,000	U.S. Treasury, 3⅝%, 1967	15,211.50	
	Total Bonds		$ 88,285.58
Stocks:			
100 shares	Allied Chemical & Dye Corp.	$ 3,813.53	
40 shares	American Airlines, 3½%, Pfd.	3,743.95	
200 shares	American Cyanamid Co.	7,972.65	
103 shares	American Electric Power	4,937.10	
418 shares	American Machine & Foundry	3,848.57	
160 shares	American Metal Climax, Inc.	5,120.00	
100 shares	Chesapeake & Ohio Railway	6,126.58	
219 shares	Commonwealth Edison Co.	4,310.99	
228 shares	Consumers Power Co.	10,638.15	
25 shares	Du Pont de Nemours Co.	2,437.92	
100 shares	Eastman Kodak	10,336.79	
270 shares	El Paso Natural Gas Co.	3,061.74	
100 shares	First National City Bank of New York	10,299.25	
200 shares	General Foods Corp.	2,235.10	
239 shares	General Telephone & Electronics Corp.	4,165.29	
200 shares	Marshall Field & Co.	3,595.66	
100 shares	Niagara Mohawk Power Co.	2,806.56	
100 shares	Owens-Illinois Glass Co.	3,927.64	
40 shares	Pacific Lighting Co., 4¾% Pfd.	4,286.48	
300 shares	Procter & Gamble	7,074.06	
100 shares	Sears Roebuck & Co.	7,121.08	
100 shares	Standard Oil Co., Indiana	4,227.94	
157 shares	Standard Oil Co., New Jersey	3,212.23	
216 shares	Texaco, Inc.	2,651.51	
300 shares	Texas Eastern Transmission Corp.	3,127.50	
100 shares	Tri-Continental, $2.70 Pfd.	5,300.35	
160 shares	Westinghouse Electric Corp.	3,688.49	
	Total Stocks		$134,067.11
	Total Investments		$222,352.69

SCHEDULE 5. INVESTMENT TRANSACTIONS, 1961

	Price	Book Value
Sold 209 Rights American Machine & Foundry	$ 478.80	$ 478.80
Sold $10,000 U.S. Treasury 2¾%, 1961	10,028.12	9,915.10
Sold $3,000 U.S. Treasury, 3¼%, 1978–1983	2,621.20	3,000.00
Sold $2,000 Chicago Union Station, 3⅛%, 1963	1,953.81	2,060.28
Withdrawn from Connecticut Savings Bank	10,000.00	10,000 00
Withdrawn from New Haven Savings Bank	12,860.92	12,860.92
Sale Fraction of share General Tel. & Elec.	6.21	6.21
	$37,949.06	$38,321.31
Loss Charged to Securities Reserve	372.25	
	$38,321.31	
Bought 20/40 share American Electric Power	30.79	
Bought 100 shares Eastman Kodak Co.	10,336.79	
Bought 100 shares First National City Bank	10,299.25	
Bought 100 shares Sears Roebuck Co.	7,121.08	
Bought 16 shares Consumers Power $800 bond exchanged and cash	160.00	
Bought $10,000 American Tel. and Tel., 4¾%	10,466.95	
Bought 72/100 share Commonwealth Edison	33.28	
Exchanged $7,000 U.S. Treasury, 2¼%, June 15, 1959–1962 and $8,000 U.S. Treasury, 2¼%, Dec. 15, 1959–1962 and $24 for $15,000 U.S. Treasury, 3⅝%, 1967		

AUDITOR'S STATEMENT

SIGMA XI

I have examined the books and supporting records of The Society of the Sigma Xi, Incorporated, for the year ended December 31, 1961.

All receipts recorded by the Secretary's and Treasurer's offices and income due from investments were traced to the Cash Account. Expenditures were verified against cancelled checks.

Cash balances were verified by reconcilement of bank statements and by direct confirmation with banks. Assets of the Investment Account were examined.

Payments for awards of Grants-in-Aid of Research were checked for proper authorization.

In my opinion, the records of the Society are properly maintained and in good order. I certify that the schedules of the report of the Treasurer are true to the best of my knowledge.

Respectfully submitted
C. S. LOCKROW
Auditor

New Haven, Connecticut
February 20, 1962

THE SCIENTIFIC RESEARCH SOCIETY OF AMERICA

REPORT OF THE TREASURER FOR 1961

	1961	1960	*Budget* 1961
RECEIPTS			
Assessments, Branches	$17,199.50	$ 15,922.50	$ 15,000.00
Assessments, Membership-at-Large	2,899.50	2,422.00	1,800.00
Charter Fees	250.00	255.00	...
Insignia Sales (Net)	3.14	96.25	25.00
Dividends & Interest (Net)	5,888.61	5,882.73	5,000.00
Miscellaneous	55.00	51.20	10.00
	$26,295.75	$ 24,629.68	$ 21,835.00
DISBURSEMENTS			
Salaries	$ 8,649.56	$ 6,908.11	$ 8,400.00
Postage and Supplies	994.33	1,187.58	800.00
Rent	312.00	312.00	350.00
Officers' Travel and Expense	1,433.36	821.49	1,000.00
New Equipment	1,232.20	520.78	200.00
Certificates	199.20	345.80	400.00
Charters	223.00	385.00	...
Refunds of Overpayments	73.50	263.50	...
Social Security	190.93	140.52	200.00
Procter Prize	1,025.00	1,025.00	1,025.00
Sigma Xi for AMERICAN SCIENTIST	7,000.00	7,000.00	7,000.00
Pamphlets	1,143.00	...	300.00
Grants-in-Aid of Research	1,500.00	1,500.00	1,500.00
Miscellaneous	66.25	110.49	100.00
	$24,042.33	$ 20,520.27	$ 21,275.00
Operating Gain	2,253.42	4,109.41	560.00
	$26,295.75	$ 24,629.68	$ 21,835.00

BALANCE SHEET AS OF DECEMBER 31, 1961, WITH 1960 FIGURES FOR COMPARISON

		1961	1960
ASSETS			
Cash, Union and New Haven Trust Co.		$ 7,750.14	$ 4,682.22
Connecticut Savings Bank		1,654.83	...
Securities (at cost)		129,505.27	114,723.95
Total Assets		$138,910.24	$119,406.17
LIABILITIES			
Research Fund, Dec. 31, 1960	$ 722.41		
1961 Contributions	814.50		
Reserve for Research		1,536.91	722.41
Securities Reserve, Dec. 31, 1960	736.00		
1961 Profit	16,436.15	17,172.15	736.00
Procter Memorial Fund		94,331.27	94,331.27
Capital Reserve		13,000.00	13,000.00
Surplus, Dec. 31, 1960	10,616.49		
Operating Gain for 1961	2,253.42		
Surplus, Dec. 31, 1961		12,869.91	10,616.49
Total Liabilities		$138,910.24	$119,406.17

INVESTMENTS

(As of December 31, 1961. Carried at Cost)

Bonds:			
$10,000	American Tel. and Tel. Co., 4¾%, Nov. 1, 1992	$10,312.50	
$10,000	Bell Telephone Co. of Pa., 4¾%, May 1, 2001	10,412.50	
$10,000	General Motors Acceptance, 4⅝%, Mar. 1, 1983	9,964.00	
$10,000	Pacific Gas & Electric 1st. and Ref., 3⅜%, Dec. 1, 1985	10,302.60	
$10,000	Southern N. E. Telephone, 3¼%, July 1, 1985	10,101.40	
$10,000	Union Electric, 4¾%, July 1, 1991	10,275.00	
$ 5,000	U.S. Treasury Notes, 5%, Aug. 15, 1964	5,000.00	
$ 5,000	U.S. Treasury, 4%, Feb. 15, 1980	4,906.13	
	Total Bonds		$ 71,274.13
Stocks;			
108 shares	Allied Chemical & Dye Corp.	$ 3,714.55	
200 shares	American Cyanamid Co.	5,813.70	
70 shares	American Machine & Foundry, 3.9% pref.	5,937.12	
251 shares	Conn. Light & Power Co.	4,049.62	
270 shares	El Paso Natural Gas Co.	4,886.91	
102 shares	National Lead Co.	3,226.65	
220 shares	Ohio Edison Co.	3,853.69	
80 shares	Owens-Illinois Glass Co.	3,010.95	
93 shares	Pacific Gas & Electric Co.	6,084.33	
300 shares	Procter & Gamble Co.	4,706.25	
155 shares	Standard Oil Co. (N.J.)	4,208.63	
216 shares	Texaco, Inc.	3,061.69	
100 shares	Texas Utilities Co.	1,922.77	
200 shares	Westinghouse Electric Corp.	3,754.28	
	Total Stocks		$ 58,231.14
	Total Investments		$129,505.27

Investment Transactions in 1961

	Price	Book Value
Sold 93 rights, Pacific G. & E. Co.	$ 26.99	$ 26.99
Sold $10,000 U. S. Treasury, 2¾%, 1961	10,028.12	9,822.50
Sold 150 shares Associated Spring Co.	2,418.68	3,125.00
Sold 100 shares Procter & Gamble	9,243.79	1,568.75
Sold 100 shares Texas Utilities Co.	10,467.05	1,922.77
Sold 100 shares Torrington Co.	5,848.62	3,375.00
Sold 200 shares Stanley Works	3,319.88	3,180.00
Sold 50 shares United Fruit Co.	1,186.68	3,082.65
Profit added to Securities Reserve		16,436.15
	42,539.81	42,539.81
Bought $10,000 General Motors 4⅝%, 1983	9,964.00	
Bought $10,000 American Tel. & Tel. 4¾%, 1992	10,312.50	
Bought $10,000 Bell Tel. of Pa. 4¾%, 2001	10,412.50	
Bought $10,000 Union Electric Co. 4¾%, 1991	10,275.00	
Deposited in Connecticut Savings Bank	1,654.83	

AUDITOR'S STATEMENT

SCIENTIFIC RESEARCH SOCIETY OF AMERICA

I have examined the books and supporting records of the Scientific Research Society of America for the year ended December 31, 1961.

Cash balances were verified by reconcilement of bank statements and direct confirmation with the banks. All receipts recorded were checked to the Cash Account and expenditures were verified against paid bank checks. All securities were examined and income receivable was accounted for.

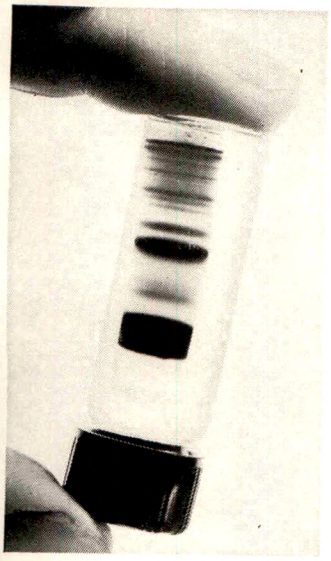
In my opinion, the records of the Society are properly maintained and in good order. I certify that the schedules of the report of the Treasurer are true to the best of my knowledge.

Respectfully submitted

Kodak reports on:

what to do till the flying-spot scanner comes . . . new personal monitoring film, very sensitive . . . gentle gravity

Disc electrophoresis



Each of the dark discs in this little tube of gel represents a different protein in a *3-microliter* sample of normal human serum. The separation was effected in 30 minutes. The pattern tells a detailed story to the practiced eye. The equipment can be jerry-built in the laboratory for a materials outlay of about $12.* We sell most of the chemicals required. To reduce dependence on perceptual machinery in the skull of the operator, a program is under development wherein protein concentrations represented by the discs are to be determined by a TV-like flying-spot scanner and the signals fed to a digital computer programmed to identify patterns at the rate of 10^4 per week.

Peripheral as may be our connection with the new disc electrophoresis, we can do our part by drum-beating in print for people to learn how Drs. B. J. Davis and Leonard Ornstein of The Mount Sinai Hospital Cell Research Laboratory in New York City have extended electrophoresis in the directions of 1) more simplified equipment that gets you into the technique easily; 2) more sophisticated equipment that speeds and refines results once you have found the technique indispensable; 3) a clear (albeit concentrated and mathematical) exposition of the physical chemistry at work.

We propose a little experiment in casting bread upon the waters. We shall count how many will accept an offer hereby made by Distillation Products Industries, Rochester 3, N. Y. (Division of Eastman Kodak Company) to send a free preprint of a paper by Dr. Ornstein on the theory and a fully illustrated set of directions by Dr. Davis on how to proceed. Then we shall wait and count how many orders come in for the Eastman Organic Chemicals named therein. The results of this experiment you will never know.

*Or it can be purchased in a variety of models from CANALCO, 4935 Cordell Avenue, Bethesda 14, Md.

Down with the administrative dose

Two little packets of film are extracted from a factory-fresh carton. One is locked away in a clean safe. The other is worn by a worker in the vicinity of ionizing radiation. After a month the two are processed together. Both turn out equally blank. This shows how much of a dose the worker has probably had less than.

Social ethics in advanced countries require the assumption that the worker has actually *had* that much radiation. It is known as the "administrative" dose. Records are kept as in a bank. When administrative and physical doses add up to a critical figure, the worker is shifted to a different job. He may habitually spend every Saturday night cruising the center line of a busy highway at 80 m.p.h. Nevertheless, the critical figure assumes that he wants to live forever and become the progenitor of an infinite line of biologically perfect descendants. Pressure to squeeze it down will never let up, we hope.

Without relaxation of solicitude, we have taken steps to cut down the waste of his job experience. By reducing the administrative dose, we can keep him in his slot longer. It is within our power. All we have to do is make more sensitive film. This we have now done. It is called Kodak Personal Monitoring film, Type 3.

The packet it comes in also includes a piece of low-sensitivity film. Its sensitivity is so low that it can measure 1800 roentgens, a horrible thought. The lower limit of dose measurement for the high-sensitivity film in the packet runs somewhere below 10 milliroentgens. The vague phrasing of that statement doesn't mean that the exact value is unimportant. The main point of this discussion is the importance of the figure.

If interested, prepare yourself by studying pp. 10-53 to 10-75 of Radiation Hygiene Handbook (*McGraw-Hill Book Company, Inc., 1959*). *Then bring your knowledge up to date by requesting a data sheet on* Kodak Personal Monitoring film, Type 3 *from Eastman Kodak Company, Special Sensitized Products Division, Rochester 4, N. Y.*

A slide projector like a merry-go-round



This is the Kodak Carousel projector. It projects slides. Carousels symbolize carefree abandon. Care lest slides jam can be abandoned. Gravity feeds them. Gentle gravity. Slides are automatically lifted back to 80-slide storage tray. Pushbuttons at end of long cord advance slides,* reverse, even refocus. (Latter is largely for kicks. Actually, slides get prewarmed not to pop out of focus.) See Kodak dealer for exact price. You will find it is less than $150. Lucky you happen to be in the market at this particular time.

*Alternatively, projector can be set to advance every 5, 10, or 20 seconds. This scheme is more urbane than an alarm clock for the chairman.

Price subject to change without notice.

THE SCIENTISTS' BOOKSHELF

By Hugh Taylor, the Associate Editors, and Guest Reviewers

The Green Flash and Other Low Sun Phenomena by D. J. K. O'CONNELL; 192 pages; $6.00; Interscience Publishers, 1958.

A romance by Jules Verne, *Le Rayon Vert*, published in 1882, aroused widespread interest in a phenomenon occasionally accompanying sunrise and sunset. The story prompted W. Swan to record, in *Nature* in 1883, a dazzling emerald-green flash seen by him from the Righi at sunrise over a distant mountain. His observation had been made in 1865. An early published account by Winstanley (1873) describes the phenomenon thus: "The green ray . . . begins at the points or cusps of the visible segment of the sun and when the setting is nearly complete extends from both cusps to the central space between." Joule's own observations of the flash are recorded by Winstanley. Lord Kelvin described a *blue* flash seen by him in 1899 as the sun rose over Mont Blanc.

Opinion has been divided as to the green flash. Retina fatigue from the reddish rays of the setting sun has been suggested as a cause. Other scientists have been convinced that the green flash is an objective phenomenon.

The Vatican Observatory at Castel Gondolfo proved to be a suitable location for test of the two alternatives. There is an unobstructed view across the Roman Campagna to the horizon in the Mediterranean, fifty miles away, for observations of sunsets. Sunrise can be observed over the Alban Hills. With the patient skill of the Observatory's instrument-maker, C. Treusch, S.J., it was found possible to photograph the green flash in color under both conditions. This book contains the early history of the observations, the Observatory photographs, eighty of which are prepared for printing (in six colors by offset process) and magnificiently reproduced by the printers, the Typografia Poliglotta Vaticana. The printed word cannot do justice to the excellence of the results obtained, not only of sunset and sunrise, but also of the green flash accompanying the setting of Venus. All the observations lead to the conclusion that the chief factors involved in the formation of the green flash are atmospheric dispersion, selective absorption in the atmosphere and the scattering and absorption by air molecules, dust and haze in the atmosphere. The skill of the Vatican observers, with a considerable assist from Kodak Ektachrome film and the Kodak Research Laboratories as to the effect of under- and over-exposure on the colors received, makes it certain that the green flash is objective, its appearance explainable by the ordinary laws of optics.

Preparative Methods of Polymer Chemistry by W. SORENSON and T. W. CAMPBELL; 337 pages; $10.50; Interscience Publishers, Inc., 1961.

Experimental polymer chemists will welcome this book particularly. It is the first of its kind and provides the sort of information necessary to prepare actual polymers, much as numerous monographs and series have provided such information for preparative organic chemistry.

The book covers the practical aspects of polymer characterization, equipment for various methods of preparation with illustrations and sufficient detail to acquire quickly "know-how" in this field. Simple procedures are outlined for polymer fabrication such as injection molding, melt-pressing, spinning of fibers, film casting etc. which can be duplicated in almost any laboratory. The

Princeton UNIVERSITY PRESS

Applied Dynamic Programming

By *Richard Bellman and Stuart Dreyfus*

Dynamic Programming has become more and more widely known and adopted as a valuable tool for the treatment of many challenging problems confronting decision-making agencies in the economic, industrial, scientific, and even political spheres of modern life. Not only graduate students in mathematics and economics, but also engineers and research workers in "operations analysis" will find interest in the book as it takes them into the computational aspects of applying the techniques of Dynamic Programming. *A RAND Corporation Research Study.*

400 pages. $8.50

Elementary Particles: A Short History of Some Discoveries in Atomic Physics

By *Chen Ning Yang*

Dr. Yang, Nobel Prize winner, reviews the history of our knowledge of the infinitesimal components that make up the atom or that are produced when particles collide at high energies. He emphasizes and discusses those discoveries that brought new insights, and shows how theory and experiment interact in unexpected ways to extend knowledge.

80 pages. $2.75

Guide to Tables in Mathematical Statistics

By *J. Arthur Greenwood and H. O. Hartley*

This *Guide* catalogues a large selection of tables belonging to the field of mathematical statistics, the bulk of which were originally issued between 1900 and 1954, but some of which relate to works as early as 1799 and as late as 1960. As well as filling a need for those actively engaged in the computational side of mathematical statistics, this work offers valuable reference to the professional computer faced with a statistical problem and to the statistician called upon to compute.

930 pages. $8.50

Order from your bookstore, or

PRINCETON UNIVERSITY PRESS

Princeton, New Jersey



cording to the type of polymer-forming reactions with representative examples of almost every important type of polymer. The largest section is devoted to addition polymers from unsaturated monomers and covers every aspect of initiation. The book is sufficiently up-to-date to include preparations of stereospecific polymers, linear polyethylene, the use of anion-radical initiated polymerization to make "living polymers" for block copolymers and other recent techniques. There are also a few preparations of inorganic polymers such as polyphosphonitrilic chloride, sulfur.

Throughout the book the user is constantly made aware of toxicity or explosion hazards. This feature is welcome and it shows the authors' thorough familiarity with an unending variety of sophisticated experimental techniques. Luckily no recipes are given for some of the fluoropolymers, for no detailed procedure could teach adequately how to avoid the dangers associated with them.

A well-organized set of indexes completes the book and lets the user find examples according to a variety of headings. This reviewer hopes that future editions will be forthcoming regularly and that they might include tables of monomer reactivity ratios to allow copolymer preparations not described in the book. Extensive references at the end of each chapter will let the user supplement the preparations.

For the advanced student in organic chemistry there should be an excellent opportunity to use this book as a guide for preparative polymer chemistry, a training rarely found in our universities.—*N. E. Wolff*

Physics and Chemistry of the Earth, edited by L. H. AHERNS, *et al.*; 317 pages; $10.00; Pergamon Press Inc., 1961.

The state of seven areas of geophysics and one area of geochemistry are covered in this review volume. The articles contain references to work published early in 1959. The two-year gap between the preparation and publication lessens the value of several of the "Variations of the Earth's Rotation" by Sir Harold Spencer-Jones, "The Oscillations of the Earth" by Robert Stonley, and "The Theory of Nutation and Internal Constitution of the Earth" by R. O. Vicente.

The most comprehensive article is that by R. Hide and P. H. Roberts on the origin of the main geomagnetic field. The present-day field and its fluctuations are described with the aid of ten carefully prepared charts. Recent paleomagnetic results and their bearing on the origin of the field are taken up in considerable detail. The authors are cautious and critical.

Lawrence Aller discusses the abundance of elements in the sun and stars and compares these abundances with those found in meteoritic material. The presentation of the observational methods and the difficulties in interpretation of spectroscopic records is rewarding. Aller has since expanded this discussion into a full-scale book recently published by Interscience.

Rhodes Fairbridge contributes 86 pages and 15 diagrams on eustatic changes of sea level. The fluctuation, in sea level deduced by Fairbridge is similar to those presented by other workers but different in the number of minor oscillations that are listed. The author feels that both major and minor oscillations during the quaternary reflect climatic changes. The question remains as to whether the stratigraphic and chronologic data are precise enough to delimit the fluctuations.

Three Russian authors discuss the problem of the generation of earthquakes. The point of view adopted is similar to that of Byerly and Hodgson. The discussion clearly reveals the difficulties in applying small amplitude elastic theory to physical processes at the focus of an earthquake.

E. Goldberg stresses the geochronology and authigenic mineralogy of deep-sea sediments. A valuable feature of the discussion is the detailing of the uncertainties involved in most methods of measuring rates of accumulation.—

International Series of Monographs on Pure and Applied Biology, Modern Trends in Physiological Sciences Division

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by Ch. Kayser, *Professor of Physiology, Strasbourg University*

A considerable amount of data is offered, with detailed discussion on the true hibernation of animals and birds.
263 pp. $9.00

VOL. 14 THE BIOSYNTHESIS OF PROTEINS

by H. Chantrenne, *Professor of Biological Chemistry, University of Brussels*

A critical evaluation of the present state of knowledge of protein biosynthesis; the first volume to cover all main aspects of the subject.
Partial Contents: The Genetic Control of Protein Synthesis; The Chemical Nature of Genetic Determinants of Protein Structure; The Colinearity Hypothesis; The Sites of Protein Formation Within the Living Cells; Nucleic Acids and Protein Synthesis; The Position of DNA in Protein Synthesis; Ribonucleic Acids and Protein Synthesis.
200 pp., illustrated $6.50

THE CHROMATOGRAPHY OF STEROIDS

International Series of Monographs on Pure and Applied Biology, Biochemistry Division, Volume 2

by I. E. Bush, *Professor of Physiology, Uuiversity of Birmingham*

This volume fills the void of information on the theory of partition chromatography and its application to the behavior of steroids in chromatographic systems.
Partial Contents: The Basic Theory of Chromatography and Some General Principles; Chromatographic Separation of Steroids; Techniques and Apparatus; Quantitative Chromatography.
420 pp., illustrated $12.50

OOGENESIS—The Storage of Developmental Information

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by C. P. Raven, *Professor of Zoology, University of Utrecht*

An excellent analysis of the formation of egg cells in the ovary and their relationships with surrounding elements, with reference to the transmission of information from one generation to the next through the germ cells.
Partial Contents: Developmental Information; The Decoding of Developmental Information; The Encoding of Developmental Information; Oogenesis; Diffuse Egg Formation; Localized Egg Formation; Follicular Egg Formation.
310 pp., illustrated $8.50

ADVANCES IN BIOLOGY OF SKIN VOLUME I CUTANEOUS INNERVATION

Edited by William Montagna, *Brown University*

A compilation of the material presented at the Symposium on the Biology of Skin, held at Brown University, and encompassing all that is known about the structure and function of cutaneous innervation.
Partial Contents: Similarities in Cutaneous Nerve-End Organisms; The Autonomic Innervation of the Skin; The Relation of Nerve Fiber Size to Modality of Sensation; Structures and Processes Involved in the Sensation of Itch.
224 pp., illustrated $10.00

VOLUME II BLOOD VESSELS AND CIRCULATION

Edited by William Montagna *and* Richard Ellis, *Brown University*

The nine papers in this book present all major methods for studying blood vessels and circulation, and were presented at the Brown University Symposium on the Biology of Skin (Blood Vessels and Circulation of Blood in the Skin).
168 pp., illustrated $10.00

PROGRESS IN REACTION KINETICS, VOLUME I

Edited by G. Porter, *Sheffield University*

The first volume in a planned new series, each of the nine articles presented reviews recent advances in a particular area of the field of kinetics.
Partial Contents: Reactions of Oxygen, Kaufman; Kinetics of the Reactions of Ions with Molecules, Lampe, Franklin and Field; Salt Effects in Solution Kinetics, Davies; Fast Reactions of Excited Molecules, Weller; Physical Chemical Aspects of Enzyme Kinetics, Peller and Alberty.
272 pp., illustrated $12.00

THE HISTORIAL DEVELOPMENT OF BRITISH PSYCHIATRY VOLUME I, 18th and 19th Century

by Denis Leigh, *Bethlem and Maudsley Hospitals*

This first volume of a trilogy on the history and development of British psychiatry deals with developments during the 18th and 19th century, and outlines the lives and work of the pioneering doctors.
282 pp., illustrated $10.00

THEORY OF THE TRANSMISSION AND PROCESSING OF INFORMATION

by A. G. Vitushkin, *U.S.S.R., translated by* A. D. Booth, *Birkbeck College*

This monograph, translated from the Russian, attempts to define mathematically the concept of the complexity of a tabulation problem (the construction of tables for functions) by using concrete problems as examples.
Partial Contents: The Concept of the Entropy of a Metrical Space; The Entropy of Analytic Function Spaces; The Variations of a Set; Estimates of the Variations of some Concrete Sets; Easily Representable Families of Functions.
224 pp. $15.00

CONVERGENCE PROBLEMS OF ORTHOGONAL SERIES

International Series of Monographs on Pure and Applied Mathematics, Volume 19

by G. Alexits, *Technical University, Budapest*

A systematic account of the convergence theory of the orthogonal series. A revised and enlarged edition of the German original, it includes all new and important discoveries in recent years.
Partial Contents: Fundamental Ideas, Examples of Orthogonal Functions; Investigation of the Convergence Behavior of Orthogonal Series by Methods Belonging to the General Theory of Series; The Lebesque Functions; Bibliography.
350 pp. $15.00

SEPARATION OF HEAVY METALS

by A. K. De

This volume is a careful compilation and coordination of currently available literature in the field of the separation of heavy metals.
Partial Contents: Liquid Extraction in the Separation of Heavy Metals; Recommended Analytical Procedures; Radiochemical Separation Procedures; Physical Constants of Organic Solvents.
350 pp., illustrated $9.00

All books available for 30 days' free examination.



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Wiley BOOKS

BASIC PRINCIPLES OF THE TRACER METHOD

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Structure of the Moon's Surface by GILBERT FIELDER; 266 pages: $7.50 Pergamon Press Inc., 1961.

Although the moon has been observed for centuries, the last four years have witnessed an almost explosive growth of interest in the nature and origin of the moon and its features. The realization that man will, in the near foreseeable future, walk upon the face of the moon has, no doubt, been the major stimulus for the renewed study of our natural satellite. For those of us, amateur and scientist alike, who have only of late been caught in the enthusiasm for the moon this book offers an excellent chance to catch up.

The first half of the book in 133 pages gives a valuable summary of the history and results of studies of the librations, elevations of surface features, and the over-all shape of the moon, its luminous intensity, luminescence, polarization, temperature, and atmosphere. The last chapter of the first half presents the conclusions that have been reached by the various investigations concerning the nature and composition of the moon's surface. The first half suffers slightly from brevity and an occasional obscure statement. Following each chapter is a short summary section (which on some topics finally elucidates what was not made clear before) and a detailed list of references.

The second half of the book is not as satisfactory. Particularly discouraging are the structural interpretations of the lunar surface. This section covers rays; lattice patterns and what he calls "the grid system"; rills, faults, and wrinkle ridges; crater distribution and frequency; and conditions relating to the origin of the moon's surface structure.

Interspersed with interesting data and concepts through the second half of the book occur statements and ideas that are often puzzling and, in places, misleading. Although many of the errors may be of minor or trivial nature, their presence, nevertheless, leaves an unfavorable impression.—*Carl Brown*

Treatise on Invertebrate Paleontology, RAYMOND C. MOORE; 351 pages; $7.50; Geological Society of America and the University of Kansas Press, 1960.

This is an ambitious attempt to provide useful information on a wide variety of fossil invertebrates to a wide variety of readers, "neozoologists as well as paleozoologists, . . . beginners . . . as well as thoroughly trained . . . workers, . . . teachers, stratigraphic geologists, and individuals engaged in research on fossil invertebrates."

Of the planned 23-volume set, nine have been published. Six volumes, or Parts, the most devoted to one phylum, are to treat the Mollusca. Part I introduces the phylum and deals in detail with the three smaller classes, Monoplacophora, Amphineura, and Scaphopoda, and primitive groups of the large class Gastropoda.

The introduction to the phylum is a concise survey of its several classes by the authority most knowledgeable on the group, C. M. Yonge.

Each succeeding chapter consists mainly of an annotated, illustrated list of the genera and subgenera comprising one class. A great deal of information on each taxon is presented very succinctly: author, date, diagnostic characters, type species, geologic and geographic range, and references are often given within a column-inch. Each genus and subgenus is illustrated; in many cases the type species is shown. The figures are clear line drawings or photographs conveniently placed adjacent to the accompanying text.

All six classes of mollusks were well differentiated in the Paleozoic, and all have extant representatives. The Gastropoda, to which most of Part I is devoted, have by far the largest number of living species. A detailed, well-illustrated 80-page introduction to this class by L. R. Cox is followed by systematic treatment of the primitive order Archaeogastropoda, which contains the modern abalones, limpets, and top shells.

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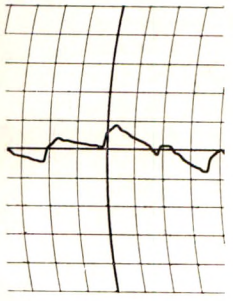
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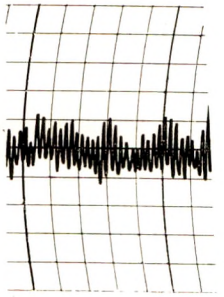
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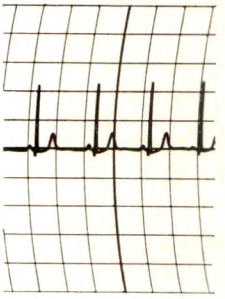
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able to deduce anatomical features from fossil shells and have successfully fitted extinct groups into neozoological classification. However, so far, too little has been learned from the fossil record to evaluate critically this theory of the course of evolution of the major gastropod groups.

The contributors to Part I merit commendation for their success in helping to break down the artificial interdisciplinary barrier between neozoology and paleozoology.—*Alan J. Kohn*

Radiation Protection (Report of Committee III on Protection Against X-Rays up to Energies of 3MeV and Beta- and Gamma-Rays from Sealed Sources); 84 pages; $3.00; Pergamon Press Inc., 1960.

This book lists recommendations for installation, operation, and monitoring of radiation sources for medicine and industry. The recommendations are of the educated common-sense variety; the book could be used as a check list for any new installation. An extensive appendix provides sufficient data for computing transmission and attenuation of X-radiation across different kinds of barriers. Depth and skin dose data at different tube voltages are included for aid in computing adequate protection. At the end of the volume, after the appendix, are added the recommendations of the International Commission on Radiological Protection. These deal with maximum permissible doses and exposure of the population.—*R. C. von Borstel.*

Plants, Viruses, and Insects by KATHERINE ESAU; 110 pages; $3.75; Harvard University Press, 1961.

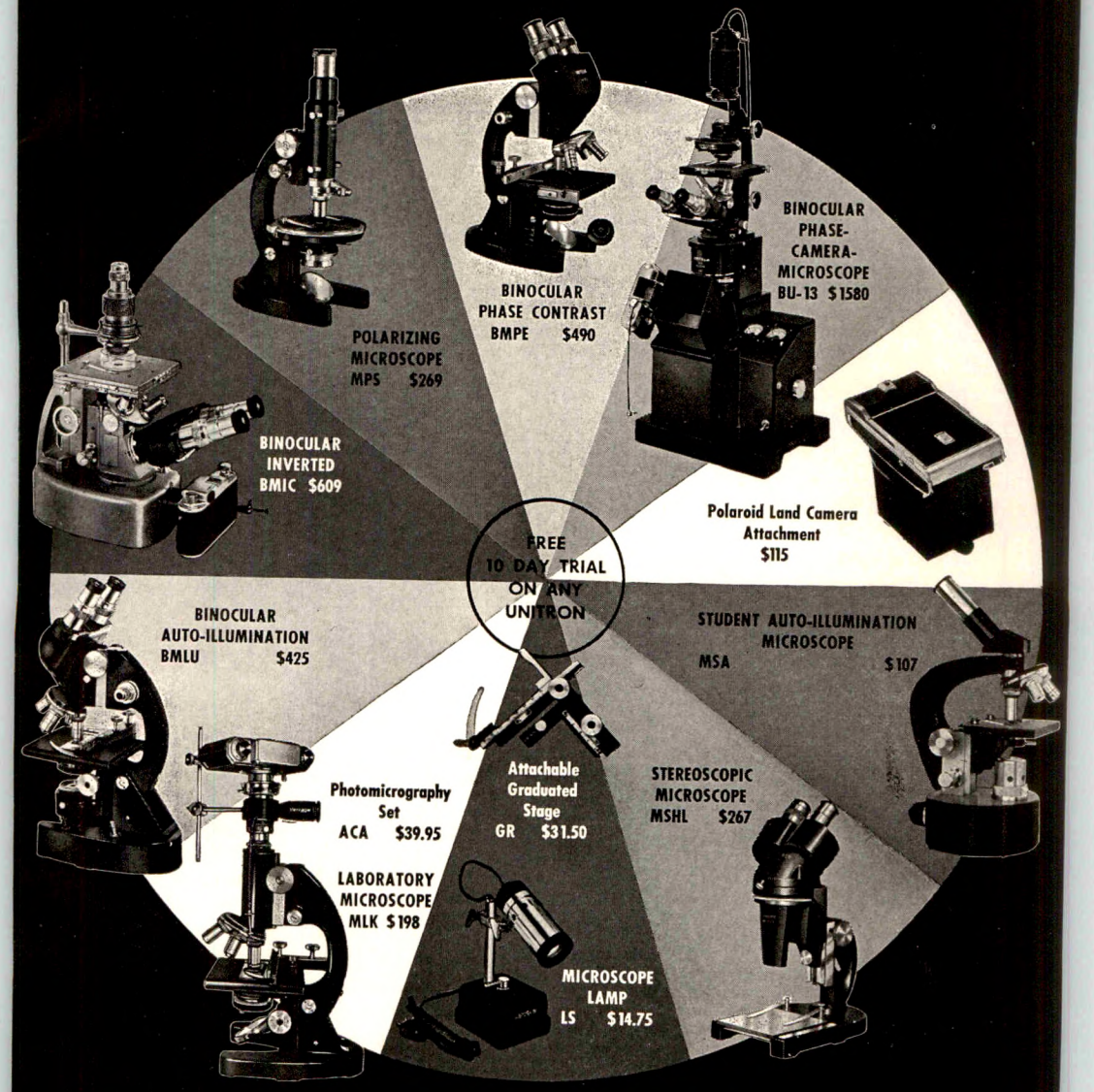
Professor Katherine Esau is internationally known as a pioneer for her contributions to the anatomy of healthy and diseased plants. The present volume is based on the edited Prather Lectures delivered at Harvard University in 1960. It highlights some of the author's research work on the movement of organic solutes through sieve tubes, the structure of the vascular tissue, and the transmission of viruses by insects.

Since the bulk of the material is based on original research conducted by the author at the University of California in Davis, it is a unique source of information on important progress in the study of plant anatomy and physiology. Of the 93 pages of text (plus 9 pages of references and 5 pages of index), 73 pages are devoted to plant tissues and the movement of solutes. The remainder —not more than 17 printed pages—is devoted to viruses and insects. In the opinion of this reviewer, the title of this book leads the reader to anticipate a different balance between the subjects mentioned in the title. The reader who is looking for information on viruses and their relation to plant tissues and insect vectors will be disappointed, especially as regards information on virus-vector interrelationships.

The disproportion between the treatment of the anatomy on one side, and the discussion of viruses and vectors on the other, reflect our lack of basic knowledge in this particular area of research. By focusing the attention of the reader on this gap of information and by a masterful presentation of the available data, the author stimulates the research-minded reader to perform more experiments. Much of the research discussed in the last pages is actively being pursued in many laboratories. Some of the concepts are still controversial and others have been changed on the basis of recent findings. The earlier belief that highly virulent plant viruses are harmless to insects is no longer held as several plant viruses have been found to be insect-pathogenic.

The size and organization of this slim volume make it very readable. The detailed discussion of translocation of organic materials begins with an interesting historical treatment of early investigations, followed by modern up-to-date experimental work of Professor Esau and others. In this lucid presentation the book is excellent. Concrete data, good illustrative material, inclusion of important references and of an index amplify the general excellence of the material. Perhaps the pioneer work of Ivanowski would have been more properly dated 1892, when his first

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classic paper on tobacco mosaic appeared. The quoted paper, published in 1903, was among his last contributions to this subject. This minor criticism should in no way detract from the usefulness of this book. Its main value lies in generating ideas for experiments that could be used to test some of the hypotheses presented in this scholarly contribution, and perhaps settle some of the existing contradictions. In addition to scientists and research workers, who will find this work to be of great importance in their research, teachers will also be interested in the book for possible use in courses in plant pathology, entomology, and virology. In spite of its small size, the book fulfills a definite need, and it will contribute to widening the understanding of a fundamental subject in virus-host interrelationships.—*Karl Maramorosch.*

Bird Study by ANDREW J. BERGER; 389 pages; $9.00; John Wiley & Sons, 1961.

The subjects included in Van Tyne and Berger's *Fundamentals of Ornithology*, 1959, are covered here, modified so that the book might "serve as the basis for a one-semester course in ornithology for liberal arts students," which it should admirably do. The topics are differently arranged and lack Food and Feeding Habits (barely mentioned), Social Relations, the World's Bird Families and the Glossary; Field Identification, Bird Habitats and Conservation are added. This last is broad in scope and exceedingly important. Its facts and philosophy should be forcefully brought home to all naturalists, farmers, ranchers, legislators, and especially to officials and staffs dealing with wildlife, agriculture, land use, and natural resources. It is of vital importance to *everyone*. "The emphasis in this book is on the living bird and primarily on North American Birds," but technical aspects are not neglected and exotic birds are introduced when apposite. The numerous illustrations are excellent.

By way of correction: More than "A few" birds, including all albatrosses, bustards, oystercatchers, alcids, most plovers, and scattered others have but three toes; many birds have a prenuptial molt, and this, when partial, may involve but a part of the body plumage and may involve wing and tail feathers. The statement, "After nesting in northern Alaska, Sanderlings . . . fly to southern Argentina" will mislead those who do not know that Sanderlings do not nest in Alaska and do winter as far north as British Columbia and Massachusetts; I do not believe that the individuals that reach Argentina cross the Andes, as the map indicates. Wheatears of Labrador would seem to cross the Atlantic Ocean as do Arctic Terns, and several other species breeding in arctic Canada and Greenland cross to and from Europe, but possibly by routes north of the Atlantic. There are other inaccuracies.—*Charles H. Rogers*

Cold Spring Harbor Symposia on Quantitative Biology, Vol. XXV: Biological Clocks; 524 pages; $8.00 to individuals; Long Island Biological Association of the Biological Laboratory, Cold Spring Harbor, Long Island, N. Y., 1961.

Some remarkable recent findings, spiced with a bit of controversy, have projected the study of periodic phenomena in organisms into current prominence. The 1960 Cold Spring Harbor Symposium brought together, for the first time, a distinguished international collection of clock-watchers, and resulted in a volume of papers which can be divided roughly into two parts—the first concerned with the nature and physiological identity of biological time-measuring systems, the second dealing instead with the role of such clocks in situations (e.g., photoperiodism and navigation) which require chronometry on the part of the organism.

Each of these categories has had significant recent shifts of emphasis and interest. Most workers in the first area now consider it unlikely that an unidentified environmental stimulus is responsible for the timing of circadian

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(about 24 hour) rhythms, and have embarked upon the search for the locus of endogenous timing mechanisms. Hopeful results are reported from studies on single cells, by Ehret and by Sweeney and Hastings; on insects, where Harker has located an endocrine control over the rhythm for locomotor activity of the cockroach in the subesophageal ganglion; and from a variety of organisms by Pittendrigh and Bruce, whose results suggest that a careful operational analysis of the entrainment and control of circadian rhythms is more fruitful at the moment than fractionation. Many of the other papers in this section, however, are difficult to evaluate because the authors so fiercely eschew the presentation of raw, untreated data.

The second area—that of chronometric performance—is noteworthy especially for a welcome spate of papers on bird and fish navigation by a predominantly young group of German workers. These emphasize the current feeling that, in homing, a variety of organisms may employ celestial factors only for the detection of compass direction, and that further unknown cues must be postulated. Some interesting studies on photoperiodism in various organisms are also reported, that by Lees being especially exciting.

The arrangement of topics is not always rational, but this is probably the fault of the discipline and not of the organizers. In general, the volume is better done than the majority of symposia, and will be an essential document for anyone interested in rhythmic phenomena. One complaint: anyone else sick of symposium "discussions" which aren't discussions at all? I'd like to read what the man's reply was at the time, not what he thought up after doing two more experiments and consulting a friendly statistician!—*Donald Kennedy*

International Review of Cytology, Vol. X, edited by G. H. Bourne and J. F. Danielli; 409 pages; $13.00; Academic Press, 1960.

This series is a standard reference for cytologists. The editors have, over the years, chosen to let the field represent itself in full. This volume in no way departs from that policy. Consequently the individual reviews vary greatly in character, quality, and scope.

The contributors to this volume and the titles of their reviews are: Kasten, the chemistry of Schiff's reagent; Sharma and Sharma, spontaneously and chemically induced chromosome breaks; Wischnitzer, the ultrastructure of the nucleus and nucleocytoplasmic relations; Wolpert, the mechanics and mechanisms of cleavage; Doljanski, the growth of the liver with special reference to mammals; Nagatani, cytological studies of the carcinogenic azo dyes for cytoplasmic components; Matoltsy, epidermal cells in culture. Also included is a cumulative index through the first nine volumes.

Kasten's review is outstanding; it is 100 pages long and is thorough and critical enough to have been published by itself as a monograph. The review by the Sharmas is a discussion of ways that some cytologists interpret chromosome breakage. Wischnitzer's article is a thoughtful summary concerned solely with electron microscopic studies of the nucleus, a cellular element that is somewhat refractory to electron microscopic study. Wolpert reviews the assorted theories that have been put forth to explain how cells go about the business of dividing; he argues eloquently for his own. Doljanski considers liver growth from the point of view of humoral control of a cell population. Nagatani has submitted a comprehensive review of carcinogenic dyes and the use of histologic methods in finding the intracellular dye-binding sites. Matoltsy compares *in vivo* with *in vitro* epidermal cell culture and discusses how he uses this method to study unorganized and organized tissue formation.—*R. C. von Borstel*

The Plant Community by H. C. Hanson and E. D. Churchill; 218 pages; $4.95; Reinhold Publishing Corp., 1961.

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By W. ROBERT BAILEY, Ph.D.; and ELVYN G. SCOTT, M.T. (ASCP). Published February, 1962. 327 pages, 6½″ × 9½″, 15 illustrations. Price, $6.50.

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By WILLIAM C. BEAVER, Ph.D., D.Sc. Published January, 1962. 6th edition, 299 pages, 7¼″ × 10½″. Price, $3.75.

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reader, *The Plant Community* is brief, concise, and couched in clear, simple language. Proceeding from the ecology and grouping of species and populations to community analysis, dynamics and classification, it concludes with a principle that applies to all science from mathematics on—namely that method should be based upon the end in view.

A brief dictum on p. 80 gives the clue to the writers' attitude: "no investigator has the right to assume unimportance in Nature." This is an effective answer to those critics who consider the study of plant communities as either outmoded or a mere emphasis of the obvious. It is also a justification for the meticulous processes which must be employed in community description and interpretation.

Aside from a valuable and comprehensive bibliography with precise references in the text, one finds much-needed bridges between genetics and ecology and among the various schools of thought and practice. There is also a gentle warning to Americans who tend to neglect important European developments in methodology.

Format and printing are convenient and attractive. Most of the pictures are clear and effective. Typographical errors are remarkably few and insignificant for a first printing. Oversights that might well be corrected in revised editions are, for example, the failure to cite Oosting's valuable book on the study of plant communities and, apropos of cyclic changes, the work of Raup. There are also instances in which concise, didactic statements such as one attributing the greater success of certain species to superior competitive ability come close to an unintended tautology.

The very compactness of the book gives play for abundant expansion by lecture and discussion, since the principles set forth are of wide application. While a majority of the photographs are from western United States, some two-thirds of the references are either general or non-western, relieving the authors of any charge of provincialism.

For the general reader a simple introductory statement that the distribution of plant life, however complex, is not haphazard, would be most helpful. I have long regretted that the invaluable volume of Weaver and Clements begins with the dictum that vegetation is the sum total of plant life in an area—a partial truth which obscures the all-important matter of form and organization. The whole point of community study lies in our experience that interrelations tend to become factorial, rather than merely additive.—*Paul B. Sears*

Comparative Plant Anatomy. (*A Guide to Taxonomic and Evolutionary Application of Anatomical Data in Angiosperms*) by SHERWIN CARLQUIST; 146 pages; $5.00; Holt, Rinehart and Winston, 1961.

The book is an excellent guide to the literature in the field. It exemplifies use of anatomy for clarifying taxonomic problems and summarizes trends in the evolution of anatomic features. The author emphasizes both the usefulness and the limitations of anatomic information for taxonomy and recommends methods of study of anatomy and of presenting results most appropriate for taxonomic and evolutionary considerations. The book is clearly written, the terminology carefully handled, but, as the preface states, knowledge of basic principles and terms in plant anatomy is presupposed.

Most appropriately, the first chapter discusses the difficulties in taxonomic use of anatomic information caused by the variability of plant features and reviews the sources of this variability. It concludes with an illuminating evaluation of anatomy in relation to phylogenetic interpretations. Nine chapters summarize anatomic features categorized according to types of cells, tissues, and plant organs. Each exemplifies the use of anatomic features with regard to taxonomy and phylogeny. The discussions on trichomes, xylem, nodal anatomy, and vascular anatomy of the flower are particularly successful.

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skillfully assembled into phylogenetic diagrams. Photomicrographs are less numerous but well chosen. Each chapter is generously documented by appropriately selected references.

Errors and inapt statements are few. For example, figures 9-1 and 9-2 are transposed; figures under 7-3 are trimmed too severely to show all of the features cited with reference to them; monocotyledons are said to lack secondary xylem (p. 45); lignification is used to imply wall thickening (p. 60); the misleading reference to cambium in vascular bundles of monocotyledons is accepted without question (p. 111). Most regrettable is the use of "monocot" and "dicot" giving a touch of negligence to the otherwise excellent writing.

The book is unique in its approach, generally highly accurate, interestingly written, and well documented. The hope of the publishers that it will serve not only mature students but also teachers and researchers should be fulfilled.—*Katherine Esau*

Morphology of the Angiosperms by Arthur J. Eames; 518 pages; $13.50; McGraw-Hill Book Co., 1961.

There has not been an English-language text on this important subject since the advent of Coulter and Chamberlain's book by the same title in 1903. Obviously, during the 20th century much new factual information has been obtained which has been responsible for the formation of many new morphological concepts as well as the modification of numerous theories on the nature of angiosperm organs, their origin, and evolution. This book is comparative in approach rather than purely descriptive, and is intended as a text and comprehensive reference volume for advanced students and teachers of botany.

The first ten chapters are concerned with almost all aspects of the morphology and anatomy of both vegetative and reproductive organs of the flowering plants. Chapters 11 and 12 (totalling almost 100 pages) represent a welcome departure from the conventional approach; Chapter 11 includes discussions in considerable detail of the research results of many workers on the comparative morphology of primitive families of dicotyledons, viz., in the orders Ranales, Dilleniales, Piperales, and Casuarinales, and of the primitive monocotyledonous families in the Helobiales, Liliales, and Palmae. The treatment of each family is followed by an extensive and up-to-date bibliography. Chapter 12 concerns the phylogeny of angiosperms, including a valuable summary of the various theories of their origin from several gymnosperm taxa.

Undoubtedly some who will use this book will wish that the author had employed a method of direct literature citation which would enable the reader to check more readily the original papers concerned. Minor objections also may be forthcoming to the Germanic compounding of such words as leaf base (pp. 6–8).

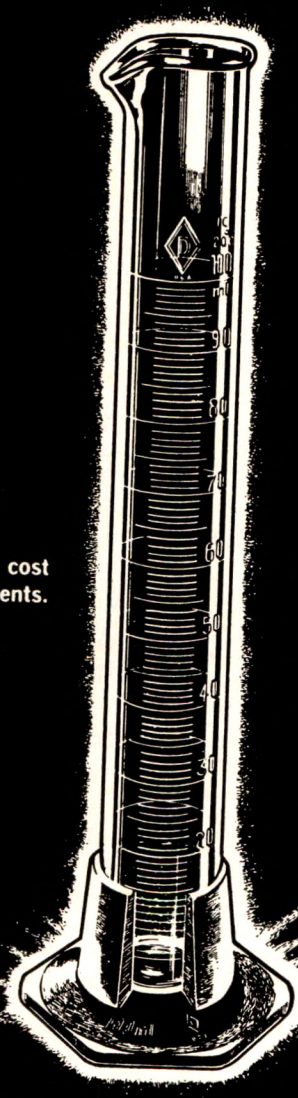
However, these are relatively unimportant criticisms and should not detract from the over-all value of this excellent volume, which should prove to be an indispensable reference work for all professional botanists for at least the next half century.—*James E. Canright*

Grasses of Burma, Ceylon, India and Pakistan (excluding Bambuseae) by N. L. Bor; 767 pages; $25.00; Pergamon Press, 1960.

In this book, Dr. N. L. Bor draws from over thirty years experience, much of it in the field, to treat the more than 1200 species of grasses (excluding bamboos) found on the vast Indian subcontinent. The clearly worded and ample keys and eighty excellent line drawings compensate for the lack of descriptions, and the book appears to achieve its aim, which is to provide in a volume of reasonable size the means of determining the correct names of the area's grasses. Numerous comments are also provided on the uses, nomenclature, distribution, chromosome number, and other features of individual species, and the special care taken by Dr. Bor is evident in the accuracy

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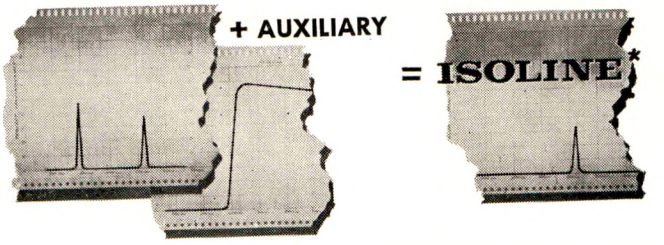
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of his citations; full synonymy is provided for each species. The introductory chapters are of particular interest, and deal with such useful topics as the rather complicated morphology of the grass plant and the uses of grasses.

The systematic section of this book should be of special interest to United States agrostologists because it contrasts so strongly with the arrangement found in the standard manuals for this country. As Bor points out, we are at the moment witnessing a revolution in ideas about the taxonomy of Gramineae. Much of the earlier work on the family has been concerned with artificial, easily observed characteristics; modern approaches are based on many anatomical features and on the chromosomes, and often dictate major revisions in the classical system. In trying to assimilate some of this recently available information, Bor utilizes many of the ideas of his distinguished colleague, C. E. Hubbard, and consequently, many of the tribes and genera are more finely drawn in this book than is customary in the more conservative North American works.

In summary, this is an outstanding contribution to the study of a very important family of plants and covers a large proportion of the species of that family. As a result, it deserves a place in every botanical library.—*Peter H. Raven*

Principles of Nematology by GERALD THORNE; 553 pages; $15.00; McGraw-Hill Book Co., 1961.

Once the study of the nematodes and the flatworms fell under the subject of helminthology. It was N. A. Cobb, unquestionably this country's most eminent "nematologist," who proposed, early in this century, a new branch of science which he called "nematology" to encompass the free-living and plant parasitic nematodes. Thus, "helminthology" took on a more restricted usage to cover the nematode parasites of animals and the trematodes and cestodes, two groups which are exclusively parasites of animals. The terms, "nematology" and "helminthology," have become widely accepted by specialists but are still unnatural in their restrictions and, as such, always have to be carefully defined to beginners. In this respect, the choice of the title, *Principles of Nematology*, for Gerald Thorne's remarkable book seems a bit unfortunate. A title such as "Plant Nematology," or more grammatically "The Nematodes of Plants," would have been more helpful in suggesting the subject covered by this text.

This textbook and reference on the nematodes parasitic on or commonly associated with plants gives biological and agricultural sciences their first real and long overdue textbook on this subject. Many more specialists will have to be trained in this field in the next decade to help combat the agricultural losses caused by the ubiquitous plant nematodes. Until now, the student in nematology or plant pathology had to resort to rather specialized monographs such as *An Introduction to Nematology* by B. G. and M. B. Chitwood and to research papers in technical journals, which are tough going before a reasonable background in the subject has been acquired. None of the existing textbooks of plant diseases and pathology treat the nematodes adequately. Only the recently published bulletin by J. R. Christie, *Plant Nematodes, Their Bionomics and Control*, comes close to satisfying the need now filled by Thorne's new book.

Just thumbing through *The Principles of Nematology*, one immediately becomes aware of the craftsmanship and knowledge that the author has put into it. The bulk of the text takes up the nematodes parasitic on or commonly associated with plants, in taxonomic order. Each taxonomic subdivision, starting with the order Tylencha and working down through superfamilies, families, subfamilies genera and species, is described in detail. The nomenclature is handled very carefully with the excellent typography assisting in the comprehension of these intricacies. Synonymy is also handled carefully. Generic and species descrip-

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tions are complete and precise so that no further reference is necessary for identification unless a new species is involved or a free-living species not covered by this text.

The illustrations deserve special mention. Not only are they a pleasing addition to the text, but they are models of craftsmanship and add immeasurably to the identification of species. They set a standard for students and specialists to strive for in their own work, and, to assist the reader in achieving these standards, one of the outstanding chapters of the text describes in great detail the method of preparing drawings of this type.

Each species that is an important cause of plant injury receives special attention. Its habitat and distribution are described. Details regarding its biology and attack on plants are given when known. Finally, methods of control are discussed.

A plant pathologist or agronomist might prefer to place primary emphasis on the disease and consider the causative agent as a secondary factor. However, in training a specialist in the field of nematode diseases of plants, Thorne's approach is probably superior and was undoubtedly used deliberately with this objective in mind. Thorne is convinced, and no doubt is correct, that the subject of "nematology" is increasing rapidly in importance and will continue to advance in the future. We must have more specialists in this field to develop knowledge to save crops from nematode damage. As the population pressure increases, so will the demands upon the productivity of our lands. As cropping increases in intensity, so will the potential for nematode damage.

The preliminaries of Thorne's book, which add immeasurably to its value as a text for teaching, consist of five chapters on basic knowledge of the subject as well as "how-to" chapters. These teach the student where and how to look for plant nematodes; how to collect and prepare them for study; how to identify known species and describe new species; and how to prepare technical drawings. The chapter on the history of nematology is of particular interest since Thorne's own career covers the whole span of the modern development of the subject and he probably knew personally all the important workers in the field. His review and appraisal of the work of these men is informative and factual. Pictures of most of the nematologists are included. The list of references to their work seems well selected and would be very helpful to an advanced student.

This new text will be a boon to everyone teaching plant diseases; it will certainly be essentially for advanced students in the field. The price may seem a bit shocking but is not inconsistent with the time and the workmanship, both on the part of the author as well as the publisher, that have gone into this book. If you cannot afford it yourself, you should make sure your library orders a copy.—*Sterling Brackett*

Chemistry of the Amino Acids by Jesse P. Greenstein and Milton Winitz; 3 vols., 2872 pages; $100.00; John Wiley & Sons, 1961.

The field of the chemistry of the amino acids started to grow rapidly more than half a century ago. In 1906 and 1923, Emil Fischer and Max Bergmann could summarize the most pertinent advances merely by editing in two volumes the published papers from Fischer's laboratory. Such a publication was incomplete, if for no other reason than that the studies of T. Curtius were not represented; nevertheless, Fischer's volumes still contain a great deal of information and have been a useful summary for those who wanted to work in this field. Since Fischer's time, no effort has been made to survey the entire field, and today this task would be next to impossible.

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in peptide synthesis. This field did enjoy a powerful revival after the introduction of a wonderful protecting group by M. Bergmann and L. Zervas, but it was blighted by a comparative lull in the forties. Consequently, in 1949 it was possible for J. F. Fruton to write an excellent review of peptide synthesis. With the advent of the different mixed anhydride and active ester methods, and of various coupling reagents, however, there has been a tremendous burst of publications. The reviewer of this field today struggles almost hopelessly against the rapidly accumulating material and reviews are usually outdated as soon as they appear. This is the present situation, not only in the field of peptide synthesis, but also in other branches of the chemistry of amino acids. One wonders if the ultimate ability of workers in this field to maintain effective contact with the discoveries of other workers will not depend on the mechanized memory of electronic machines.

In an epoch when chemists are faced with such an information dilemma the appearance of this monumental work by Greenstein and Winitz is most welcome. Dr. Greenstein passed away suddenly in 1959 when the manuscript was nearly complete, and this treatise is a fitting memorial to an outstanding and prolific lifetime of research and creative thought on the chemistry and biochemistry of the amino acids. The work could not, of course, escape the difficulty that, while it was being written and printed, the progress of research did not stop, so that a number of important recent publications are not included. This, however, is not a serious criticism in this case, because *Chemistry of the Amino Acids* is, in the best sense of the word, a handbook, and not a series of review articles. We shall see it for a long time in laboratories in the hands of peptide and protein chemists.

The purpose of this handbook is not to bring system into a complex body of material by means of a set of definitions and generalizations. Although it contains a vast number of references (there are 2262 references in Volume 1 alone) it is far more than a critical compilation of abstracts. For one thing, the reference material is dealt with in such a fashion that it is not necessary to bring life into it by looking up the original papers. Furthermore, the monograph gives the historical background and discusses the logistics of the development of different aspects of amino acid chemistry, in addition to describing experimental procedures and tabulating and graphing great masses of experimental data. These features make the Greenstein-Winitz treatise a work which communicates to the reader with vitality. It will also prove to be tremendously useful in the laboratory, and will probably play a role in peptide and protein chemistry similar to that of Beilstein in organic chemistry.

The first volume begins with a relatively brief chapter on nomenclature, structure, and criteria for accepting evidence for the natural occurence of an amino acid. The second chapter devotes almost 200 pages to the problems of determining the absolute configurations of the amino acids by chemical, optical, and biological methods. Chapter 3 is an extremely interesting account of the role of amino acids in nutrition. This chapter begins with a fascinating history of the concept of protein digestion during the nineteenth century, and a description of the development of the notions of dispensible and indispensible amino acids. A detailed account is given of the discovery of the indispensible amino acids of various animals (including salmon fingerlings and honey bees!) and man. On page 299 there is a most interesting and provocative discussion of the ethical questions involved in performing nutritional experiments on human beings. The chapter closes with a discussion of the economics of providing all of the amino acid requirements of a man by means of synthetic materials. In 1956 a daily intake of 100 grams of appropriate amino acids, supplying 2000 calories, would have cost $71, and reasons are given for believing that large-scale commercial production might easily reduce the

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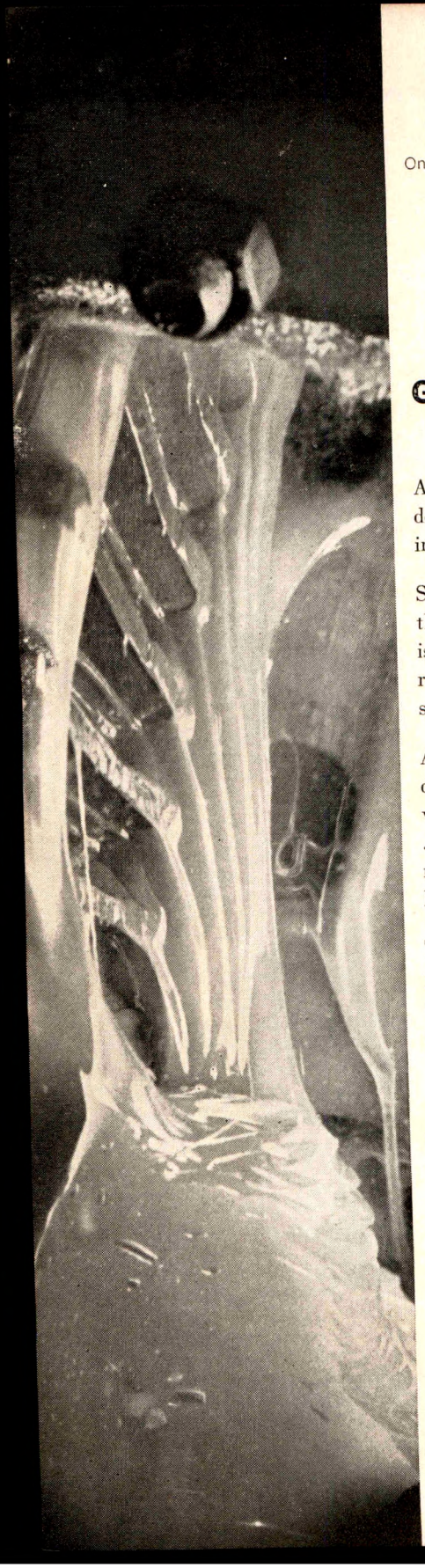
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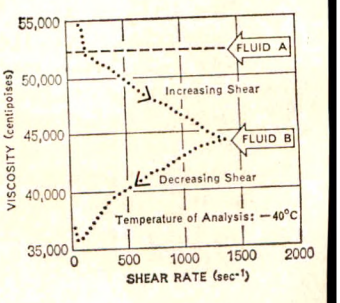
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daily cost to about $25. It is concluded that "if the current high level of development of research and technology continues, there is little doubt that some diet with all chemicals defined will become a practical reality well before the end of the earth's biological resources demand it." The next four chapters are concerned with various aspects of the physical chemistry of the amino acids: their behavior as dipolar ions, certain thermodynamic properties, complex and salt formation, and the effects of radiation. Volume 1 is concluded with brief chapters on general methods of synthesis of amino acids and the resolution of optical isomers.

In Volume 2 the book-length chapter on chemical methods for synthesizing peptides and the chapter on sequential analysis of peptides are magnificently organized and are second to no other available treatises on these subjects. Eight other chapters on analytical procedures for amino acid determination, especially those on titrimetry and chromatography, are less extensive than numerous available monographs devoted to the particular analytical methods. A chapter on enzymes employed in amino acid technology concludes Volume 2. Volume 3 is devoted entirely to detailed procedures for preparing specific amino acids.

While he recapitulates what he already knows and learns much that he did not know, the reader of these volumes cannot fail also to be entertained. The personal element often intrudes, since a number of brilliant methods and quite a few important findings that are reported in detail come from the author's own laboratory. This adds much to the pleasure of reading this work. The colorful quotations from the papers of Fischer and others (all given both in the original language and in translation) also help to make the reading of these impressive volumes really enjoyable.—*M. Bodanszky, W. Kauzmann*

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Gas Chromatography (Second International Symposium Held Under the Auspices of the Analysis Instrumentation Division of the Instrument Society of America, June, 1959), edited by H. J. NOEBELS, *et al.;* 463 pages; $16.00; Academic Press, 1961.

The large number of monographs available on gas chromatography, in view of the short history of the subject, attest to the importance of this analytical method. The books which have been published fall into two categories. First are those which survey the field, serving as introductions or guide books to those who wish to use the method. Second are a number of collections of papers presented at meetings and symposia on gas chromatography. *Gas Chromatography*, a representative of the latter type, is a record of the proceedings of the Second International Symposium on Gas Chromatography, held at Michigan State University. Thirty-four research papers covering various aspects of the subject are presented. The previously published proceedings for the first conference contained a bibliography of 443 references to the literature through 1957. This commendable precedent is continued here; an additional 1532 references through June 1960 are included in an appendix. Unfortunately, the subject index does not cover these papers, making effective use of this bibliography difficult.

The appeal of a volume such as this is chiefly to the specialist in gas chromatography, especially since about a third of the papers deal with specific applications and an additional number of papers consider details of changes in operating variables or in instrumental design. It is recommended, however, that the occasional user of gas chromatography become acquainted with this volume, since the variety of subjects treated insures the probability that investigators will find at least several to be of interest. For example, A. T. James presents an anecdotal account of the early development of gas chromatography. The editors would have done well to have begun the volume with this short history. Methods for identifying components present in mixtures of totally unknown composition are outlined; theory and practice of large-scale gas chromatography are presented and there is an extensive collection of retention-time data for different classes of compounds on different columns. Detectors are the subject of a number of studies and high temperature operation is discussed in several papers.

Collections of research papers on subjects such as gas chromatography are welcome, even though the useful life of such compendia is shorter than the more general treatments. The publishers would assist matters if the lag between presentation and publication could be cut shorter than the nearly two years needed for the appearance of the present volume.—*Paul Schleyer*

X-Ray Analysis of Organic Structure by S. C. NYBURG; 434 pages; $13.00; Academic Press, 1961.

The science of X-ray crystallography has advanced very rapidly in the last two decades and has such diverse applications in the study of solids. As a result, it is exceedingly difficult for one to incorporate in a book of reasonable size even the minimum fundamentals needed for scientific workers in this field, both academic and industrial. Nyburg's book is an apt illustration of it. The book is mainly addressed to these organic chemists and biochemists who wish to understand the subject of X-ray structure analysis without going much into the mathematical aspects. The conscientious readers will, however, find this tour disappointing.

The book is divided into nine chapters. The first six deal with the methods of analysis and the last three are a survey of organic materials examined. The various crystalline materials have been grouped as: low molecular weight, high molecular weight, and fibrous, and only such analyses as give an idea of the likely molecular configuration and packing are included. As the author has stated in the preface, the funda-

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mentals and experimental methods are not in any sense directed either to the crystallographer or one intending to be one. But what disturbed the reviewer is that even the various classical methods which are still extensively used for obtaining X-ray photographs and their interpretations have not been adequately dealt with. The complete absence of crystallographers' "cherished" concepts of reciprocal space is, indeed, disturbing even to an organic chemist in this field. This criticism should not, however, take away the merits of the book. The chapters on the crystalline and fibrous macromolecular substances have been succinctly written and give a comprehensive and critical survey of the architecture of giant molecules as revealed by X-ray diffraction. The reader will find the Index of Crystal Structures at the end of the book very helpful for reference. On the whole, in the reviewer's opinion, the book is very useful for the ever-increasing number of industrial scientists who wish to use this technique as an analytic tool and it certainly justifies its place on their shelves.—*V. D. Gupta*

The Physical Principles of Astronautics by ARTHUR I. BERMAN; 350 pages; $9.25; John Wiley & Sons, 1961.

The launching of satellites and space probes has resulted in renewed interest in the celestial mechanics of the solar system. This book is an example of this renewed interest. It is divided into three parts. The first part has as its aim to introduce the reader to the physics and astronomy of the solar system. In the sixty pages which are devoted to this, it is clear that the author can only touch on a few topics. Besides some introduction to the mechanics of the solar system, this part consists mainly of a description of the earth, moon, planets, and, to a lesser degree, the sun. Everything considered, the author does quite well in this description. Many recent results (and speculations) are included and the details are, for the most part, as accurate as space will allow.

The second part of the book is a straight-forward development of the elementary mechanics which is needed in order to discuss motions in the solar system. A very nice feature of this development is the use of vector notation which emphasizes the vector character of such quantities as force, velocity, and angular momentum.

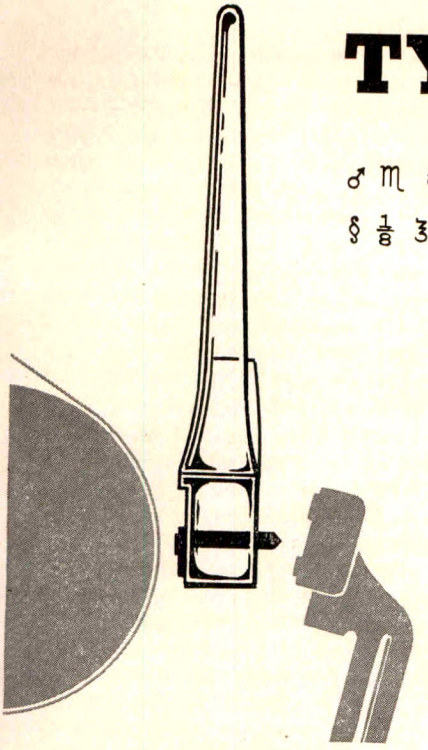
In the first chapter of part three, the two-body problem is worked out in detail. The derivation of Kepler's laws of motion from Newton's equations and the universal law of gravitation (or vice versa) can be followed through without the necessity of working out many intermediate steps or consulting other texts. Even the relevant analytic geometry of conic sections is derived. Following this, the trajectories of space probes from the earth to other planets (with Mars as an example) are discussed in the framework of two-body solutions. The remaining chapters discuss perturbations of two-body orbits and the dynamics of a rocket.

Among the ten appendices are a discussion of vector calculus, a description of rocket propulsion methods, a glossary of astronautical terms, tables of physical and astrophysical constants, and a bibliography. Many illustrative problems are solved in the metric system of units. Additional problems are included at the end of each chapter.

This reviewer concludes that this is a good book. The fact that it is easy to read and self-contained makes it useful for the scientist or engineer who wishes to obtain an introduction to astronautics. The detail with which the two-body problem is discussed yields many convenient formulas for the person who wants to compute a numerical answer quickly. This book also seems well suited as a text for an introductory course in celestial mechanics or astronautics. Finally, the illustrations and text are printed in a very attractive way which adds to the pleasure of reading the book.—*Robert E. Danielson*

From Dualism to Unity in Quantum Physics by ALFRED LANDÉ; 114 pages; $3.75; New York: Cambridge University Press, 1960.

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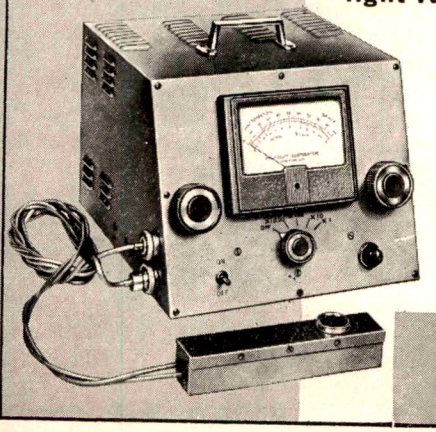
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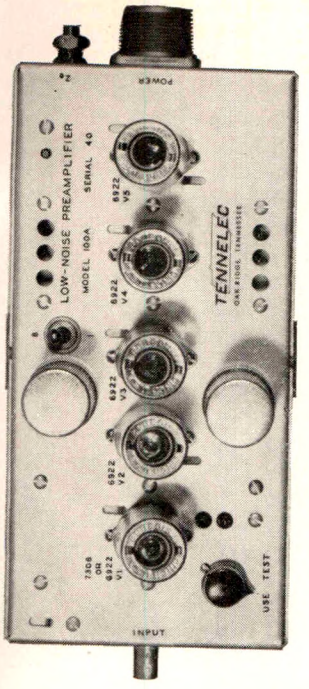
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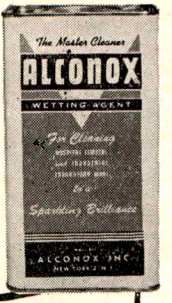
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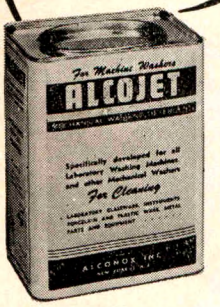


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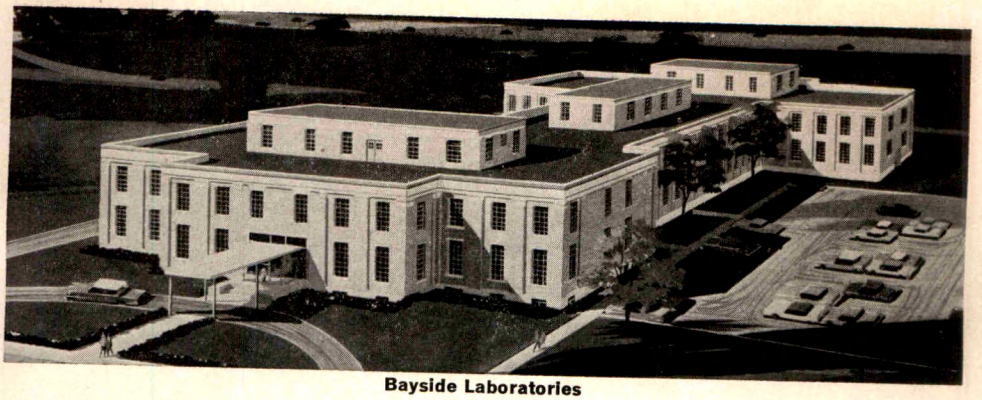
Galapagos (The Noah's Ark of the Pacific) by IRENAUS EIBL-EIBESFELDT; 192 pages; $3.95; Doubleday & Co., 1961.

This little book, directed to the general public, tells about the observations and adventures of the author during his two visits to the Galapagos, an archipelago famous for its unique fauna.

Since there are no terrestrial predators on the Galapagos, most animals are remarkably tame and can be observed at a very close distance. It makes an evening of delightful reading to see, with the author, the sea lion bull soothing two quarreling members of his harem, to watch the ceremonious tournament of two male iguanas, the courteous greeting ceremony of a couple of flightless cormorants, and many other fascinating features of the abundant animal life. However, the book contains more than just entertaining stories about the strange behavior of animals. The author, a noted ethologist, uses many of the observed instances to illustrate general principles of biology and animal behavior.

Above all, this book is a passionate plea for effective protection and conservation of the fauna of these islands. Due to their tameness, many species are threatened with extinction resulting from persecution by man and his followers. It is good to hear that thanks to the tireless efforts of the author a "Charles Darwin Foundation for the Galapagos Islands" has been established under the protectorate of UNESCO. The aim of this foundation is to establish a permanent biological station on the Galapagos. Such a station would not only facilitate scientific work on the islands, but would also grant the enforcement of the existing protection laws. We would thereby avoid the irreplaceable loss of the fauna on a group of islands that Darwin has called "the living laboratory of evolution."

The book has lost none of its freshness and appeal in the translation from the German by A. H. Brodrick. It is illustrated with many outstanding pho-



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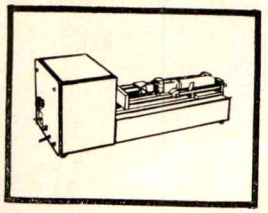
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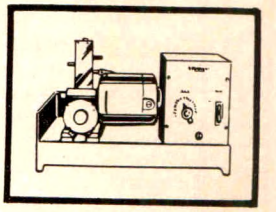
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tographs by the author, which deserve better reproduction than that afforded by the publishers.—*Klaus Hoffmann*

Geology of the Atlantic and Gulf Coastal Provinces of North America by GROVER E. MURRAY; 692 pages; $24.00; Harper & Brothers, 1961.

The Atlantic and Gulf coastal provinces extend as low land and shallow seas for some 4000 miles from the Newfoundland Banks to British Honduras, with breadth of 100 to 600 miles. Geologic interest is in the underlying, generally seaward-thickening mass of sedimentary rocks above a basement of more indurated and crystalline rocks. The great volume of sedimentary rocks reaching several miles thickness form coastal geosynclines that developed through the late Mesozoic and Cenozoic eras. The writer, professor in Louisiana State University, has extensive experience on the Tertiary of the Gulf Coast, though he began his studies in North Carolina, along the Atlantic.

The magnitude of compilation can be appreciated in that more than ten thousand published articles have treated the geology of the region; some three thousand are listed in the closing bibliography of nearly one hundred pages.

Of the nine chapters, the first discusses briefly the highlands adjacent to the Coastal Plain and the rocks underlying the coastal sequence. The second chapter on the "Pre-Coastal Province Rocks," on the basement underlying the sediments, is an exceptionally fine summary of the literature dealing with information from wells and interpretations that have come from geophysical investigations. The main contribution of the text is in the succeeding chapters on structural geology, faults and fault systems, salt structures, and regional stratigraphy. More than thirty separate elements or provinces are summarized in the ninety pages on structural geology, each with a synthesis which is necessarily brief. An interested reader will pursue his inquiry on any of these regions more deeply by consulting the many author-year references lists, full

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titles being in the appended bibliography. The author introduces the terms posiment and negament, useful abstractions of positive and negative elements; forebasin seems an unnecessary synonym for foredeep. Succeeding chapters on faults and fault systems, and on salt structures consider two most interesting and economically significant structural phenomena.

The chapter on stratigraphy discusses Jurassic and younger sediments in about twenty time-rock categories, principally stages, each being covered over the whole region, with selected references for each stage. Correlation diagrams have sketched continuous correlations; lack of horizontal time datum-lines is unconventional, though allowing greater freedom in symbolism and lettering. Several original restored sections greatly aid in comprehending lateral relations of time-rock and lithic intervals. More than sixty photographic reproductions are assembled, though with limited integration with the text.

This synthesis of the geology of such a long coastal belt enables a reader to gain substantial understanding of the whole and to proceed into significant literature on selected topics. It brings material into comparison that is generally treated rather provincially. The book is timely. In the past decade wells have penetrated deeper, but even greater insight has come from the application of techniques of geophysics. Evidence is now sufficient to present a rather good picture of a whole rather than of only a shallow slice. The volume brings this knowledge into a condensed and balanced treatise.
—*Marshall Kay*

Basic Concepts of Historical Geology by EDGAR WINSTON SPENCER; 499 pages; $8.50; Thomas Y. Crowell Co., 1961.

History of the Earth by BERNHARD KUMMEL; 610 pages; $8.75; W. H. Freeman and Co., 1961.

Among the several new texts on historical geology Spencer's *Basic Concepts of Historical Geology* and Kummel's

History of the Earth are basically different approaches, aimed at a different group of students and imposing a different task on the instructor.

Spencer's book, the more radical departure, is for first-course students without a background in natural science. Its intent is to impart something of the geologic record while emphasizing selected topics that illustrate modes of scientific investigation.

Extended introductory chapters (135 pages) discussing geologic time, correlating the rock record, uniformitarianism and modern environments of deposition, fossilization, and the morphology of major fossil groups guide the reader through much material generally left to the instructor.

A laudable excursion into the dawn of Earth History (50 pages) leads the reader into territory of growing importance in geology—the origin of the Earth, its hydrosphere and atmosphere, and a relatively detailed review of the Precambrian rocks of North America.

The long central section (200 pages), exploring the geologic history of the United States, reviews the Cambrian to Pleistocene record. Each chapter elaborates some topic such as temporal transgression of rock units, evaporites of the Silurian, and detailed paleogeography of the Jurassic.

Evolution, sketches of trends among some groups of invertebrates, and a brief history of the vertebrates comprise the short (60 pages) final section.

A study set of five colored geologic maps is appended.

In line with the current trend, the book is handsomely illustrated with many maps, line drawings, and photographs. Regrettably, few of these are referred to in the text.

Such a pioneer venture involves problems of presentation and organization. The style is loose and informal. Errors of fact mar some of the discussions and several typographical errors slipped past the proofreaders. A few topics are not well integrated into the whole picture. Although the descriptions of modern depositional environments are most welcome they constitute a poorly-organized chapter. The discus-

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sion begins with deep sea sediments (seldom found in the geologic record) and omits most nonmarine environments which are reconstructed in detail in chapters on Triassic, Jurassic, and Cenozoic history.

Despite these lapses Spencer's text is a noteworthy attempt to bring to the beginning student up-to-date information, examples, and ideas about the wide range of intriguing topics in historical geology.

Kummel's *History of the Earth* is organized in rather conventional form, but broadened to include all continents. It is intended for the serious beginner who can digest a wealth of supporting material, and to complement lectures by providing a world-wide view.

Like its companion in review, it is extensively illustrated, but here the illustrations are consistently referred to in appropriate places.

The introductory chapters (69 pages) on geologic time, the fossil record and a brief statement concerning evolution, and the nature of sedimentary rocks and the framework of continents are among the best in introductory texts. But they are essentially short summaries of the data; little is said about how a geologist works with these records.

There is no concern with the Earth's origin or its early seas and atmosphere in this text. Kummel turns directly to the Precambrian records of all the continental shields. This skimpy review (16 pages) omits the Belt and Grand Canyon systems and tells little about Precambrian history or major problems.

The bulk of the text is a competent, systematic, chronological review of the Paleozoic (121 pages), Mesozoic (110 pages), and Cenozoic (118 pages) eras, each covered in chapters on the physical history of North America, the physical history of the world outside North America, and the organic record. Numerous accompanying maps and diagrams emphasize the interplay between mobile and stable parts of the continents. Excellent correlation charts are assembled in an appendix.

The concluding chapter on the Pleistocene Epoch (36 pages) is devoted

mainly to a discussion of evidences of ancient climates and a fine summary of the history of Ice Age man.

Within this coverage is inserted a most welcome chapter on Gondwana formations (20 pages), with brief discussions of continental drift and paleomagnetism.

Kummel's text is well organized, very thorough, and well edited, with a strong bias toward the paleontologic record. The result is a synthesis of observations and conclusions which by its very world-wide coverage fails to develop a depth of discussion on selected topics. But surely such new material as the description of the Tethyan region, Gondwanaland and the Alps, and the admirable review of the evolution of the vertebrates will stimulate students to further reading. For this reason the book deserves a better bibliography.

"History of the Earth" sets a new standard for the traditional treatment of historical geology and fulfils the need for a text dealing with continental development of the whole Earth, but it does not meet the growing demand for penetrating discussions of principles and concepts of Earth History.—*F. B. Van Houten*

Mechanics by L. D. Landau and E. M. Lifshitz; translated from the Russian by J. B. Sykes and J. S. Bell; 165 pages; $6.50; Addison-Wesley Publishing Co., 1961.

This book is volume one of the nine-volume "Course of Theoretical Physics" written by the same authors. The subject matter is concerned solely with the dynamics of particles and of rigid bodies and the method of development is deductive. The presentation is based throughout on two general principles, namely on Galileo's relativity principle and on Hamilton's principle of least action. Problems are given at the ends of all chapters, and, indeed, at the ends of most sections. The solutions of all problems are sketched in the text.

The authors and translators are to be congratulated for providing a beauti-

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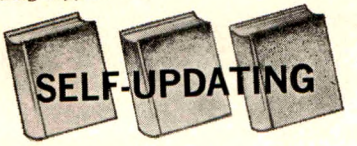
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fully clear and succinct presentation of Newtonian dynamics essentially as it was developed by Euler, Lagrange, Hamilton, and Jacobi. The method of development of material and the choice of examples make this book extremely useful as a source of techniques in classical mechanics which find application in modern quantum physics. It should find wide use in American universities as a text for a course following the intermediate-level course in Mechanics. Although the developments are extremely abbreviated they are adequate and the problems supplement the text quite satisfactorily.

There were no obvious errors in the text. This reviewer would have preferred a somewhat fuller discussion of canonical transformations. Proofs given in the text are best characterized as expository rather than rigorous. The rather loose use of the word "field" to mean the potential energy, potential, or the derived force-field constitutes a minor blemish on an otherwise rather carefully worded text.—*Walter S. McAfee*

Fatigue Testing and Analysis of Results, edited by W. WEIBULL; 305 pages; $12.00; Pergamon Press Inc., 1961.

The importance of fatigue in the failure of metal structure is reflected in this important summary published for the Advisory Group for Aeronautical Research and Development, North Atlantic Treaty Organization. The eminent Swedish authority, W. Weibull, is ideally suited to present the tremendous detail represented by more than 1100 papers and over 80 texts; significantly, over half of the texts are on statistical analysis. After an outline of fatigue testing methods, equipment, instrumentation, and test specimens, the factors affecting test results and the planning of testing programs are well set forth. The presentation of results by graphical methods shows the development of increasingly sophisticated graphical procedures closely linked to appropriate equations. Finally, the methods of analysis are shown, starting with the pertinent statistical analysis

procedures, and developing the methods of handling stress-life-probability distributions.

This book not only summarizes and orders the available literature but places it in perspective so that the difficulties and the problem can be seen. Weibull is particularly well fitted to perform this compilation and his work shows clearly the skilled use of statistics and logical reasoning, backed by an almost intuitive feeling for the handling of data. Surprisingly few research workers display real facility in the handling of the highly variable data resulting from life testing. Not only is tremendous patience required to wait out the seemingly indestructible device, but the tremendous scatter is particularly frustrating to those who like their data and calculations neat and tidy. Small wonder that so many scientists prefer the experiment under careful control which can be neatly reproduced with small standard deviations.

The natural desire to reduce the problem to a series of controlled experiments becomes particularly difficult when the object being studied is complex and difficult to represent by simple laboratory specimens. Such objects take on some of the complexity of the human machine (but fortunately do not appear to offer personality problems), and the effects of interrelations must be carefully studied by planned experiments.

The book is clearly printed and well bound, but suffers from the lack of an index other than a detailed table of contents. The text has few printing errors, but unfortunately Figures 73.1 and 74.1 were omitted.

The book is essential for workers on fatigue testing of all types of materials, since the methodology is applicable in part to the study of life distributions produced by a wide variety of experimental aging conditions. The extensive chapter on analysis of results is a worthwhile discussion of applied statistical analysis.—*James H. Wiegand*

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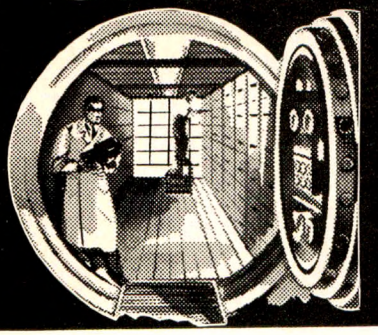
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Introductory System Analysis: Signals and Systems in Electrical Engineering by W. A. LYNCH and J. G. TRUXAL; 450 pages; $7.50; McGraw-Hill Book Co., 1961.

Once in a while a book comes along that makes this reviewer wish that he was once again an undergraduate student so that he could have learned the introductory concepts of electrical engineering in a lucid, well-motivated, intuitively-appealing manner from just such a book. Professors Lynch and Truxal are to be congratulated for having written it.

As in any introductory book nothing is said that is new or that cannot be found elsewhere; the great merit is in how the matter is presented. An introductory text must perforce be reviewed in the light of the personal experiences of the reviewer. Long ago he went through the older methods of introducing electrical engineering concepts, and then had to unlearn and relearn a large amount of material. He deplored the fact that even in full knowledge of modern developments, undergraduates were still being taught things *they* would have to unlearn as they went on to graduate studies. Consequently, he never liked to teach undergraduate courses in the prescribed manner. In very recent years there has been a sudden swing away from the older methods, but, as is often true, the result has been so-called "introductory" texts which are so abstract and unmotivated that even a competent teacher would have difficulty teaching from them. Here, at last, is a text which the reviewer would gladly use in a sophomore class.

The last chapter entitled Summary Illustrative Example is an innovation which should be used more often in texts: one illustrative problem that utilizes all the concepts and skills taught in the rest of the book.

One final word: After you get the book (of course you will), do not forget to read the Preface.—*N. H. Choksy*

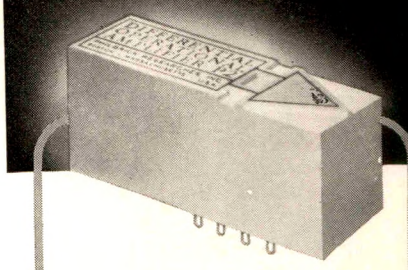
Technology, Engineering, and Safety, Volume 3 of *Progress in Nuclear Energy*, Series IV, edited by C. M. NICHOLLS; 460 pages; $17.50; Pergamon Press Inc., 1960.

This third volume in the broadly titled series of Technology, Engineering, and Safety contains fifteen separate papers arranged within four subheadings. In general, the papers are selected either for their lasting interest or for their value as review articles. A number of the latter contain extensive bibliographies, especially of British publications.

The four subheadings are: Engineering; Technology; Nuclear Safety—Criticality; and Nuclear Safety—Reactor Safety. Each section has at least one major article with the other papers either supplementing or complementing the main article. The section on Engineering contains two papers on pumps and compressors for the nuclear industry. The paper on gas bearings is particularly valuable and contains a considerable amount of useful design data. In Technology there are five papers on thermal engineering, stress analysis, and materials. They include a very useful tabulation of expressions for the calculation of thermal stresses in regular geometric bodies. Nuclear Safety—Criticality has five papers including an outstanding review of U. S. criticality accidents from 1945 through 1959. This article alone would warrant inclusion of this volume in many libraries. The last section on Nuclear Safety—Reactor Safety contains four papers on such diverse subjects as stored energy in graphite, reactor emergency shutdown devices, and gaseous effluent control. The lack of a detailed discussion of the 1957 Windscale "Technology, Engineering, and Safety" accident in the article on stored energy in graphite was disappointing. Also the article on gaseous effluent control does not consider the important subject of meteorological dispersion but considers only in-plant control measures.

The opinion of this reviewer is that this volume does represent a valuable

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addition to the permanent literature on Nuclear Reactors, particularly in the field of reactor safety. It is hoped that the editors can assemble an even more homogeneous collection of subjects for future volumes.—*Dean E. McFeron*

Microwave Antennas by A. Z. FRADIN; 668 pages; $15.00; Pergamon Press Inc., 1961.

This book is a translation of the Russian edition published in 1957. It is a fairly comprehensive study of the microwave antenna field. A good balance between theory and practice is obtained by giving mathematical derivations in outline only and omitting details of construction and mechanical design. The author draws freely on the published literature both of the U.S.S.R. and the West, although if a choice is to be made between parallel developments he chooses the Russian source. It is interesting to note that much of this original source material is in textbooks

AMERICAN SCIENTIST

VOLUME 50 SEPTEMBER 1962 NUMBER 3

PUBLISHED IN THE INTEREST OF SCIENTIFIC RESEARCH

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CONTRIBUTORS

ARCHIE CARR,

Orientation Problems in the High Seas Travel and Terrestrial Movements of Marine Turtles 359

Dr. Archie Carr, who holds a distinguished research professorship at the University of Florida (Gainesville), is noted for his most interesting studies on giant sea turtles as well as for his exceptional ability to write (e.g., *The Windward Road*). In 1955 he received the Elliot Medal of the National Academy of Sciences and in 1956 the Burroughs Award. He has conducted major expeditions to three continents, North and South America, Africa and Western Europe.

ALI BULENT CAMBEL, *Magneto-Gasdynamics: Its Science and Technology* 375

The Chairman of the Department of Mechanical Engineering and the Director of the Gas Dynamics Laboratory in Northwestern University (Evanston, Illinois) outlines in this Sigma Xi-RESA Lecture for 1961–1962 the new science of magneto-hydrodynamics that has emerged in the last decade. An important application is the generation of power which may find use in the thrust into outer space and, in the form of magneto-aerodynamic drag, in slowing down a vehicle returning to the earth's surface. The new science is a storehouse of problems for the mathematician, the biophysicist, as well as the engineer. Dr. Cambel is associated with the American Rocket Society in a number of capacities, received their G. Edward Pendry Award in 1959 for outstanding contributions to astronautics and jet propulsion. In 1960 the American Society for Engineering Education gave him the Curtis W. McGraw Award for outstanding research achievements at an early age.

M. J. ZUCROW, *Space Propulsion Engines–Their Characteristics & Problems* 409

The Atkins Professor of Engineering, who came to Purdue University's School of Mechanical Engineering (Lafayette, Indiana) in 1946 from a position as Technical Assistant to the Executive Vice President of Aerojet-General Corporation in California, specializes in the field of jet propulsion, guided missiles and rocket engines. He has received the Pendry Award in 1952 of the American Rocket Society, the Vincent Bendix Award in 1960 of the American Ordnance Association, as well as the Sigma Xi Award of Purdue University in 1956. As Sigma Xi-RESA National Lecturer for 1961–1962 he shows the possible techniques for delivering man into outer space.

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A. G. NORMAN, *The Uniqueness of Plants* 436

That plants are unique in many ways that scientists, engineers and laymen each in their own way might find difficult at first to comprehend is ably set forth by the Professor of Botany at The University of Michigan, Ann Arbor, in this 1961–1962 Sigma Xi-RESA lecture. As Director of the Botanical Gardens of the University he was recently responsible for their transfer to a new site, a rare experience, to such directors. His research interests have long been with problems basic to soil-plant relationships, soil microbiology, and plant biochemistry.

JOHN R. VAN WAZER,
Structural Reorganization Through Ligand Interchange 450

The Assistant Director and Senior Scientist in the Inorganic Chemicals Division of the Monsanto Chemical Company reached this position via a Bachelor's degree at Northwestern University and a Ph.D. at Harvard. With subsequent experience in a number of industrial organizations, he now is primarily engaged in fundamental scientific research. He shows in this Sigma Xi-RESA National Lecture for 1961–1962 the broad field of chemical research, now just beginning, where the achievements in the chemistry of carbon compounds can also be extended to the other hundred elements now known to science. He illustrates how new techniques and computers can be brought in to aid the solutions sought. Address: Monsanto Chemical Co., 800 N. Lindbergh Blvd., St. Louis 66, Missouri.

LYMAN SPITZER, JR., *The Beginnings and Future of Space Astronomy* 473

An astronomer with a Bachelor's degree at Yale, a student at Cambridge University, and a Ph.D. at Princeton, a National Research Fellow at Harvard, with faculty appointments at Yale and Columbia Universities, he became the Chairman of the Department of Astronomy and Director of the Observatory at Princeton University in 1947. A pioneer in the development of fusion processes for conversion of hydrogen to helium, he now takes his readers to the vast reaches of space, the problems involved in getting there, and the scientific dividends that may be expected from the effort.

MAURICE L. HUGGINS,
Physicochemical Aspects of Hydrogen Bonds and Their Application to Biology 485

With two Bachelor's degrees and a Ph.D. from Berkley, California, a National Research Council Fellowship at Berkeley, Harvard and California Institute of Technology, faculty appointments at Stanford and Johns Hopkins Universities, a research associateship at the Eastman Kodak Laboratories for twenty-two years, the author of this article became, in 1959, the manager of physical and inorganic chemistry research at the Stanford Research Institute, Menlo Park, California. In 1955 and 1956 he was a Fulbright lecturer and visiting professor in Osaka and Kyoto Universities in Japan. This is the English version of an essay which will appear during the current year in Japanese, in a book entitled *Physical Chemistry of Drugs.*

WILFRIED F. H. M. MOMMAERTS,
Molecular Physiology—Analysis and Synthesis 497

A student of biology at the University of Leiden, Holland, and research worker with Szent-Györgyi in Hungary, he later taught biochemistry, chemistry and physiology at the American University in Beirut. His United States academic posts have been at Duke and Western Reserve Universities, with an interval of work with A. V. Hill in London. He is now Professor of Medicine and Physiology at the University of California Medical Center, Los Angeles. Most of his work has been devoted to the mechanism of muscular and cardiac contraction. The essays by G. G. Simpson and Clifford Grobstein in the jubilee issue of AMERICAN SCIENTIST, March 1962, stimulated the present contribution.

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PHILIP J. DAVIS,
The Criterion Makers: Mathematics and Social Policy 258A

The author of a forthcoming book on the theory of approximation and of "The Lore of Large Numbers" is Chief of the Numerical Analysis Section of the National Bureau of Standards, Washington, D.C. He received his doctorate in mathematics from Harvard in 1960. His address to the Annual Sigma Xi dinner at Auburn University, Auburn, Alabama, in May of this year is a welcome feature of the News and Views section of the present issue.

ROBERT T. HERSH, *Parkinson's Law, the Squid, and pU* 274A

An Associate Professor of Comparative Biochemistry in the University of Kansas, Lawrence, Kansas amplifies Professor A. B. Pardee's contribution in the June 1962 issue of AMERICAN SCIENTIST, relating pU to the work of a former Princeton biology professor, to the squid, and to Parkinson's Law.

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NEWS AND VIEWS

By the Board of Editors and the Membership of the Sigma Xi-RESA Societies

In the September issue of AMERICAN SCIENTIST each year, the important news from the Officers of the Society is the report of The Committee on Grants-in-Aid of Research. In the following report of the Committee, 156 grants are recorded to scientists in the United States, Canada, Stockholm, Istanbul, and Tokyo. Of particular interest is the first award under the Daisy Wu Endowment provided last year in memory of Dr. Wu's scientist husband, Hsien Wu.

REPORT OF THE AWARDS MADE BY THE GRANTS-IN-AID OF RESEARCH COMMITTEE FOR 1962

Although slightly more than $50,000 was awarded by the Grants-in-Aid of Research Committee this year, setting an all-time record, many worthy requests had to be denied entirely and most of those granted had to be less than the full amount requested. It should be pointed out that all monies contributed for research and all income from research funds are expended in support of the Sigma Xi-RESA Grants-in-Aid of Research. In addition to this, a sizable contribution is made each year from the operating income of the National Society. This portion, however, was somewhat less this year as a larger expenditure was authorized for the Anniversary Issue of the AMERICAN SCIENTIST.

The Committee on Grants-in-Aid of Research as well as the National Executive Committee are much encouraged in their consideration of the future opportunity of the Society to make a greater contribution to the "encouragement of scientific research." By virtue of the raising of the national assessment at the 1961 National Convention and by the increase being realized for the contributions to research from the Membership-at-Large and individual chapters and clubs, 1963 should see a real gain in this most important Society activity.

The specific awards made during the year to assist in the study of the subjects named are set forth in the following list which gives the amount awarded, the recipient's name, his institution, and the title of the study to be undertaken.

1. $300 to George A. Agogino, Harvard University, "To find the actual age of the controversial 'Torrington Skulls' long suspected of being Pleistocene human remains."

2. $500 to Yong Kee Ahn, Kansas State University, "Models of flow systems and their transient behavior."

3. $200 to Yog R. Ahuja, Loyola University, "Human cytogenetics with reference to disease-chromosomal complement in Eisenmenger's Complex."

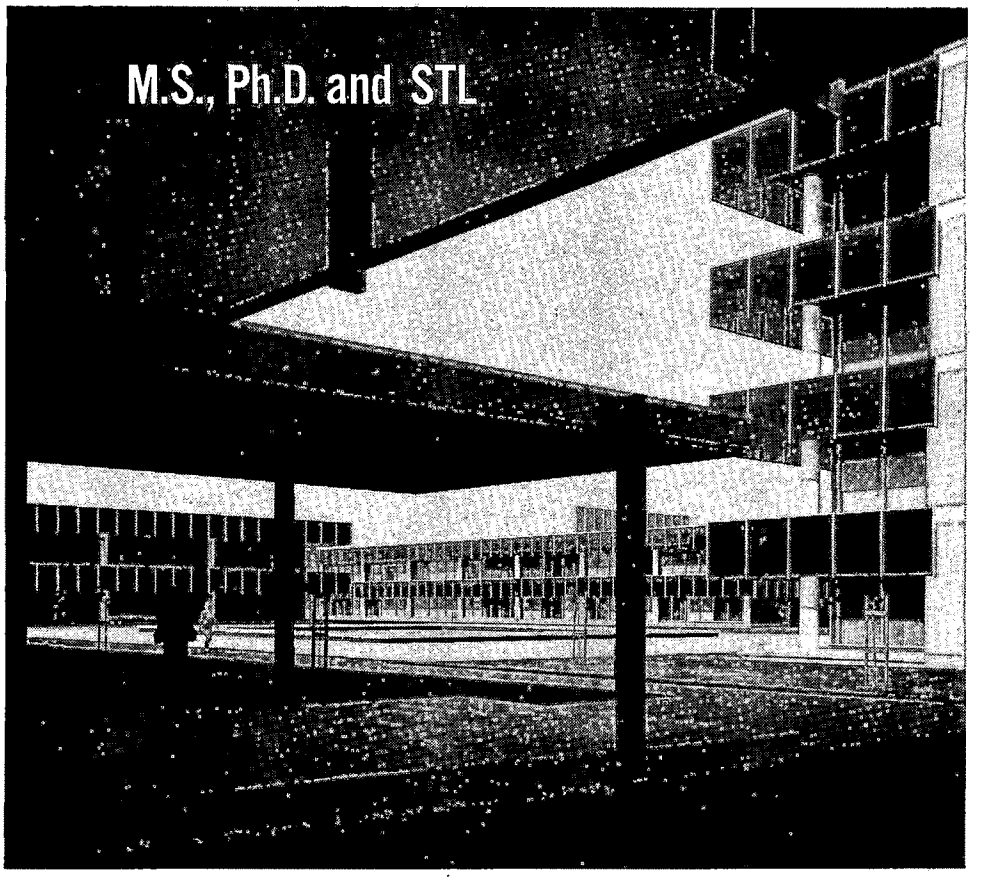
4. $300 to Richard Paul Alvord, Yale University, "Middle Ordovician Pelecypods of Minnesota: a study of the oldest, large clam fauna in North America."

5. $250 to Mary Needler Arai, Illinois State University, "Taxonomy and morphology of Ceriantharia (burrowing sea-anemones) of British Columbia."

6. $500 to Justin M. Aronfreed, University of Pennsylvania, "The effects of reinforcement *vs.* control of resources by a model upon children's learning to imitate the model's behavior."

7. $250 to Raymond F. Askew, Auburn University, "A Cockcroft-Walton accelerator for use in the study of neutron-capture gamma ray spectra."

8. $200 to Benjamin Harrison Banta, California Academy of Sciences, "Great Basin reptiles in the collections of Brigham Young University and the University of Utah."



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9. $300 to Jon Charles Barlow, University of Kansas, "The study of the White-eyed and Bell's Vireos."

10. $400 to William A. Berggren, University of Stockholm, "The study of the stratigraphic range and geographic distribution of Upper Cretaceous and Paleogene Planktonic Foraminifera."

11. $400 to Joseph Albert Beatty, Harvard University, "A revision of the spider genus *Ariadna*, with studies of behavior and statistical analysis of variation in certain characters."

12. $300 to Mark J. Behan, Montana State University, "The effect of the composition of the root atmosphere upon the water relationships of *Helianthus annuus* L. grown in mist culture."

13. $300 to Harold Joseph Bissell, Brigham Young University, "Realms of dolomite sedimentation in strata of Late Paleozoic Age in Western Utah and Eastern Nevada."

*14. $300 to Verle R. Bohman, University of Nevada, "A comparison of the toxicity of inorganic and 'organic' forms of molybdenum to the rabbit."

15. $300 to Kenneth Jay Boss, Harvard University, "Systematic studies of the family Tellinidae (Mollusca; lamellibranchia)."

16. $400 to Branley Allan Branson, Kansas State College, "Embryological development of the hyoid apparatus in *Ictiobus bubalus*."

17. $190 to David Alan Breternitz, University of Arizona, "The archaeological interpretation of tree-ring specimens for dating southwestern ceramic styles."

18. $300 to Thomas E. Bridge, University of Texas, "The contact metamorphic effects of a small igneous body which has intruded a thick sequence of Paleozoic Limestones in the Diablo Plateau area of West Texas."

19. $250 to Harold Kelly Brooks, University of Florida, "The Paleozoic Eumalacostraca of North America."

20. $300 to Lauren Evans Brown, Southern Illinois University, "Relative fitness and luxuriance of *Bufo woodhousei*, *Bufo americanus* and their interspecies hybrids."

21. $300 to Robert Thorson Brown, Michigan College of Mining and Technology, "Influence of naturally occurring growth substances on plant reproduction."

22. $250 to Reinhart Albert Brust, University of Illinois, "Bionomics of a subarctic group of pest mosquitoes known as 'black-legged' *Aedes*."

23. $250 to John Milton Campbell, University of Illinois, "The Mexican species of the genus *Lobopoda Solier* (Coleoptera: Alleculidae)."

24. $300 to Chih Shan Chen, Northwestern University, "The lithostratigraphy and geochemistry of the Paleocene-Eocene rocks of Florida."

25. $300 to Do Sup Chung, Kansas State University, "Dynamic characteristics of modified fluidized beds."

26. $400 to Llewellya W. Hillis Colinvaux, Duke University, "Morphologic and taxonomic study of the genus *Halimeda* (order Siphonales)."

27. $350 to James Elvin Conkin, University of Louisville, "Smaller Foraminifera (Protozoa) of the Lower Mississippian (Paleozoic) of Iowa and Northern Missouri."

28. $150 to Richard Timothy Cronin, Fordham University, "In population genetics; a study of wing length character in insular and continental populations."

29. $350 to Kenneth Leland Crowell, Duke University, "The effects of species interactions of habitat selection and population density in small mammals."

30. $500 to Mary Elizabeth Cunningham, University of Oregon, "The degree of circular polarization of bremsstrahlung produced by beta rays from Bi^{210} (ReE)."

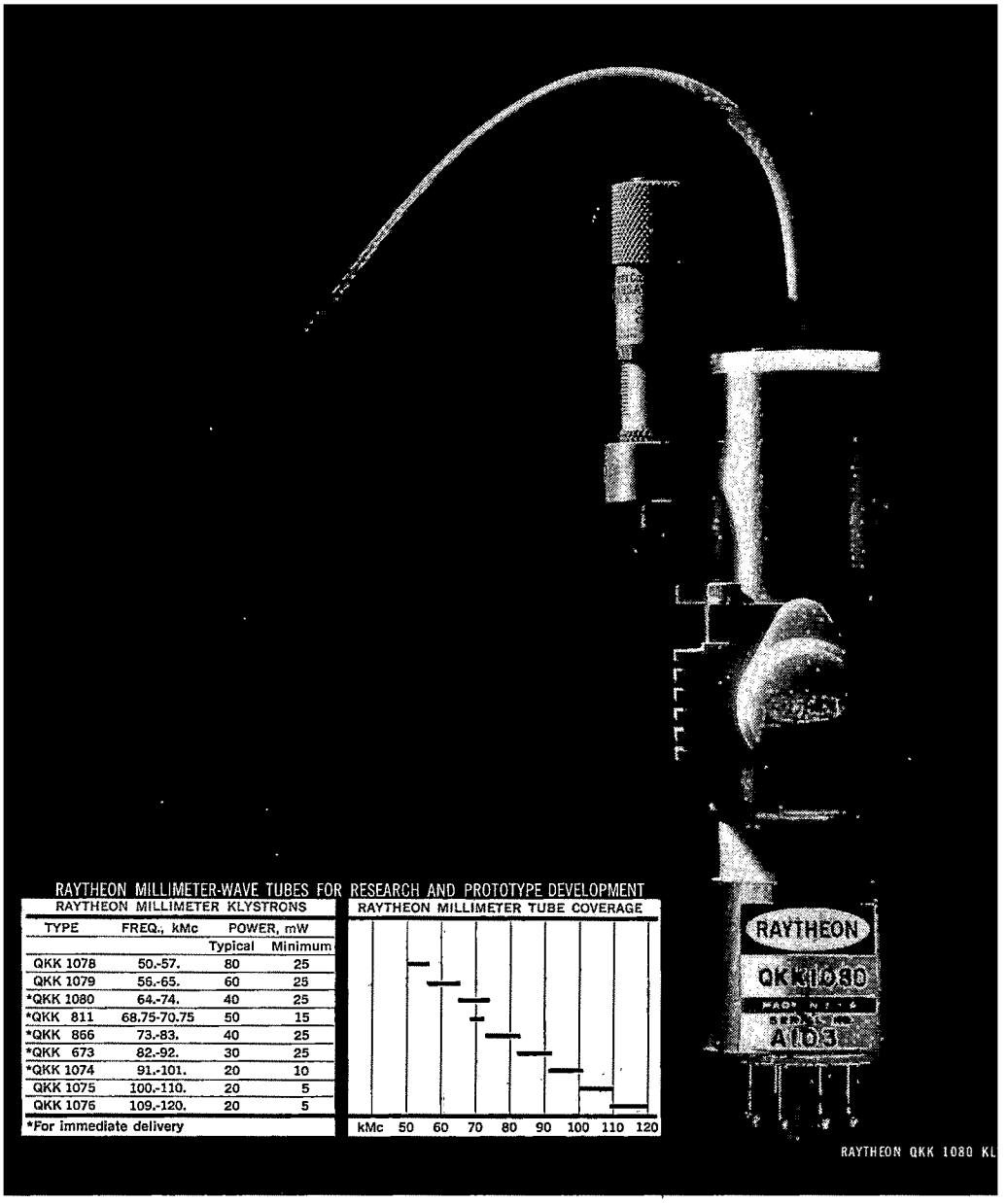
31. $200 to Mary Judith Dady, University of Rhode Island, "Electrocardiography and survival of submerged frogs and toads at various temperatures."

32. $300 to Graham Johnson Davis, East Carolina College, "The effects of light quality on leaf."

33. $400 to Robert Michael Delaney, St. Louis University, "Temperature

(*Continued on page 240A*

* Designated as the Daisy Wu Award, the first to be given in memory of Hsien Wu (see AM. SCI., *49*, 120A, 1961).



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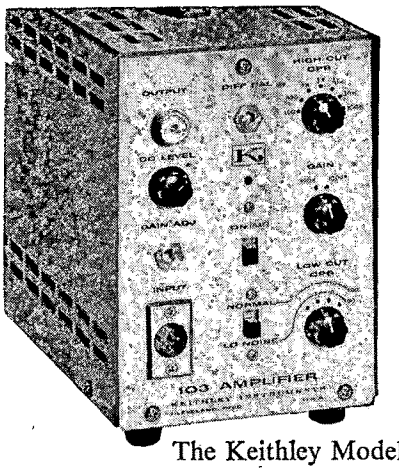
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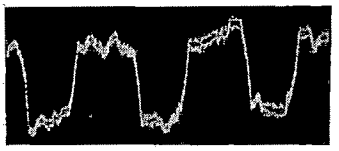
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3 kc	0.9	0.6
1 kc	0.7	0.4
300 cps	0.5	0.3
100 cps	0.4	0.25

This oscillograph shows a 2 kc square wave of 5 microvolts peak-to-peak amplitude at input with the amplifier in "Low Noise" position. Horizontal calibration equals 200 μ sec. per division; vertical equals 2 μv per division. Low cut is 10 cps, high cut 1 kc. The unusually low noise levels in the 103 are achieved through the use of ceramic tubes in cascode circuitry.



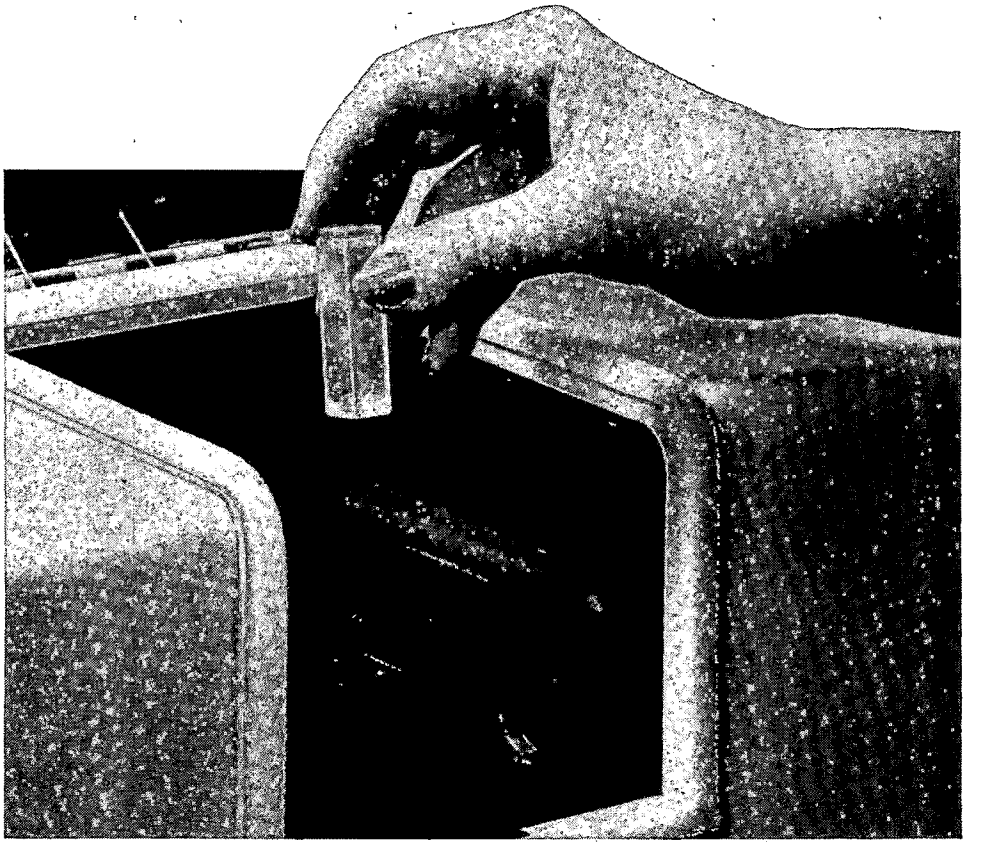
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239A

(*Continued from page 236A*)

distribution in a hydrogen plasma jet using neutron thermometry."

34. $200 to George Henry Denton, Yale University, "Glacial geology and geomorphology of Kaskawalsh Valley, Icefield Ranges, Yukon Territories."

35. $600 to David Leonard Dilcher, University of Illinois, "Cuticular analysis of Eocene leaves."

36. $300 to David Warren Dunham, Cornell University, "The comparative ethology of the Richmondenine Finches, and especially the genus *Pheucticus*: its form, function, ontogeny and probable evolution."

37. $200 to Christopher John Durden, Yale University, "Intensive collection of fossil insects at known localities, with emphasis on Blattaria."

38. $500 to Donald Alan Eggert, Southern Illinois University, "The Petrifaction Flora of coal balls occurring in the #6 (Herrin) coal of Illinois."

39. $400 to Elwood B. Ehrle, State University College at Geneseo, New York, "The Bryophyte Flora of Livingston County, New York."

40. $100 to Stanley Alan Fagan, University of Pennsylvania, "The effects of variations in the nature of the relationship between own competence and others' competence on conformity behavior."

41. $150 to William A. Feder, Marine Laboratory, Institute of Marine Science of the University of Miami, "Detection and occurrence of Nematode-capturing Predaceous Fungi in marine habitats."

42. $200 to Millicent Sigler Ficken, Cornell University, "Comparative ethology of the Blue-winged-Golden-winged Warbler complex."

43. $500 to Harold Dennis Fishbein, University of Pennsylvania, "The relationship between efficiency in reaction time and efficiency in eyelid conditioning."

44. $400 to John J. Fisher, University of North Carolina, "A comparative study of the caves of Anglo-America."

45. $300 to Marjorie Hopkins Fox, Louisiana State University, "Congenital cardiac pathogenesis in rats."

46. $500 to Wayne Frair, King's College, Briarcliff Manor, N. Y., "Comparative immunological study of proteins from Reptile Sera."

47. $500 to Macdonald Fulton, Loyola University, "Identification of a collection of 1000 strains of bacteria belonging to a considerable number of different Genera collected from reptiles and amphibians in Puerto Rico in 1960 and 1961."

48. $500 to Joseph Daniel Gettler, New York University, "The mechanism of formation of Pyrazole, Diazolone, and Oxadiazole Rings."

49. $100 to David Lee Giles, Miami University, "The geology at the north end of the Sierrita Mountains—Gunsight and Soto Peaks, Pima County, Arizona."

50. $500 to Norman Lewis Goldman, Queens College, Flushing, N. Y., "Mild methods for the generation of benzyne type intermediates: attempted dimerization of certain benzynes to give non-benzenoid aromatics."

51. $200 to Joseph Bernard Gonthier, University of Massachusetts, "Bedrock geology of the northern half of the Torrington Quadrangle, Connecticut."

52. $500 to Harry William Gordon, St. Barnabas Medical Center, "The relation of graft temperature to rejection."

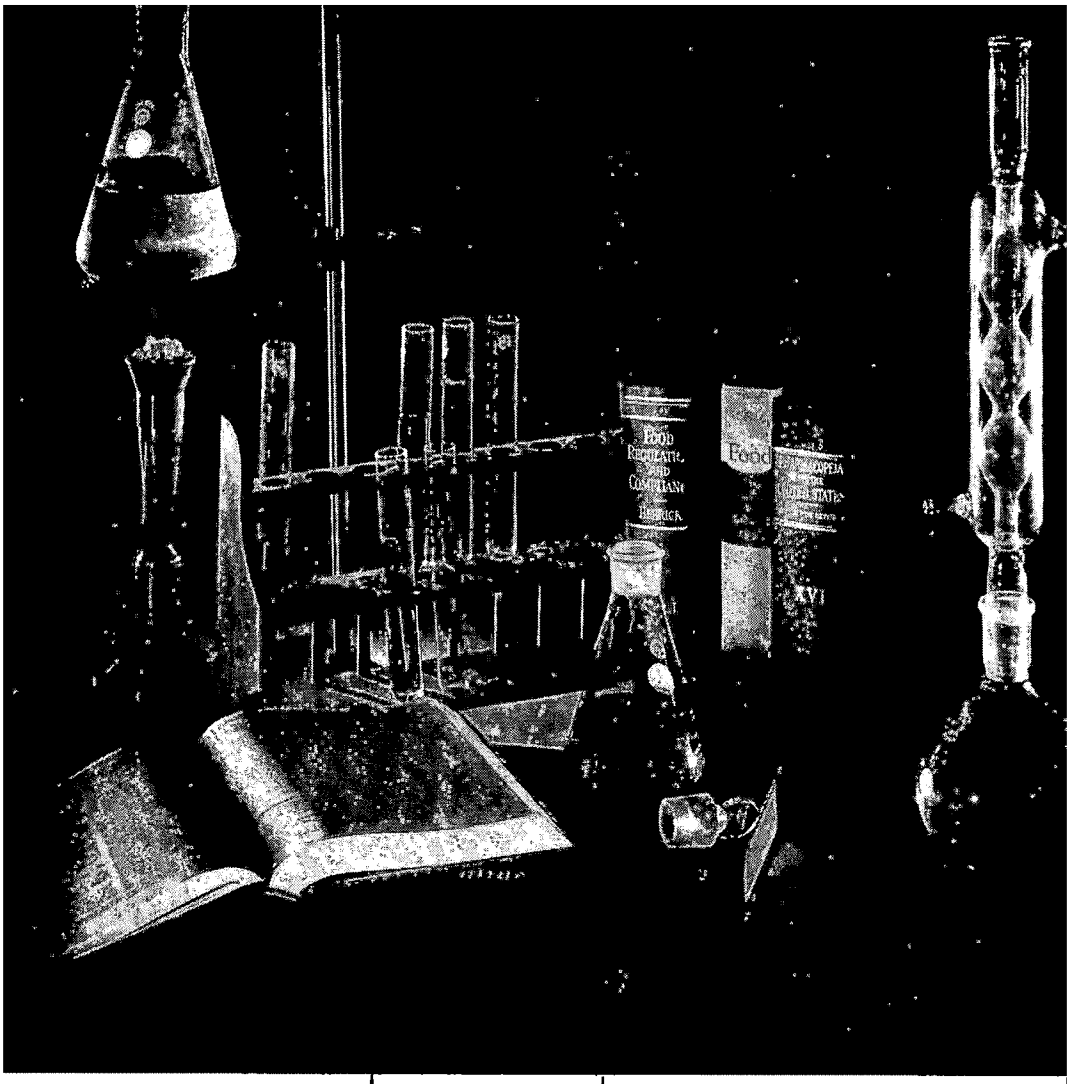
53. $400 to Vinton Edward Gwinn, McMaster University, "Completion of stratigraphic, petrologic, and environmental studies of Cretaceous Colorado Group sediments in the vicinity of the Boulder Batholith, Western Montana."

54. $300 to Jack Parker Hailman, Duke University, "Ontogenetic development of feeding in gull chicks, with special attention to pecking by chicks of *Larus atricilla* and *L. argentatus*.

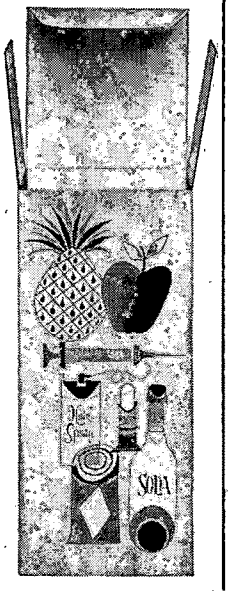
55. $400 to Edward Carlton Hansen, Yale University, "Retrograde metamorphism and superposed folding in basal Gneiss."

56. $250 to Christopher Peter Macclesfield Heath, University of Illinois, "The petrology of the birdspring formation, Arrow Canyon Range, South Nevada."

57. $400 to Eberhardt William Hein-



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rich, University of Michigan, "Precambrian geology of the Arkansas Canyon Area, Colorado."

58. $150 to Charles James Hillson, Pennsylvania State University, "Studies on the distribution, ontogeny, and comparative anatomy of the sieve tubes in the division Phaeophyta."

59. $300 to Edmund S. Hobson, University of California, "Feeding behavior in three inshore predaceous fishes."

60. $300 to Albion Reed Hodgdon and Radcliffe B. Pike, University of New Hampshire, "Comparative study of Floras of the individual islands comprising the Wolf Island Group, Charlotte County, New Brunswick."

61. $400 to Jerry Martin Hoffer, Washington State University, "Plagioclase variation in some flows of Columbia River Basalt."

62. $250 to Harry Theodore Horner, Jr., Northwestern University, "A study of the origin and differentiation of the micro- and megasporangia of *Selaginella*."

63. $250 to James E. Huheey, Worcester Polytechnic Institute, "Application of mathematical methods to delineation of biogeographical regions."

64. $500 to Grace Blanchard Iverson, Miami, Florida, "Evolution of the karyotype in the leafy liverwort genus *Frullania*."

65. $300 to Crawford Gardner Jackson, University of Florida, "The ecology and natural history of the Suwannee terrapin, *Pseudemys concinna suwanniensis*."

66. $400 to David Ware Johnston, Wake Forest College, "Experiments on autumnal migration in the indigo bunting."

67. $300 to Arthur Louis Kelly, Boston University, "Blood elements in local fresh-water fish and seasonal changes in the hemoglobin and blood elements of these fish."

68. $300 to Edmund Taylor Kittleman, Hampden-Sydney College, "Attempts to incorporate hydroxymethyl and carboxy groups into Chromium (III) Chelates by indirect routes."

69. $400 to George deVries Klein, University of Pittsburgh, "Sandstone petrogenesis of the Stanley-Jackfork Boundary, Ouachita Fold Belt, Arkansas and Oklahoma."

70. $200 to Irving William Knoblock, Michigan State University, "The North American species of *Cheilanthes:* the contribution of anatomy to speciation problems in certain members of the genus."

71. $350 to Baerbel Koesters, Pennsylvania State University, "Field mapping and investigation of the mechanics of faulting in the area of Morrison Lake Quadrangle, Montana–Idaho."

72. $500 to David H. Krantz, University of Pennsylvania, "The mathematical structure of color mixture data and its psychophysical implications."

73. $150 to Clair Leonard Kucera, University of Missouri, "Growth characteristics in races of *Eupatorium rugosum*."

74. $200 to Frank Edmund Kurczewski, Cornell University, "A comparative, ethological study of several species of *Tachysphex* (Hymenoptera: Sphecidae) which occur in Southern Florida."

75. $250 to Urless Norton Lanham, University of Colorado, "Eco-taxonomic structure of an alpine insect fauna: partition of the fauna among families and orders, and its ecological and evolutionary significance."

76. $350 to Michael Howard Levin, Rutgers, The State University, "A comparative floristic study of early secondary succession on the outer coastal plain of New Jersey."

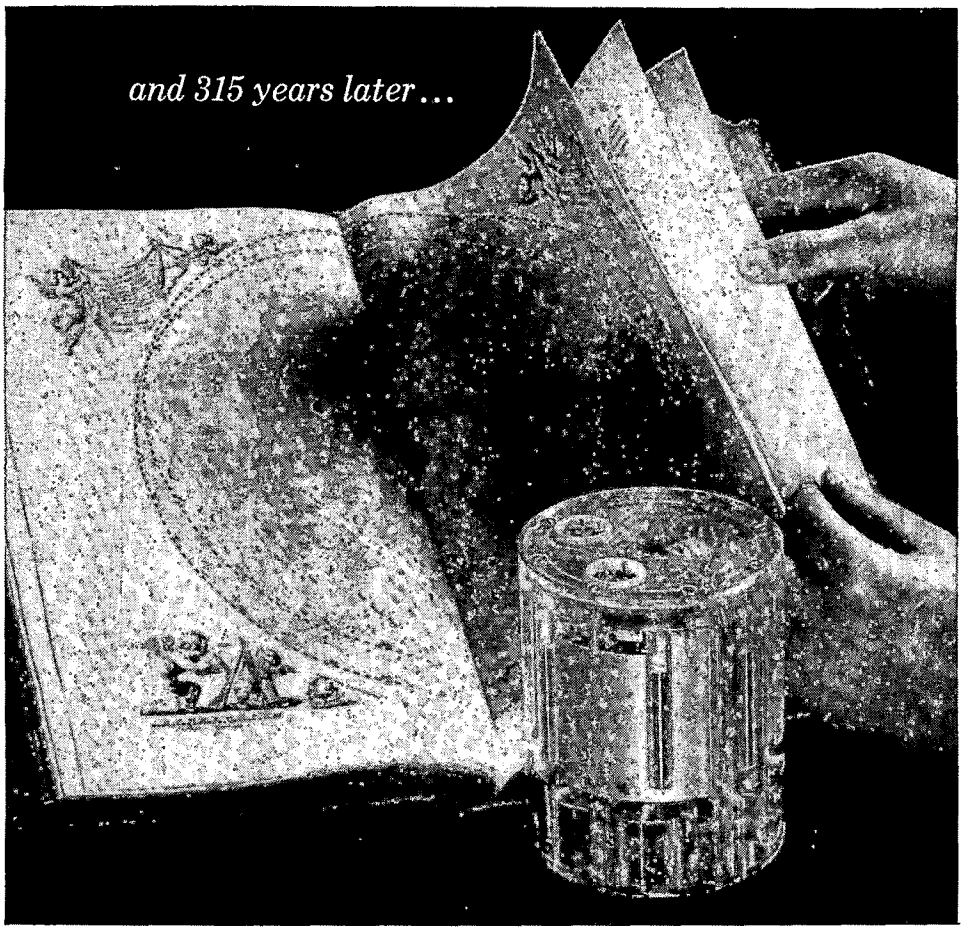
77. $400 to Louis Levine, City College of New York, "Factors affecting mating competition in mice."

78. $300 to Charles A. Long, University of Kansas, "The recent mammals of Wyoming."

79. $500 to Horace O. Lund, University of Georgia, "Preserving the P. W. Fattig insect collection."

80. $350 to Richard Frank Madole, Ohio State University, "Glacial geology of Middle and North St. Vrain Valleys and chronology of glacial and postglacial terraces along St. Vrain River and Lefthand Creek, Boulder County, Colorado."

(Continued on page 246A)



Shown before a rare copy of the Selenographia, first lunar atlas, printed 1647, is a transparent model of the first Lunar Seismometer. The seismometer, when equipped with amplifiers and telemetry, is an integral part of a major U.S. space probe vehicle and is designed to detect minute surface motion on the moon. UED can supply this omni-directional, self-centering instrument suitably modified for remote applications in any terrestrial environment, where the following characteristics are particularly applicable: small size, extreme ruggedness, extended period, high output.

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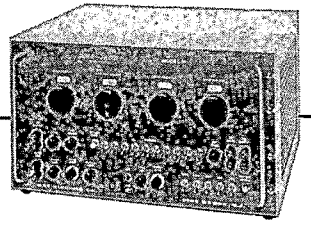


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141-C

- **Sweep audio frequencies high-Q filters, tape recorders.**
- **Ultra-stable narrow frequency sweeps 20 cps to 200 kc.**
- **200 cps to 200 kc in single frequency sweep.**
- **Both linear and logarithmic sweeps plus manual sweep control.**
- **Built-in audio detector.**
- **Fixed and variable pulse type markers.**

New Alignment Technique

The Sona-Sweep Model M adapts the accepted techniques of r-f swept frequency alignment to audio and ultrasonic (e.g., tape recorder) bandpass measurements and adjustments. In addition, the highly stable response curve developed by the Model M and the parallel display provided by its manual control, give easier, more accurate checks of high Q filters and sharp slope devices.

Detecting At Audio

A major disadvantage of previous sweeping oscillator techniques for use at audio frequencies has been the difficulty of obtaining a clean envelope response. Usually, no single audio frequency detector covers the octaves of the full audio range. Often, too, the large bypassing necessary slows down detector response so as to make it inaccurate for changing (swept) conditions. To eliminate this difficulty, a synchronous detector has been provided, giving adequate bypassing down to about 200 cps.

Single Trace With Markers

A clean, detected envelope of audio frequency bandpass characteristics clearly defines amplitude vs frequency. The increase in trace intensity provided by the detected signal (rather than by a diffused a-f pattern) makes it much easier to view the response characteristics and to monitor any adjustments of the circuit under test. The sharp pulse type markers provided by the Model M precisely and clearly define critical points along the trace.

Model M — A Complete System

The Model M provides a complete measurement system, including—logarithmic, linear and manual sweeps, or a calibrated c-w signal; sharp, "crystal," pulse type frequency markers and precision step attenuator.

Variable Center Frequencies

The Model M is a double heterodyne sweep generator employing three crystal controlled oscillators. Either linear or log sweeps are available with center frequencies continuously variable between 20 cps and 200 kc. The swept output is blanked during retrace time, providing a zero-voltage base line. To eliminate phasing adjustments, a sawtooth voltage, synchronized with the swept output, is available to drive the X-axis of the scope.

Varied Sweep Rates

For checking high-Q circuits and low frequency response characteristics, variable rep rates down to 0.2 cps are available. This wide choice of rep rates, continuous to 25 cycles, plus a 30 cycle lock, permits selection of the optimum rep rate which gives an accurate response display of the circuit being tested, plus ease of viewing on the scope screen.

Varied Sweep Widths

Sweep widths are continuously variable from 20 cps to 200 kc. In each of three steps — 2 kc, 20 kc, 200 kc — new modulating circuits are switched in to provide maximum stability in each range.

Logarithmic Sweeps

A nominally logarithmic sweep, most useful for studying audio and video low pass circuits, provides an expanded view of the low frequency end, while showing overall frequency characteristics.

Manual Sweep

A manually controlled swept output provides a means of varying cw signal in synch with the oscilloscope display. The manual control covers the same frequency range to which the Model M is set for electronic sweeping. It can be used to examine response characteristics in detail, or in response to cw and near cw conditions. When the Sona-Sweep is used with an electronic counter and accurate voltmeter, it can perform all the checks where cw is preferred. In addition, it can utilize the counter and its scope synchronization feature to frequency calibrate the oscilloscope display.

High Level Output

The Model M delivers a high level output of 5 volts rms into 600 ohms over the entire frequency range. The built-in, precision step attenuator provides up to 59 db of attenuation in discrete steps. An additional 6 db of variable attenuation is provided. Output is flat within ±0.5 db.

SPECIFICATIONS

Center Frequency Range: 20 cps to 200 kc. Continuously variable.

Sweep Width — Three ranges: 20 cycles to 2 kc., 200 cycles to 20 kc., 200 cycles to 200 kc.

Sweep Output And Repetition Rates: Sawtooth for horizontal deflection of scope trace. Low impedance output, approx. 3 V pp.

a. Fixed at 30 cps in Line-Lock mode.

b. Variable 0.2 to 25 cps for logarithmic sweep.

c. Three continuously variable linear ranges: 0.2 cps to 1 cps, 1 cps to 5 cps, 5 cps to 25 cps.

Output Level: 5 volts rms into 600 ohms.

Flatness: ±0.5 db over widest sweep.

Markers: (Optional): Ten crystal pulse type markers, positioned at customer specified frequencies; e.g., 200 cycles, 500 cycles, 1 kc, 2 kc, 5 kc, 20 kc, 50 kc, 100 kc and 200 kc.

Markers designated for use at wide sweep (200 kc) are not applicable to other sweep widths. Specify whether for wide (200 kc) or narrow (less than 20 kc).

Calibrated CW Output: 20 cps to 200 kc.

Built-In Attenuator: Switchable steps, 3 db, 6 db, 10 db, 20 db, 20 db, plus 6 db variable.

Power Supply: Input approximately 220 watts. 117 volts (±10%), 60 cps B+ electronically regulated.

Dimensions: 19⅜" x 10⅞" x 16⅛".

Price:
$1295.00 f.o.b. factory $1425.00 f.a.s. N.Y.
$17.00 ea. for markers $19.00 ea. for markers

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(*Continued from page 242A*)

81. $400 to Jerome Frank Machamer, Pennsylvania State University, "The origin of the iron ore deposits in the Zenith Mine, Ely, Minnesota."

82. $300 to Sister M. Celine Martin, University of Notre Dame, "Ecological life history of *Geranium maculatum*."

83. $300 to Eugene William McArdle, Saint Mary's College, Winona, Minnesota, "Somatic variation in the Hymenostome Ciliate, *Tetrahymena Rostrara*."

84. $200 to Roy Wallace McDiarmid, University of Southern California, "An Ecogeographic analysis of the Herpetofauna of the Tropical Transition Zone in Western Mexico."

85. $500 to Robert William McFarlane, University of Florida, "The taxonomic significance of Avian Sperm, their comparative morphology and phylogenetic relationships."

86. $200 to Jereld Edward McQueen, University of Texas, "Fracture pattern analysis of Southern Burnet County, Texas."

87. $350 to James Charles Melik, University of Michigan, "The hingement of Paleocopid Ostracods and its relation to their evolution and classification."

88. $400 to John Thomas Mickel, Iowa State University, "Floristic and biosystematic studies on the ferns of Eastern Oaxaca."

89. $500 to Donald Spencer Miller, Rensselaer Polytechnic Institute, "The Uranium-Lead Ages of Rock Zircons from the Adirondack Metamorphic and Igneous Complex."

90. $350 to Richard Scott Mitchell, University of Virginia, "X-ray diffraction study of heat treated metamict minerals from Virginia."

91. $100 to Robert H. Mohlenbrock, Southern Illinois University, "A monograph on the genus Zornia."

92. $500 to James Robert Moore, III, Texaco Laboratories, Bellaire, Texas, "Relationships between provenance and composition of the recent marine sediments in Cardigan Bay, Wales, United Kingdom."

93. $350 to Theodore Mosquin, University of Alberta, Calgary, Alta, Canada, "Chromosomal and morphological variation in *Epilobium angustifolium* plus *Arabis*."

94. $250 to William Wayne Moss, University of Kansas, "The external and internal morphology, taxonomy and behavior of the Trombidiid Mite, *Allothrombium lerouxi* Moss."

95. $400 to James Lester Murphy, University of Michigan, "Conflagration control on forest lands."

96. $500 to Thomas San Oey, Ohio Wesleyan University, "Chlorito complexes of Cd and Zn."

97. $350 to Allen Roger Ormiston, Harvard University, "A systematic and biostratigraphic study of the Devonian trilobites of the Canadian Arctic Islands."

98. $500 to Ervin George Otvos, Yale University, "Quantitative interrelationship between wave and tide action and beach sedimentation, with special emphasis on concentration of heavy mineral layers and beach cycles."

99. $600 to Robert Lewis Packard, Stephen F. Austin State College, Nacogdoches, Texas, "Life history and ecology of the fulvous harvest mouse, *Reithrodontomys fulvescens*, in Pine-oak Uplands."

100. $250 to Margaret Cranston Parsons, University of Toronto, "The morphology and phylogeny of the aquatic and semi-aquatic heteroptera."

101. $350 to Thomas Hudson Patton, University of Texas, "Artiodactyls of the Gulf Coastal Plain."

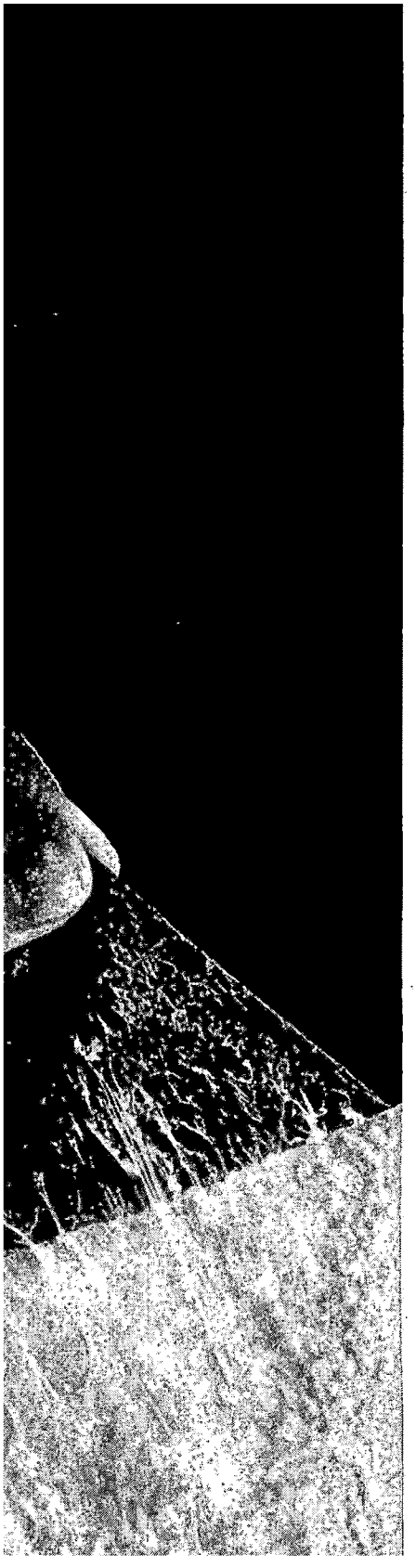
102. $350 to Raymond Pestrong, Stanford University, "Initiation, development and mechanism of meandering channels on tidal flats."

103. $250 to Sister Mary Petra (Monica J. Kevin), Fordham University, "The effect of antimetabolites of R.N.A. on chromosomal elimination."

104. $600 to Lester Glen Phares, University of Pennsylvania, "Effects of power and dependence on conformity and satisfaction in small groups."

105. $300 to Ronald Carl Phillips, Seattle Pacific College, "The seagrasses of Puget Sound, Washington."

106. $300 to William Russell Pierson, California College of Medicine, "Application of monophotogrammetry to hu-



One of a series briefly describing GM's research in depth

Adhesion: describing an elephant of science

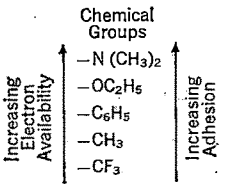
Adhesion has certain similarities to the elephant the blind men were asked to depict. The mechanisms and principles of adhesion seem to have eluded an overall theoretical description by the several disciplines concerned with them. Perhaps not for long.

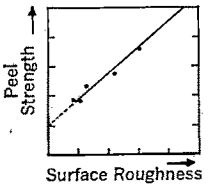
Food for inductive thought is being gathered from fundamental research studies around the world. At the General Motors Research Laboratories, for example, polymer scientists have found that the more available the electrons in the chemical groups they synthesized into the polymer structure, the stronger the adhesion. Similarly, the rougher the metal surface, the more force required to break the adhesive bonds between the polymeric coating and the substrate.

This experimental approach is enriching our understanding of some of the fundamentals affecting adhesion. It is also finding practical use in General Motors, helping in improving the adhesion of paint, rubber, plastics, and metals to each other. It's another example of GM's continual quest for—A BETTER WAY.

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man subjects and a comparison of the results with those obtained by formulae for specific gravity, body volume, and surface area."

107. $400 to Max Grow Pitcher, Columbia University, "Evolution of the Chazyan (Ordovician) Reefs of Eastern United States."

108. $450 to Louise Frances Potter, Windham College, "Pollution, bacteriological and chemical, of Flathead Lake."

109. $300 to Alexander D. Poularikas, University of Arkansas, "Impedance versus frequency characteristics of a D-C plasma."

110. $500 to Melukote K. Ramaswamy, Ohio State University, "Nuclear properties from radioactive decay."

111. $150 to Kenneth R. H. Read, Harvard University, "Thermal stability of enzymes of bivalve molluscs of differing ecologies and geographic range."

112. $200 to David Edward Reichle, Northwestern University, "The distribution and abundance of beetles in the family Pselaphidae associated with bogs."

113. $400 to Edward Leon Richards, Kansas University, "Monograph of the genus *Ratibida* which is in the family Compositae."

114. $300 to Donald Warren Rogers, Robert Kolei, Bebek, Istanbul, Turkey, "The phase titration of binary systems."

115. $350 to Charles Llewellyn Rowett, University of Oklahoma, "The study of the fauna and biostratigraphy of the Wapanuka formation for the purpose of establishing faunal zones to aid in regional correlation of Oklahoma, Texas, and Arkansas."

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117. $150 to James Bivin Rucker, Louisiana State University, "Distribution and durability of selected trace elements in recent and tertiary Gulf Coast Mollusca."

118. $200 to Ronald A. Ryder, Colorado State University, "Migration, mortality and longevity of the American Coot (*Fulica americana*), Mallard (*Anas platyrhynchos*) and Pintail (*Anas acuta*)."

119. $500 to John E. Sanders, Yale University, "Primary sedimentary structures in Silurian-Devonian limestones, Gaspé, Quebec."

120. $400 to Alvin Sarachek, University of Wichita, "The biochemical genetics of dimorphism in *Candida albicans*."

121. $350 to J. Henry Sather, Western Illinois University, "A study of movements and behavior patterns of white-tailed deer (*Odocoileus virginianus*) in West-Central Illinois."

122. $300 to Frank Philip Saul, Harvard University, "The osterbiography (osteometry, demography, pathology, etc.) of the ancient populations of Altar de Sacrificios, a Mayan site in Guatemala, which has been excavated by G. R. Willey of Harvard."

123. $200 to Hans-Ulrich Schmincke, Johns Hopkins University, "The stratigraphy, sedimentology, and petrology of some pliocene fluvial, lacustrine, and volcanoclastic sediments of the Columbia River Plateau, Washington."

124. $300 to Richard Evans Schultes, Harvard University, "Native agriculture in Northwest Amazon."

125. $200 to William Henry Scott, Yale University, "Metamorphic structure and stratigraphy of a portion of the Norwegian Caledonides."

126. $600 to Joseph Anthony Sherma, Lafayette College, "Developing the technique of thin-layer solubilization chromatography for the separation of water-insoluble organic solutes."

127. $150 to John Ford Shroder, Jr., University of Massachusetts, "Stratigraphy and tectonic history of Mississippian Monction Group of Nonmarine Red Beds."

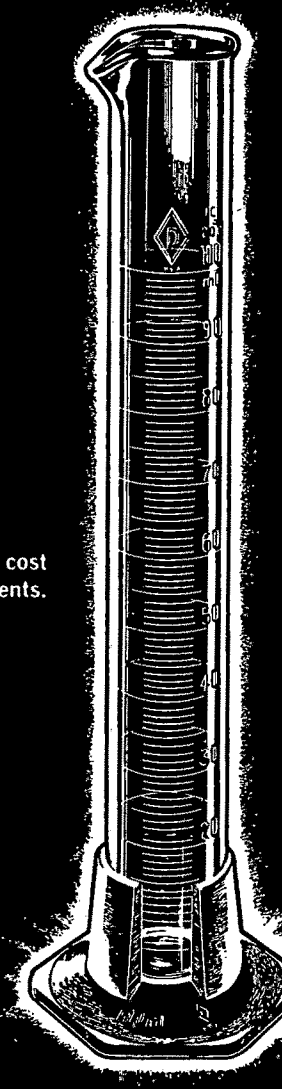
128. $700 to Jay Joseph Singer, University of Pennsylvania, "The effects of intracranial chemical injections upon the ongoing sexual behavior."

129. $250 to Leslie Arthur Sirkin, New York University, "The postglacial of Long Island from the standpoint of stratigraphy, floral and faunal succession, paleogeography and climatology as revealed by the deposits which have accumulated in Long Island Sound."

(*Continued on page 252A*)

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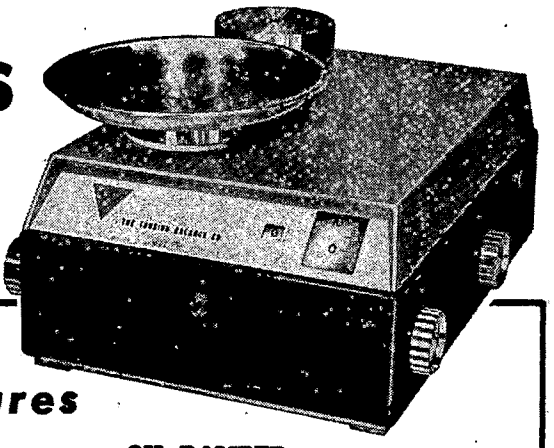
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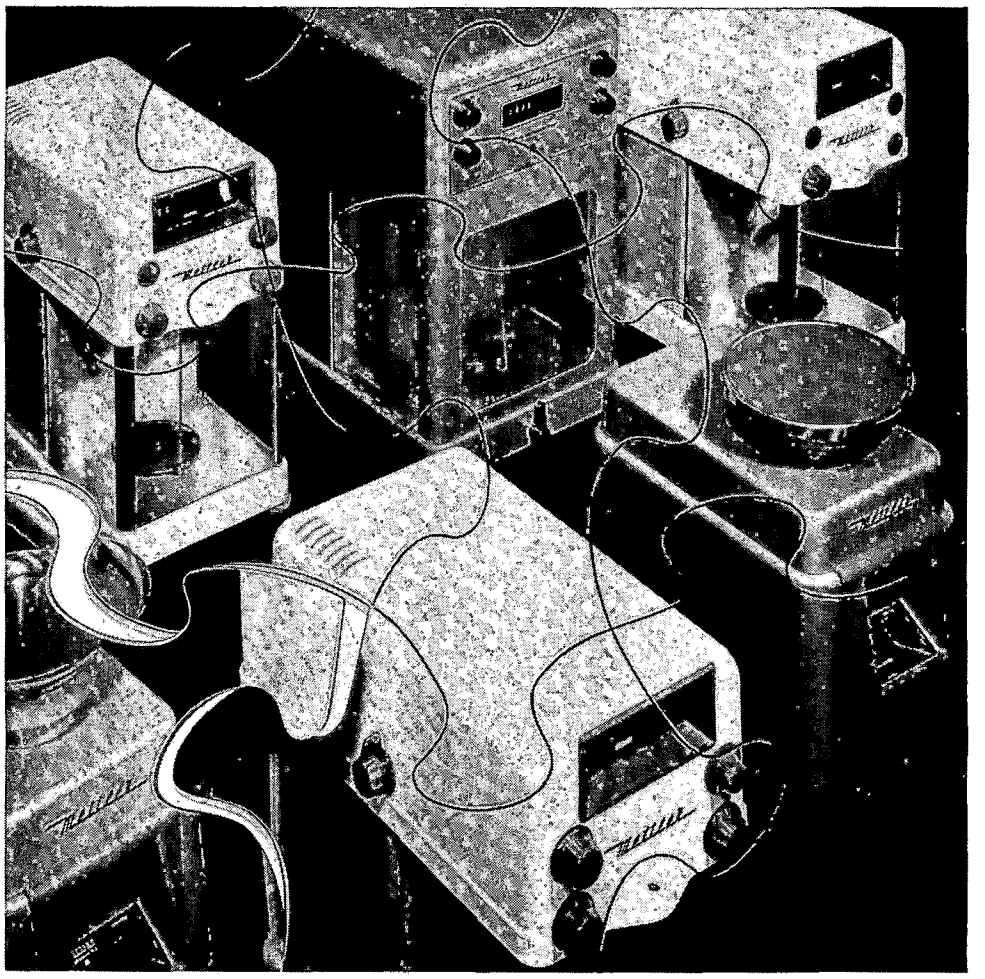
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(*Continued from page 248A*)

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(*Continued on page 258A*)

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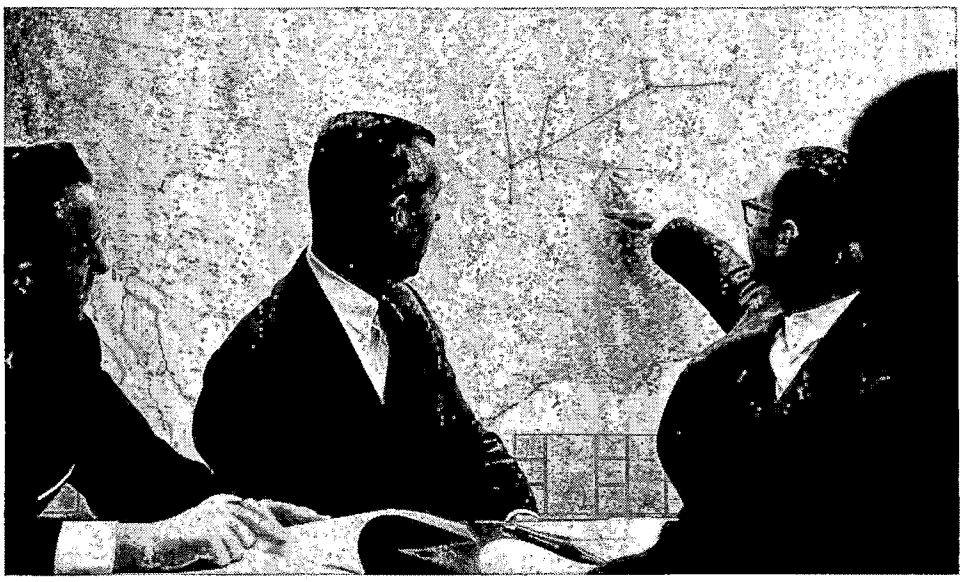
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(*Continued from page 254A*)

THE CRITERION MAKERS: MATHEMATICS AND SOCIAL POLICY*

By PHILIP J. DAVIS

At the outset, I must confess to a certain lack of qualification to handle my topic. By training I am a mathematician. I have worked in the fields of functional analysis, approximation theory, and numerical analysis. I have been close to computation since my hitch with the NACA during the war. At the moment, I am trying to think up efficient schemes for computing the values of highly multiple integrals such as arise in the theory of gas dynamics. All this I admit, is far from "social policy." In my professional life, I believe I have undertaken no scheme which would move a man—in his social life—farther than from a desk to a blackboard.

My topic is one which is discussed everywhere—and yet, in a certain sense, nowhere. It is about a method and an attitude. You will find the method in every classroom in mathematics. More specifically, you will find it in a vast and ever increasing flow of literature on the applications of computers, on operations research, econometrics, automation. You will find the attitude in the hearts of the mathematicians, but it is seeping out to the world at large. There are hints of it in the daily papers and in reports of congressional committees. I have seen a part of this vast flow and it inevitably calls forth a reaction from me. I will attempt to relate the two: the method and the attitude. My qualification is my willingness to speak from a position of "contemplative, but isolated, contiguity."

Let me set the stage for my subject matter by relating a familiar story.

On a recent trip to New York, I got off at Pennsylvania Station. I walked to the taxi platform, but the train had been crowded and soon dozens of people poured out of the station for cabs. Some of them waved and yelled; some stepped in front of the cabs rushing down the incline; some, who had porters in their employ, seemed to be getting preferential treatment. I waited on the curb, convinced that my gentle ways and the merits of my case would ultimately attract a driver. However, when they did, he was snatched out from under me. I gave up, took the subway, cursing the railroad, the cabbies, and people in general, and hoping they would all get stuck for hours in the crosstown traffic.

Several weeks later, on a trip to Philadelphia, I got off at the 30th St. Station. I walked to the taxi platform. A sign advised me to take a number. A dispatcher loaded the cabs in numerical order, and I was soon on my way to the hotel. It was rapid, it was pleasant, it was civilized. And this is a fine, though exceedingly simple, way in which mathematics may affect social affairs. The criterion for loading is the order of arrival on the platform. The numbers have an order, and can be used to simulate a queue without the inconvenience and indignity of actually forming a queue. The numbers are a catalyst that can help turn raving madmen into polite humans.

Let me go back a bit in history. Mathematics is a subject with two faces. There is pure mathematics and there is applied mathematics. Pure mathematics sets up formal systems of deduction and derives possible consequences of one's assumptions. The derivations follow the strict pattern of logic. The fruits of pure mathematics are its theorems, i.e., statements which say that such and such assumptions lead to such and such conclusions.

Applied mathematics applies the results and the attitudes of pure mathematics to the concerns of the world around us. Applied mathematics is close to the other sciences and to technology, and frequently cannot be distinguished from them. Neither pure mathematics nor applied mathematics is prior to the other, for each derives sustenance from the other. Where pure mathematics tells applied mathematics what is logically possible, applied mathematics tells pure mathematics where to look and what is interesting and important outside the latter's narrow world.

* An address delivered at the Annual Dinner of the Society of the Sigma Xi, Auburn University, Auburn, Alabama, May 23, 1962.

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The application of mathematics to the physical sciences and to technology is an old story. The mathematics employed is frequently of an older vintage. For instance, the theory of satellite orbits was worked on by mathematicians and astronomers for two hundred years following Newton. But by the early 1900's, celestial mechanics, as it is called, was of small interest and, though many difficult problems were left unsolved, the field attracted little attention.

There were only a handful of people who would have known how to compute the orbit of a comet. Today, the satellites have sent space theoreticians scurrying back to the work of the old masters. While John Glenn was making his remarkable flight, the NASA's computing machines in Greenbelt, Maryland, were grinding away most crucially. But the mathematics they were performing could easily have been set up by the Marquis de Laplace who lived in 1800.

A major impetus to applied mathematics has, in recent years, come from a new and surprising source: the social and economic behavior of human beings, people in the market place, people as they attempt to thread their way through masses of other people. We have, as examples of recently developed disciplines within applied mathematics, game theory, operations research, statistical decision theory, data processing, information theory, econometrics, sociometrics, psychometrics, biometrics, human engineering, and others. These are all large and lusty babies, crying for attention and for the public bottle, and all promising to grow up to become world shakers. To indicate their degree of growth let me give you this comparative figure: the first issue of the *Math Reviews* published in 1940 carried three reviews in these fields. The December 1961 issue carried over a hundred.

Though the abstract mathematical bases of these fields do not lie as deep as, say, that of quantum physics and could have been promulgated many decades ago, there are good reasons why we had to wait till the 1950's to see a blossoming. In the first place, social situations require the handling of a great many pieces of interlocking information. Prior to 1946, when a man computed with pencil and paper, or had an adding machine to help him out, ten simultaneous equations in ten unknowns would make him groan. Twenty equations would be at the limit of his endurance. Now social situations involve hundreds, thousands, millions of people, and when the interplay has been cast into mathematical language, billions and trillions of arithmetical operations may be involved in order to arrive at a prediction. To cope with arithmetic in this bulk requires the abilities of a high speed electronic computer. Electronic computers with great capacities for retaining information and with unbelievably fast operating speeds are now commonplace and there is therefore the possibility of making concrete the abstract formulations of econometrics, etc.

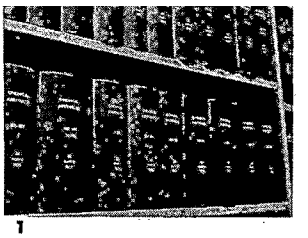
There is a second reason and it is a psychological one. For many years the social and economic scientists have suffered from an inferiority complex. They would look across the halls to their colleagues the physicists and envy their wonderful successes in planetary theory or in electromagnetic theory. "We must wait for our Newton," seemed to be their attitude.

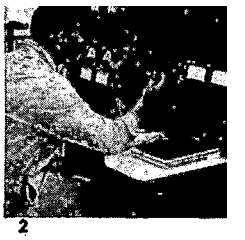
The philosophers of physics took a more modest view of the achievements of their field. They pointed out that the successes were limited to relatively simple situations. Professor Philipp Frank of Harvard used to dramatize this point by holding a thin strip of paper in the air and allowing it to flutter to the floor. Though the basic laws of mechanics and aerodynamics are known, no physicist, he would assert, could predict the resting place of the paper. The philosophers pointed out also, that, since the collapse of classical physics at the turn of the century, the physicist has a more modest opinion of what physical law is. It now appears that such laws do not come from the Cosmological Writ, but from the minds of man; and that they are mere "models" of reality, of limited applicability, to be used or not depending upon the purposes of the investigator.

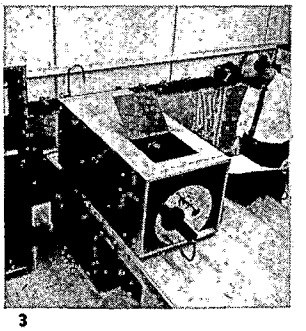
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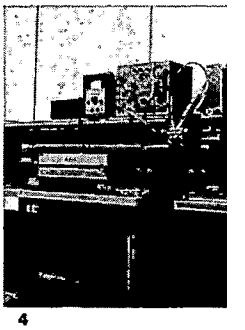
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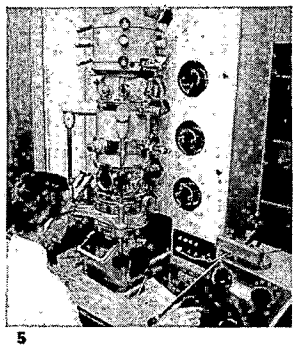
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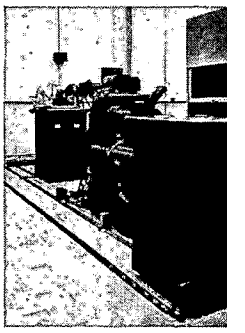

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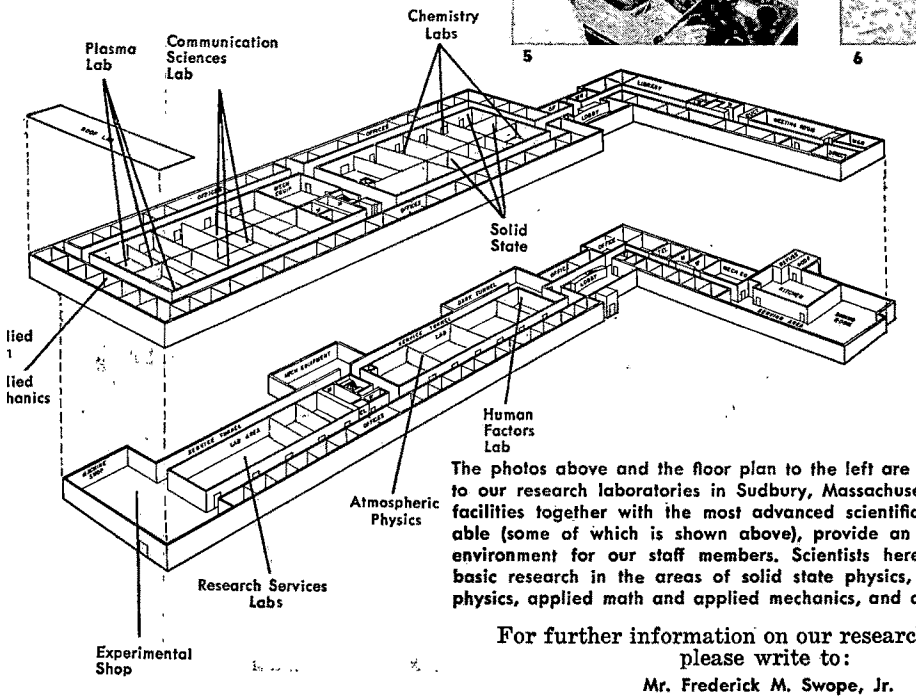
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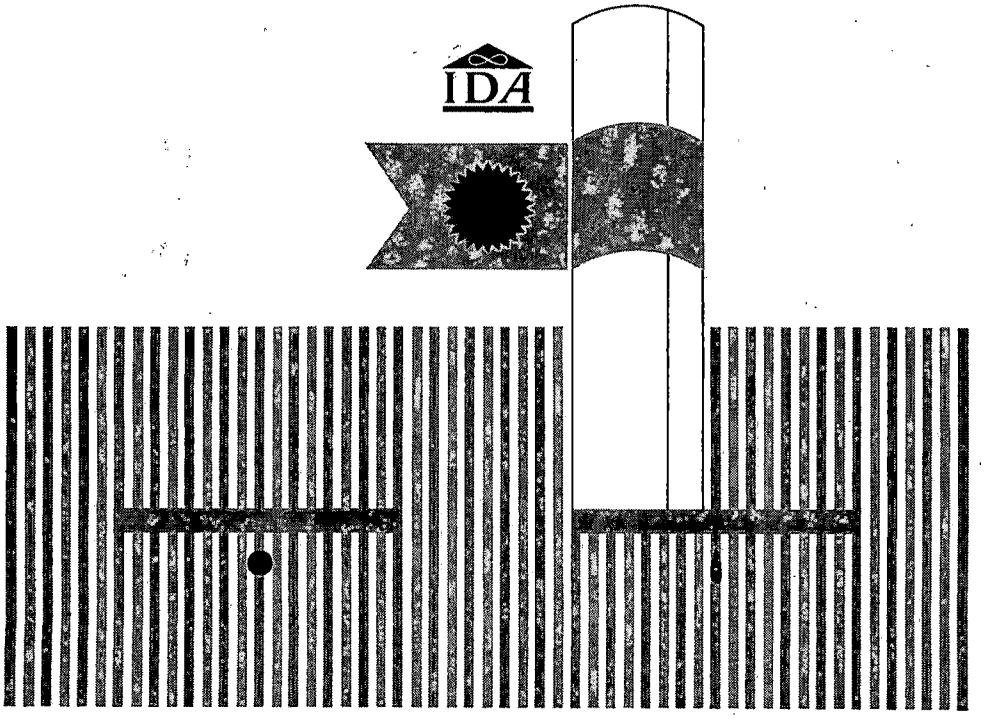
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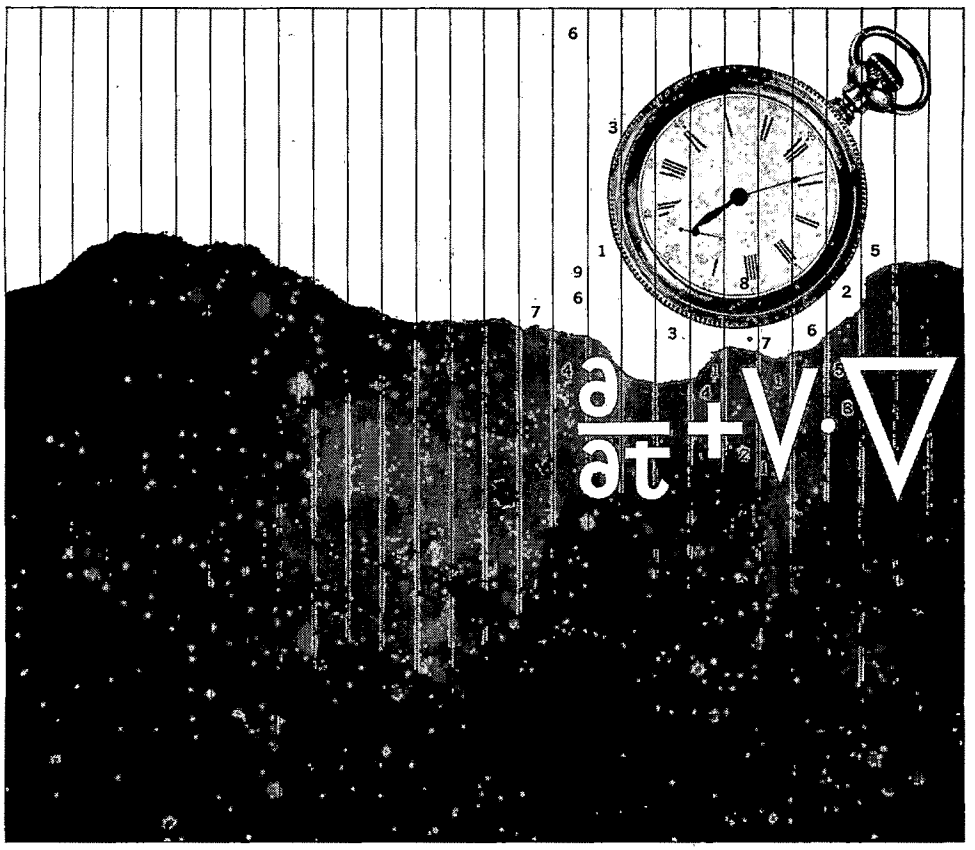
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(*Continued from page 260A*)

cist and from his notion of a theory as a mathematical model, the social scientist took heart. He crossed his fingers, he simplified, he built models, he hoped that on this platform of work a Newton might stand, but he would settle, in his own day, for a Galileo or even a Ptolemy.

But I must return to mathematics and social policy. It would be out of place for me to give formal definitions of these new areas of applied mathematics. But I must show how they can affect our day to day life. I will give a number of examples. Some are familiar: they are the stuff out of which misleading Sunday supplement articles are written. Some are for the future in the brave new world. None is particularly controversial. All illustrate the way that the mathematical outlook can influence social policy. All illustrate the method, the attitude, the process of criterion formulation that makes the whole thing work.

A. *Marriage by UNIVAC.* "America Makes a Marriage Machine" says an alliterative front page headline of the *London Observer*, Sunday, February 25, 1962. This idea has been around so long that the public is aware of its general features. There is available to a data-processing machine a large pool of information on men and women of marriageable age; their ages, social and religious backgrounds, occupations, education, interests, requirements in a mate. In other words, all the information that a marriage counsellor might consider pertinent. For a given person selected, the machine will go through its store of information, and come up with the names of those of the opposite sex who would make the best matches. What is crucial here is an a priori formulation of the measure of the "distance" or the "closeness of match" of a given male, female combination. This measure, which may in part be numerical and in part logical, is the criterion which the machine uses for selection. The machine simply computes this measure for all pertinent combinations of individuals, and selects those where the measure is best. The machine does not formulate the criterion. This is done by the master of ceremonies, the marriage counsellors, or any other interested individual who thinks he knows what ingredients make better marriages than the romantic process. The mathematical outlook here consists of the notion of the measure, and the possibility of formulating it, the notion of minimizing this measure, and the utilization of electronic machines to carry out the bookkeeping rapidly and efficiently. It is, in fact, a modernization of the way in which marriages have been made in most societies in many periods of history.

The idea, at the moment, is a gimmick. It is shocking to some who prefer to see marriages made by the random processes of romance and to others who view a machine as a blind instrumentality rather than as the updated little black book of an efficient and knowledgeable matchmaker. It is interesting to those who believe that marriages ought to be made on the basis of rational selection. At any rate, there is no evidence to show that UNIVAC marriages are less successful than those contracted by couples who have met on the escalator at Macy's. Quite the contrary, according to my London article, the Scientific Introduction Service of New York claims that one divorce has resulted from 500 marriages made in this way since 1956. The national rate is one in four marriages.

B. *Occupational Placement.* The question of testing to determine occupational placement is related to "A," but is scarcely a gimmick. Deep theories of mathematical statistics are employed in the proper formulation and validation of the tests. There are millions of dollars and thousands of people tied up in it. The value of the method varies from application to application. Testing for scholastic achievement has, for instance, shown considerable predictive value. On the other hand, extensive work has been done in the area of pilot selection and some critics feel that after one has accounted for such obvious factors as health and general intelligence, further placement testing has accomplished little. A criterion must be formulated of the best measure of fit between individuals and occupations.

One of the decidedly unpleasant as-



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pects of reducing an individual to a few marks on a sheet of paper or to a few holes on a punch card is that his future can hang by an irrelevant thread. Here is a true story I heard recently. A student at a midwestern college was given a series of tests when he entered. On the basis of these tests, the dean advised him that he had a bright future ahead of him in architecture. The student took appropriate courses, did poorly, and was miserable. Two years later, the dean called him back and told him (ha, ha! and chin up, Smith!) that the office had confused some of the identification numbers.

C. *College Placement.* Every year the Navy has several hundred medical internes whom it must place in various medical hospitals over the country. Each doctor has certain qualifications and certain preferences as to where he would like to go. Each hospital has a certain capacity and certain requirements. Until several years ago, the assignments were made by hand, using trial and error, a formidable job. Today, the assignments can be made by electronic computer using the mathematical methods of linear programming. What is necessary here is to formulate a criterion for determining which, of two given total assignments, is the more desirable. The doctors are given their choices consistent with the internal constraints of the total assignment. Assignments have been produced by a machine and have been surveyed by a reviewing board. The results are said to be a great satisfaction to all concerned.

This method has wider applications. Think of a college that has room for 1000 freshmen and has 5000 qualified applicants. Each applicant has particular qualities, and the deans have a picture of what the freshman class should look like. Surely there is room for a bit of mathematics here to relieve the intense problem of paper juggling. Think, if you like, of a still wider problem. Most applicants apply to two or three colleges. This results in an enormous duplication of paper. Perhaps groups of colleges in certain categories ought to pool their admissions work. With an electronic computer or two and a workable criterion for admission in the presence of competition, the admission officers could push a button, and go back to the blackboard and lectern where their hearts in many cases really are.

D. *Flow of Automobile Traffic in a Tunnel.* A stream of traffic is flowing through a long tunnel. Maximum and minimum speeds are posted. There are inclines at either end. Each driver accelerates and decelerates according to their speeds. There is a distribution of reaction times and of rates of acceleration. What happens in the tunnel? How does the flow proceed? What does the spacing of the cars look like statistically? How often does a car plough into the one in front? Do bottlenecks form and how can they be eliminated? What is the maximum safe capacity of the tunnel in cars per hour?

These questions can be answered by new computational techniques known as "simulation." An abstract model is set up of what are considered to be the crucial aspects of the physical situation. The model is then "run" on a high speed electronic digital computer. It is a form of mathematical experimentation. Though it does not involve the manipulation of physical models, it has many features in common with it. The experimenter observes what happens and interprets the results. He could then change the conditions and again see what happens. With two or three holes on an IBM card, he might raise the maximum speed to 60 miles per hour or lower it to 10. He could introduce a drunken driver, i.e., one with a very slow reaction time, and see what happens. He can observe the over-all patterns. For instance, it is known that the flow of traffic exhibits certain of the characteristics of gases with shock waves of cars developing a traveling back through the tunnel.

In experiments carried out at MIT for the Port of New York Authority, a program of admission to tunnels in "platoons" was developed which eliminated bottlenecks that appear when the car density becomes sufficiently high. This system also increased by 5 per cent the amount of traffic that the tunnel could carry.

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The Bureau of Standards has been simulating the flow of traffic over a portion of downtown Washington, D.C. The model is complex, the code is long, the results are tentative. But you see what kinds of questions are potentially answerable. Where shall we put the lights, how shall we stagger them, what should be the posted maximum speed, should we make this street or that street one way?

As people multiply to the point where they collide, react, and combine with one another en masse, what is observed bears strong resemblance to gases where myriads of molecules are in mutual play. The laws of history, yet to be discovered, may be to the patterns of Spengler and Toynbee as the partial differential equations of gas dynamics are to the ancient saw that "nature abhors a vacuum."

The method of modelling and simulation, brought about in part by studies in probability and in part by the existence of extremely rapid and versatile computation facilities, offers wide potentialities for application. Many groups ranging from government laboratories to university business schools are learning how to use it. One of the great advantages of abstract experimentation is that one is able to change the assumptions quite easily, whereas to do so in real life may be impossible or undesirable. We cannot safely introduce a drunken driver into a real tunnel and observe his effect under a variety of traffic conditions.

But though it promises much, the method is still wet behind the ears. An abstract model, after all, can mimic only certain aspects of reality. In a given situation it may be hard to pick out those elements that are important. In addition to the mathematician and the statistician, the sociologist or the psychologist may be needed.

E. *Relief for the Straphangers.* A task of major proportions is now facing a group of scientists at Columbia University. They are studying the daily flow of traffic on the subways and busses of New York City. Their object is to promote this flow. Perhaps the way to eliminate the terrible crushes is to stagger the work hours. But if so, how shall the staggering be scheduled in different parts of the city and in different industries? A man working in Brooklyn reaches his home in the Bronx an hour later. One industry has natural ties with another industry and should, logically, work the same hours. To determine a good policy, one must unravel a Gordian knot of changing conditions and mutually contradictory requirements. When proposals are finally made, the method of mathematical modelling and simulation will undoubtedly have played its part and the criterion maker his role.

F. *Life and Love in a Small Town.* Still more complex is the economic life of the nation. Economists can argue night and day about the domestic and international effect of lowering the cost of autos $100, or of raising the minimum wage by 25¢ without coming to any agreement. With modelling and simulation, one can study the effect of changing economic conditions. These techniques offer the possibility of converting economics into an experimental science.

Recently, at MIT, certain aspects of the economic behavior of a small town of 2000 people were modelled and simulated. The population was distributed appropriately in age and in economic status. Month by month, these abstract people, inhabiting the memory cells of an electronic computer, marry, have children, get a raise, lose their jobs, buy a house, a car, die, all on a probability basis according to reasonable statistics. The computer invents and keeps track of hundreds of thousands of transactions. A ten-year period has been simulated on an IBM 704 computer in 10 hours' time. The results are promising and it is not too much to expect that, in the future, modelling will yield information that will be of importance to the economist, the industrialist, and ultimately to the legislator.

G. *Peanut Butter and Potatoes.* What foods should be purchased and in what amounts to provide a life-sustaining diet at a minimum cost? This is a problem that falls under the heading of "linear programming" and has been solved in numerous special instances. As an example, selecting from among 77 different foods and asking for minimum requirements of 9 nutritive elements, and using 1939 prices, the low cost diet was found

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to consist of flour, corn meal, evaporated milk, peanut butter, lard, beef liver, cabbage, potatoes, and spinach. To arrive at an answer, one must examine the costs of the thousands of possible minimal diets. The computing machine, reinforced by the theorems of linear programming, can do this in a reasonable time.

Forget the prestige-laden and expensive shrimp for the moment, and think of the undernourished nations. Perhaps it is important to know at any given time what the list consists of, and to solve the problem subject to the local constraints of availability as well as religious and social taboos.

In a similar vein mathematically, but far removed from peanut butter and potatoes, is the question of the optimal allocation of resources. A company wants to put up a new plant. Where shall it build? Where labor is cheap? Where raw materials are plentiful? Where large amounts of power are available? Where the market is close? Mathematics can say much here, but someone must first formulate a criterion of desirability. Maximum profits to the stockholders? Maximum social benefit in some wide sense?

To make this dilemma real, consider the problem of New England. It has been losing its textile mills since 1920. The amount of human misery caused has been enormous. We can alleviate it either by introducing special conditions which maintain the mills there in contradiction to the natural conditions which are forcing them out, or, we can view it as a nation-wide problem on the optimal allocation of resources, and allowing the industries to fall where they may, alleviate the suffering by drawing on the total economic resources of the nation to reconstitute the affected population. The first solution turns legislators into special pleaders for their constituents and makes them far less liberal than national leadership requires. The second may be said to be the mathematical solution and the one which, despite obvious difficulties, must prevail in the long run. If this country dumps butter abroad, the man on the street in Denmark may be hurt. The computing machine offers the possibility of coping with the interlocking relationships of world trade. We are our brother's keeper but are only gradually achieving the means to study out how one man affects his neighbor.

These examples can be amplified many times. From the express counter in supermarkets, to automatic diagnosis of disease, to matters affecting defense policy, these are instances of the increasing importance of the mathematical approach.

The scientist, the computer expert, sits at his desk and smokes his pipe. Let us suppose he has a bit of the utopian in him. He dreams a dream. What is it? The world is complex. The population is increasing. The demands are many. The points of attraction between individuals and between groups are many. The antagonisms are many. Perhaps the power of mathematical thought and the use of computing machines will help us process the trillions of bits of relevant information. Perhaps we can control properly the flow of people and equitably the flow of goods and satisfactions. Perhaps here and there order can be established out of chaos. Perhaps the hungry can be fed, crime and civil disturbance abated, wars quelled. Perhaps, in this way, we can be led to the gate of the Garden. With this thought, exhilarating but sobering, the dream evaporates and our dreamer returns to the real world.

What are the dangers of the dream? But wait! We are hardly beyond the stage of taking a number for "next" at the taxi stand; why talk about the dangers? I think it is crucial to do so, for in anticipating what they might be, we shall be in a better position to guide our actions.

I have said the dangers of the dream, but I really should have said the dangers of the method and the attitude—for the dream itself is a noble one. (1) The method can be misapplied, imperfectly applied, or applied to wrong ends. This possibility is so obvious—think of atomic power—that it needs either no further comment or to have us all get up and shout it from the rooftops. (2) The method becomes the basis of a scientific priesthood, generally composed of sec-

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ond-raters, which isolates itself to protect its position and does not communicate with the lay public. (3) Worse than this, and as Professor Norbert Wiener has pointed out, the machine, with the method built in, can become a sorcerer's apprentice, pouring out satisfactions at one end and chaos at the other, and the satisfactions imparting to it the status of a holy, untouchable, un-turn-off-able object. (4) The method requires a criterion to work. The criterion is frequently extra-mathematical, extra-scientific. Conflicting criteria can cause strife and dissension. As the great comedian Willie Howard said years ago, "Comes the revolution, you'll like strawberries and cream." Today he might say, "Comes the mathematical revolution, you'll like peanut butter and potatoes."

There is a final danger that comes from the mathematical attitude. To see what it is, we must examine the mathematicians' outlook.

As individuals, confronted with their own problems of daily living, they are as bright and as stupid, as liberal and as conservative as nonmathematicians. But collectively, in their institutional capacity, so to speak, there is a certain attitude that emanates from their work. It comes from the great mathematical discussions of the past which are now being brought to bear on the affairs of the world. Mathematicians employ the axiomatic method with hypothesis leading to conclusion via logical operations. They are adept at symbolization, abbreviation, strict definition. They show a great interest in relations, in symmetry, pattern, and variation. They revel in abstraction and generalization. A mathematician is apt to see what is arbitrary or conventional and what is basic.

The mathematical outlook displays great willingness to alter the hypotheses, to simplify and change the original problem in order to progress. The mathematician lives in horror of circular reasoning and of inconclusive proof. There is a universalism to the method which appears beyond the clutches of dominant philosophies and politics. After all, we and the Russians and the Chinese agree on the multiplication table. When we probe deeply into the workings of the examples I have told you about, we find all of these elements present.

There is an opposite side to the coin. A curious neutralism emanates wherever mathematics comes into contact with the world of men. The mathematician approaches calmly many things that are emotion laden. The difference between monopoly and duopoly may be the basis of an antitrust suit lasting a decade. To a mathematical economist, it may be "merely" an additional term in a set of equations.

Despite the potential applicability of his modes of thought to the real world, the mathematician exhibits an aloofness to it. He derives the statement p implies q, and is interested only in the implication and its derivation, but neither in the antecedent p, nor in the consequent q. "If you tell me what to maximize, I will try to tell you how to do it." This is what he says, and he means it. The onus of determining what is significantly maximized is on others.

But the onus is not passed on so readily to others. The mathematician knows the method. The method demands a criterion to make it work. Willy-nilly, the mathematician will be forced into the role of the criterion maker. I fear he may be singularly unsuited for this role. He tends to be impatient. He tends to be rigid and authoritarian. He is a true or false man, a zero or one man, a right or wrong man, a yes or no man. But the world, we know, is more complex than this. It is full of paradoxes and contradictions, unresolved and unresolvable. Can the mathematical attitude resolve the paradox of Job? In an attempt to do so, it might do considerable damage.

When a man steps onto the platform and takes a number, he is sped on his way, and that is good. But when he takes a number, he becomes, in part, a number, and there's the rub.

What is the solution? The solution, it seems to me, lies in the cultivation of strong values that lie outside science. We must have more education for scientists in the humanities, in history. We cannot afford to be ignorant



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mechanics. We must have less rigidity of thought. We must avoid a scientific priesthood. The solution also lies in mixing science and technology with the rest of life in proper proportions. I should like to preach the golden mean, an unexciting but vitally necessary doctrine.

We must remember that though Mathematics may be the Queen of the Sciences, Science is not the only principle of life. "The tree of life is larger than the tree of thought," said Emmanuel Kant. I believe it. Let us cultivate men of the mind who will also be men of the heart.

PARKINSON'S LAW, THE SQUID, AND PU

By R. T. HERSH

Recently Dr. Pardee [1] has introduced a new quantity of pU into Biochemistry. In order to keep this factor in proper perspective and since it has only been recently discovered, the writer feels that a few historical comments are pertinent.

As with any great generalization in science there have been other equivalent statements of this law in other branches of knowledge. The overlooking of these equivalent statements is due to the high degree of specialization of scientists today and the rush for publication. E.G. Conklin [2] referred to a statement attributed to Professor Farlow of Harvard University that today's sciences can be compared to the ejection of large quantities of ink by the squid while moving backward. Pardee's Law states that an inverse relationship exists between the uniqueness of an observation (U), and the number of investigators reporting it simultaneously. By equating the number of investigators (N) to the large quantity of ink (I), it can readily be shown that the U factor is related to the squid statement referred to above. ($I = \Sigma i_j$ where i_j is the ink ejected by the jth author and the summation goes from $2 \ldots\ldots N$, i.e., all other authors).

Both these statements may be shown to be related to Parkinson's Law [3]: "Work expands so as to fill the time available for its completion."

First one must point out that biochemistry is a vocation and can thus be equated to work (in the socio-economic sense, not physical sense). Secondly, dropping for the time a thorough discussion of four dimensional space [4] (space-time concepts), space may be added to the word time in Parkinson's Law, and thus a corollary to this noted statement is: "Biochemistry expands so as to fill the space and time available for its completion and publication."

We thus see that the recent statement of Pardee of Princeton is closely related to the work of a former Princeton professor and to the work of Parkinson.

Department of Biochemistry
The University of Kansas
Lawrence, Kansas
June 22, 1962

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LETTERS TO THE EDITORS

DEAR SIRS:

In reference to Cyril Comar's article, "Biological Aspects of Nuclear Weapons" (AMERICAN SCIENTIST, June 1962), it is implied that the driving of cars and the testing of bombs are analogous risks. The risks are similar in degree but different in kind. When a person chooses to drive a car, he accepts the risk of accident. It is the voluntary acceptance of a small risk for a large gain, and it is his own risk only that is involved. When bomb testing, although the risk may be small depending on the value one places on human life, and although the gain may be large depending on one's philosophy of the use of force, it is a risk that is placed on all human life. It is to say, "For my gain you shall have to accept this risk also." No person has the right to place risk on others for his own gain. It is improper to attempt to make the risk of bomb testing acceptable because it resembles the risk of

TECH NEWS

For Scientists, Mathematicians
Operations Evaluation Group



One of our analysts has returned from field assignment with the fleet and told us a significant improvement resulted when one of his recommendations was put into practice during fleet maneuvers. OEG's field activities, assigned on a rotational basis, represent unique travel opportunities for scientists and mathematicians. There are OEG men with the fleet in the Mediterranean, the Far East, Hawaii, Key West, Norfolk, and San Diego, and field representatives in Newport, R. I. and London, England.

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automobile driving. Whether or not the testing should be done, the difference in the two kinds of risks has to be faced.

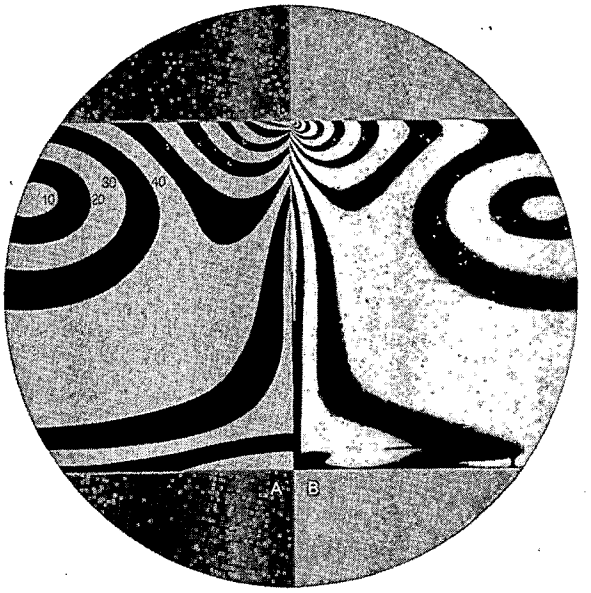
Very truly yours

J. CHANDLER SMITH, M.D.
Division of Radiation Biology
Department of Radiology
Western Reserve University
Cleveland 6, Ohio

In reply, the author made the following statement:

J. Chandler Smith mis-implies and, I am afraid, misses the intended point. In the order raised: the risks from the driving of cars are certainly not similar in degree to those from the testing of bombs; the driving of cars wreaks much greater havoc on the human population of the United States than does the testing of bombs. In our highly organized society voluntary acceptance of risks is an ideal, seldom to be attained realistically. For example, many of us have to drive cars to do our work and live where we want to; we accept traffic codes, sanitation codes, housing codes, and numerous actions taken on a group basis for the benefit of the whole population. It is obviously illogical to attempt to justify one risk on the basis of its comparison with another risk, and I would repeat the statement made in the article, which to my mind emphasizes the important point: "These figures are not quoted to justify any unnecessary additions to individual suffering, but merely to emphasize that normally we do not have personal anxiety about such risks." We do not protect our children by keeping them out of automobiles or by not taking them for rides in the country, and it is just as logical not to try to protect our children by trying to individually-manipulate their diets as many people are proposing. Of course, the fact that individuals need have little anxiety about personal well-being does not mean that duly organized authorities should not take cognizance of the situation and take whatever steps are possible.

PHOTOELASTICITY



The analysis of stresses caused by thermal shrinkage has always been a knotty problem, especially in complex three-dimensional shapes. The two figures above compare the results of a complex analysis (A) with an experimental photoelastic study (B) so that even nonscientific members of the solid propellant community can see the progress we have made in three-dimensional stress analysis. The photoelastic picture shows the "fringes" from a slice of a thick-walled cylinder of photoelastic resin cast into a steel tube, with the resin shrinkage producing the same distribution of stresses that occur upon cooling a rocket motor. The analytical figure shows the calculated contours of tensile stress using the computer to handle the finite-length problem through the stress function technique. ■ The success demonstrated by these illustrations shows the progress that may be realized when scientists with diverse interests, such as stress analysts, grain designers, organic chemists, and rheologists, work toward a common goal in an environment conducive to research. Management people would say that we have improved our communication; we scientists say we have improved our ability to handle difficult design problems.

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(1962 publications unless otherwise noted)

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A Century of Science Fiction, edited by D. KNIGHT; 352 pages; $4.95; Simon & Schuster.

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Introduction to Electronic Data Processing Equipment by R. V. OAKFORD; 340 pages; $10.00; McGraw-Hill Book Co.

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Science and Religion by P. CHAUCHARD; 156 pages; $3.50; Hawthorn Books.

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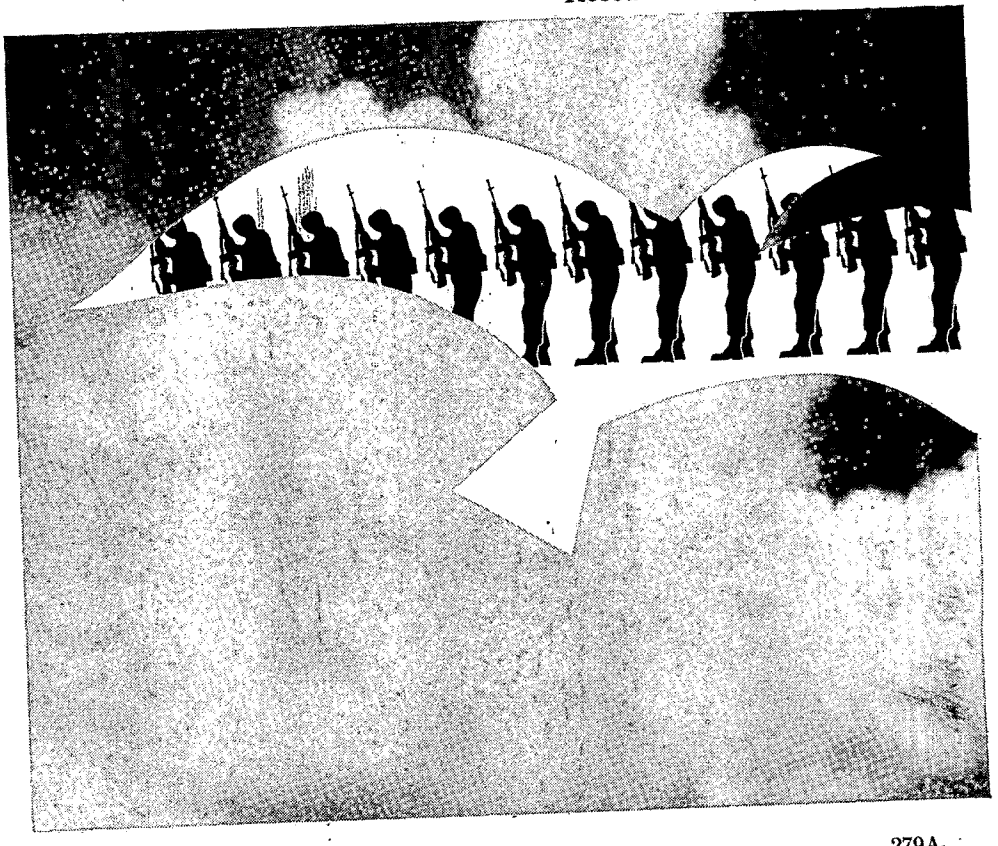
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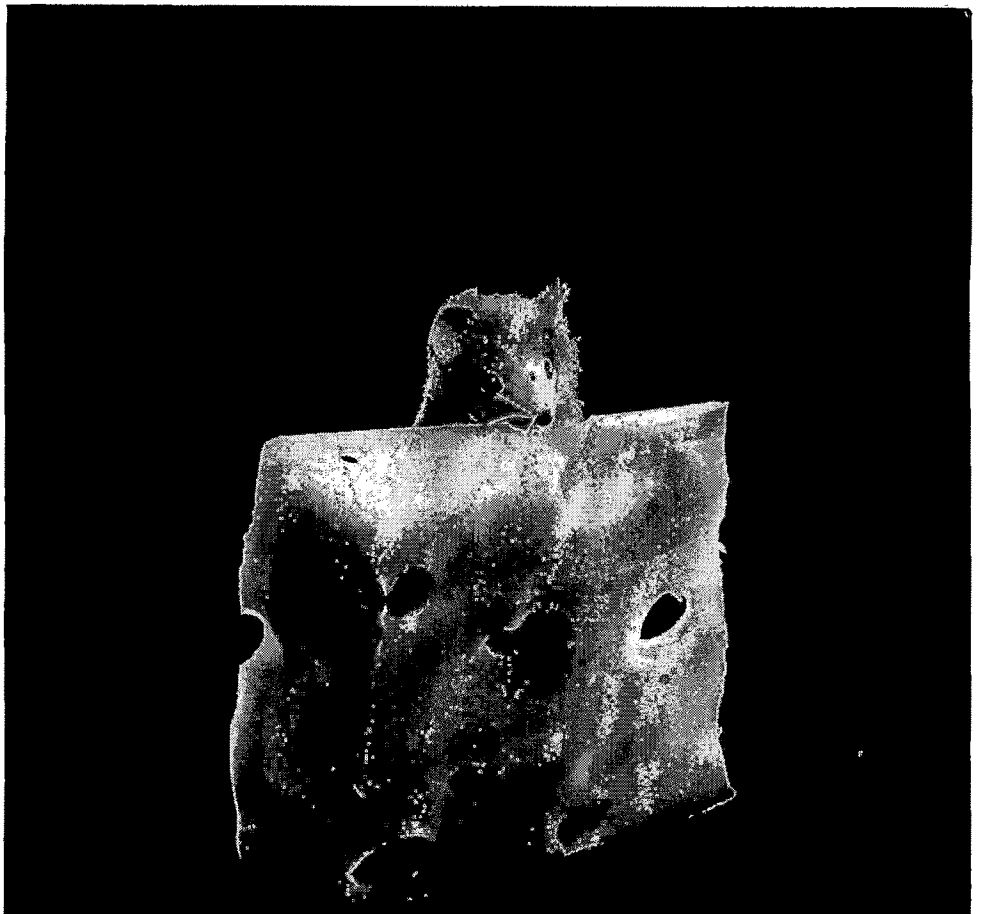
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Signal Flow Graphs and Applications by L. P. A. ROBICHAUD, *et al.;* 214 pages; $9.00 trade edition; Prentice-Hall.

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A Geography of Manufacturing by E. W. MILLER; 490 pages; $12.65 trade edition; Prentice-Hall.

Proceedings of the Symposium on Nuclear Instruments, Harwell, 1961, edited by J. B. BIRKS; 252 pages; $10.00; Academic Press.

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The Behavioral Basis of Perception by JAMES G. TAYLOR; 379 pages; $8.50; Yale University Press.

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Tectonics, by JEAN GOGUEL, translated by HANS E. THALMANN; 384 pages; $10.00; W. H. Freeman and Company.

Mathematics for the Physical Sciences by H. S. WILF; 284 pages; $7.95; John Wiley & Sons.

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Quantum Statistical Mechanics by L. P. KADANOFF and G. BAYM; 203 pages; $4.95 paper; $6.95 cloth; W. A. Benjamin, Inc.

Computation of Multistage Separation Processes by D. N. HANSON *et al.;* 361 pages; $8.75; Reinhold Publishing Corporation.

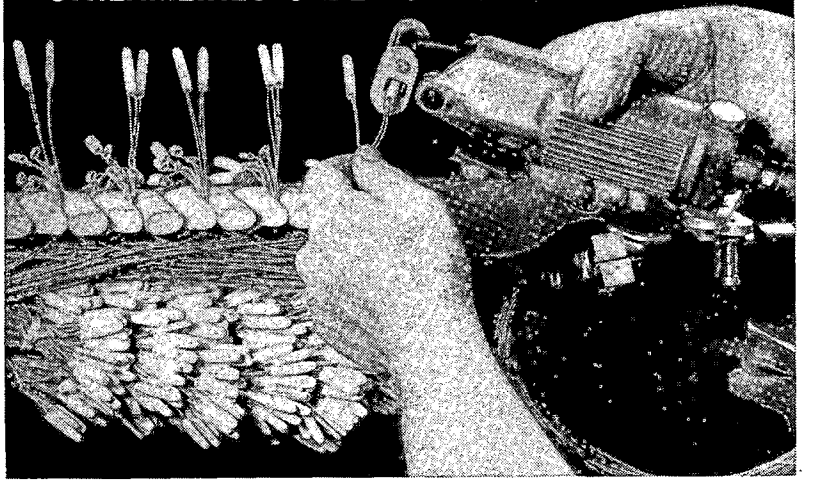
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Neutron Physics, Vol. II (Proceedings of R. P. I. Symposium, May 1961), edited by M. L. YEATER; 303 pages; $12.00; Academic Press.

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Telephone craftsman uses special pneumatic tool to flatten connector onto insulated wires. Metal tangs pierce insulation and produce a splice that is equivalent to a soldered joint.

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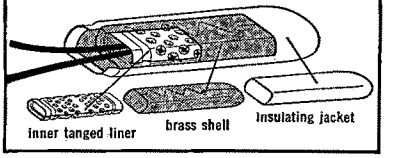
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New wire connector has three parts:



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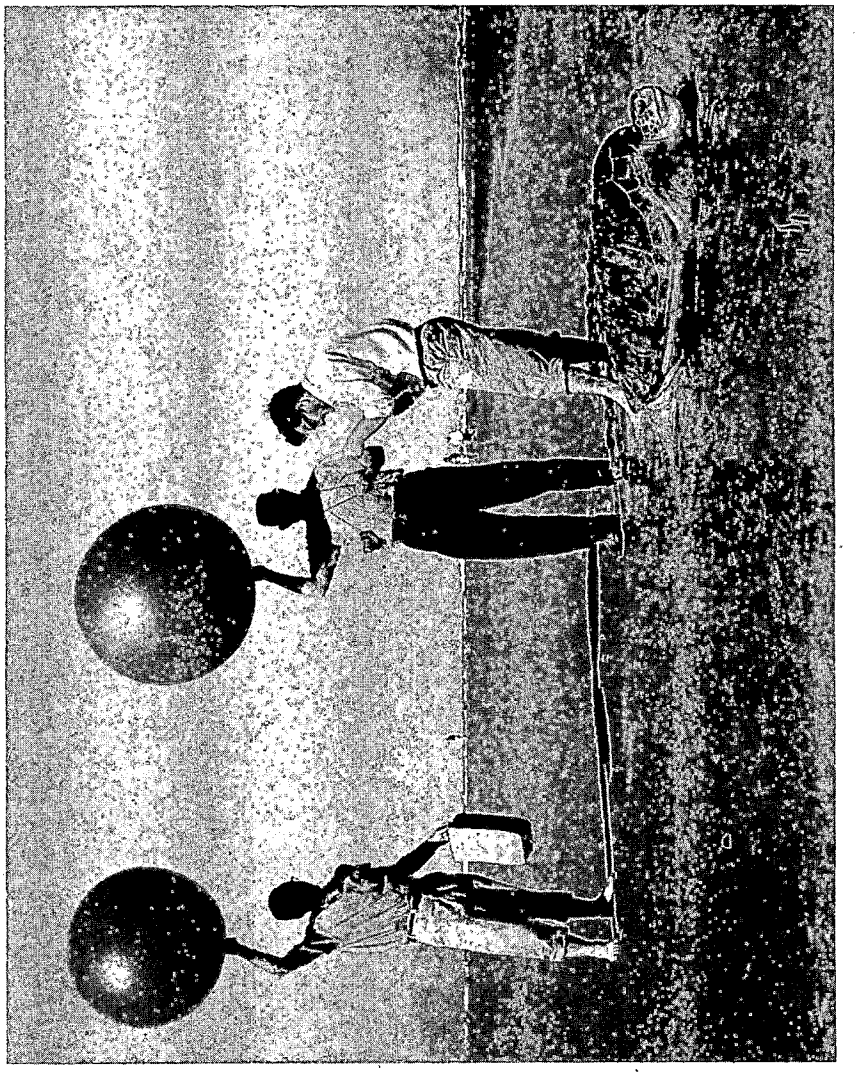


FIG. 3. The rig used in balloon tracking tests. The styrofoam float (held by man at left) was fastened to the hind edge of the shell with 30 ft. of monofilament line. Two balloons rose on 10 and 15 ft. lines. The tow-line swivel was attached to the shell with thin iron wire, designed to rust away within a short time and free the turtle of the rig. All turtles tested were marked with standard fin tags.

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ORIENTATION PROBLEMS IN THE HIGH SEAS TRAVEL AND TERRESTRIAL MOVEMENTS OF MARINE TURTLES

By ARCHIE CARR

STUDIES of American sea turtles made during the past six years[1] have been mainly concerned with piecing out the fundamental natural history of the five genera. One important aim, to establish the long-suspected fact of long-distance migration, has been virtually achieved; but a number of problems await techniques for maintaining contact with the animals through marked changes in ontogeny, ecology, and range. Besides the still unanswered questions of ecological geography, a complex of problems concerning orientation responses, possibly ranging from simple taxes to celestial navigation and piloting by obscure landmarks, will probably be solved only through the use of distant monitoring and electronic tracking methods.

Fortunately, the necessary instrumentation appears to be materializing in the growing technology of telemetry. Telemetry as applied to biology is defined as the use of instruments such as miniature radio transmitters attached to animals, or placed at points distant from the observer, to locate the animals or to monitor changes in the internal or external environments. Application of telemetry to biological problems has had to await advances in miniaturization, and these have been greatly accelerated by the guidance demands of the missile and space programs. A recent symposium on bio-telemetry[2] revealed some impressively

[1] Supported by National Science Foundation Grants: G-1684, G-5479, and G-10717, and by the Caribbean Conservation Corporation.

[2] This paper was presented at the Interdisciplinary Conference on the Use of Telemetry in Animal Behavior and Physiology, The American Museum of Natural History, March 28–31, 1962.

refined and effective applications in physiology and medicine, and also several examples of its use in studying animals in the field. This essay, which was presented at that conference, calls attention to the potential for telemetric tracking and monitoring in the study of the complex orientation life of the green turtle, *Chelonia mydas*.

In a tagging program, carried out at Tortuguero on the Caribbean Coast of Costa Rica, 2000 mature female green turtles (*Chelonia*) have been marked. There have been 64 postseason returns, all but two from outside Costa Rican waters. As Figure 1 shows, the recoveries spread

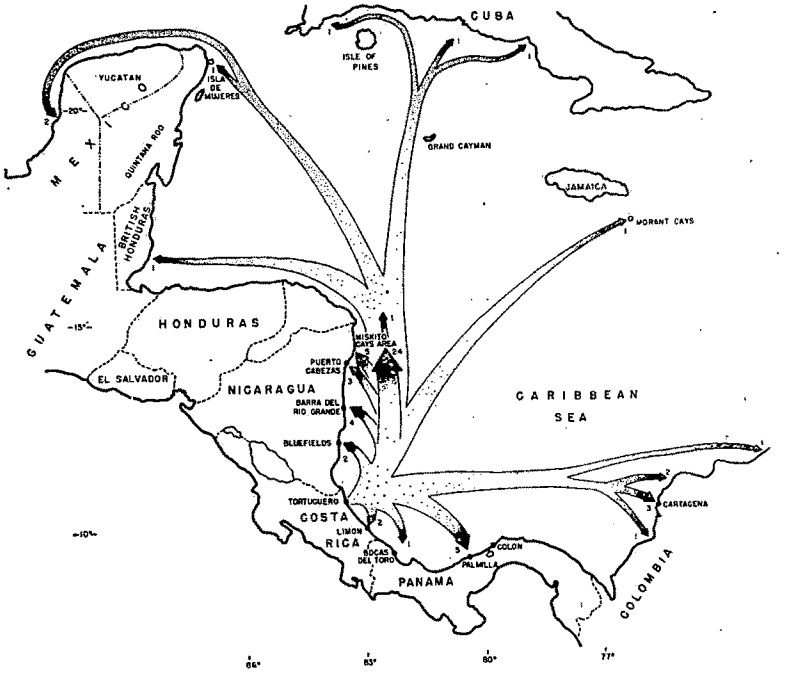


FIG. 1. Map showing recoveries of tags from 2000 mature female green turtles (*Chelonia mydas*) marked at Tortuguero, Costa Rica. The arrows only indicate spread from the tagging locality, and are not intended to suggest routes.

throughout the western Caribbean. Such data almost prove that periodic migration is occurring, but not quite. The green turtle nests on a three-year cycle, with a subsidiary two-year cycle shown by thirty to forty per cent of the rearrivals at the Costa Rican nesting ground. We have retaken turtles in Costa Rica after two- or three-year absences on many occasions, and because there is no resident population in Costa Rican waters these returns reinforce evidence that migratory commuting between Tortuguero and distant pastures occurs. The site tenacity shown by both these long-term rearrivals, and by turtles renesting three

or four times during a single season, adds further circumstantial evidence. One way of proving the case would be through recapture at Tortuguero of turtles tagged there on previous arrivals, recaught on the pasture ground, and released again. We have so far made no effort to persuade fishermen at the Miskito Cay pastures and elsewhere to release tagged turtles. If we had, the expected recovery number would have been only 1.9 turtles.

To inch a little closer toward direct proof of migration we undertook a tagging program on Ascension Island in the middle South Atlantic. The nesting colony there shows trenchant seasonality, disappearing completely when nesting is done; and the resident population of the Brazilian Coast evidently nests nowhere along the mainland shore. The hypothesis that the Brazilian turtles go to Ascension to nest thus seemed reasonable. During the Spring of 1960 Harold Hirth tagged 206 turtles at the island. That the four returns to date have all been from the coast of Brazil (see Fig. 2) seems as near closing the circle of evidence as can be expected from a one-way tagging program. Overshadowing such bits of progress, however, is our complete ignorance of routes and schedules of the journeys and of the cues and senses by which they are guided. This will be relieved only when we are able to keep in protracted contact with the traveling animals.

Once it had been shown to be able to make appropriate landfalls after long travel through the open sea, the green turtle became a promising subject for orientation research. We have made crude tracking tests with three species of sea turtles, using helium balloons rising from towed styrofoam floats (see Fig. 3). In some of these the animals were released on the beach after nesting had been completed, with the aim of learning what a female turtle does and where she goes for the twelve-day interval between her renesting emergences of a single season, or to test ability to keep a seaward course through a natural maze of short coastwise bars. The other trials were made offshore and out of sight of land. Tracking was done with transits, alidades, or an optical range finder. Results have ranged from the ambiguous to the meaningless. Gross procedural troubles cancelled most of the efforts. In the inshore tests the simple act of getting the equipment through surf presented problems. It took several trials to show how deep a turtle cruises, and thus how long the towline should be to preclude dragging the float under, and it was found that nothing could forestall the occasional erratic sounding of a subject passing over a rockpit or other attractive foraging site. There are other disadvantages. Helium escapes in long tests, or the balloons burst if overheated. Hydrogen is looked on with disfavor by boat skippers, and any balloon tracking is likely to end if more than a slight breeze blows or the towline is dragged through floating vegetation. Curious crews of shrimp boats have aborted two of our runs.

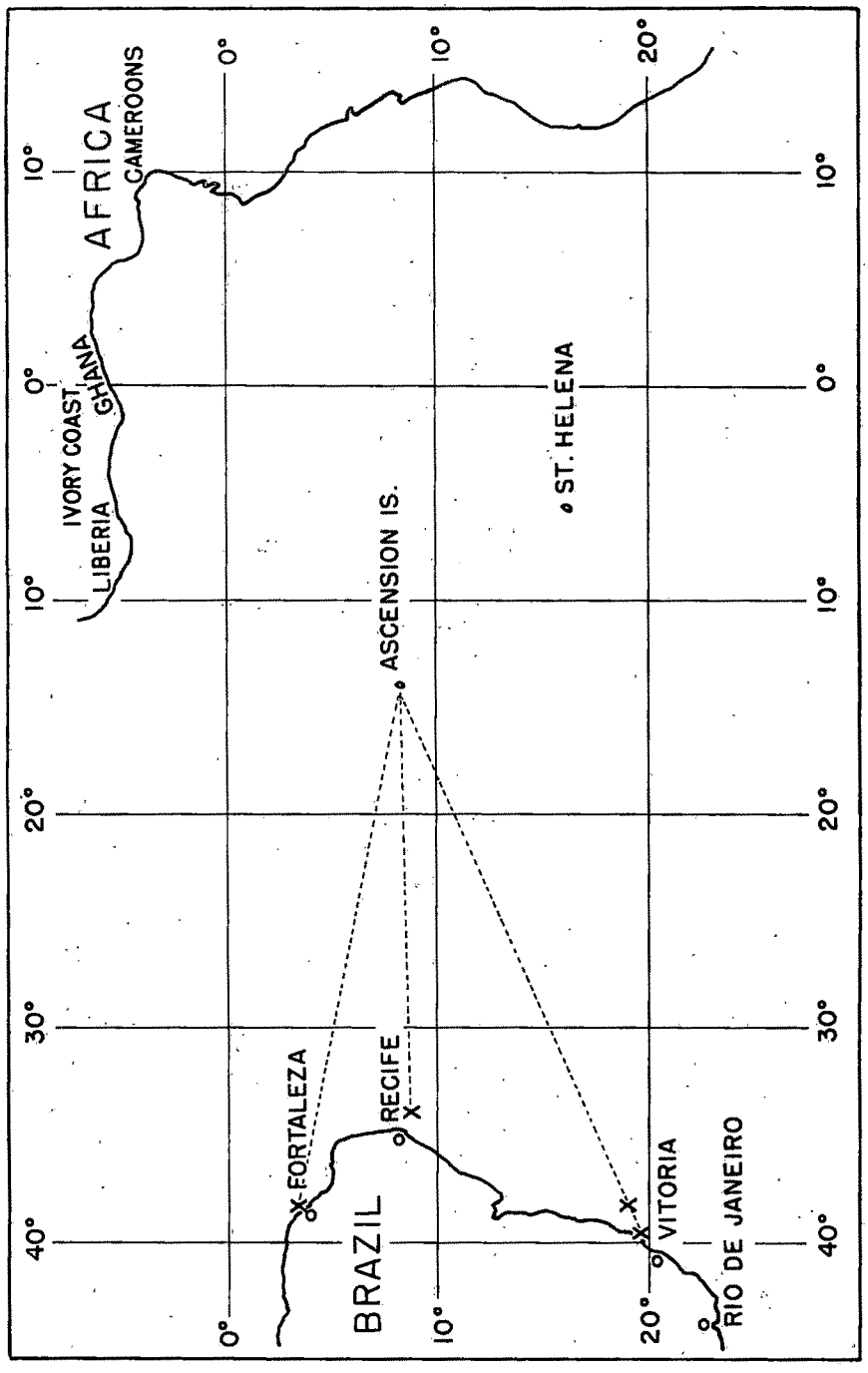


FIG. 2. Map showing the four recoveries from 206 green turtles tagged at Ascension Island in the spring of 1960.

Visual tracking also carries the inherent drawback of loss of contact at night. Although this can be circumvented by installing a light to rise with the balloon, such embellishment of the turtle's celestial sphere blurs the experimental picture. Balloons may likewise mar the daylight tests, but the likelihood that a near bright light overhead would confuse the stellar pattern seems considerably greater.

It is thus clear that, when serious tests are undertaken, telemetry must be used, and, as was said, the genus *Chelonia* seems in some ways a specially good prospect for such manipulation. The most reliable and predictable travel motivation would be found in mature females, which usually weigh from 250 to 350 pounds. The bulk and strength of these, and the free bony overhang of the shell make extreme miniaturization of equipment unnecessary and permit the bolting down of apparatus without undue irritation to tissue. Another advantage is the relatively slow speed of travel—evidently not more than 20 to 30 miles a day in sustained migratory swimming. The aquatic medium is of course unhandy, but at least the subject breathes air and must rise to blow, at intervals of the order of one to three minutes while active, and two to thirty minutes when at rest. The exposed fore margin of the shell is awash during breathing, or at most no more than an inch or so out of water. Thus, if radio tracking were used, antennal elevation would be slight except where emergence occurred on the crest of a wave. In this regard a sea turtle will be a more refractory subject for radio tracking than a flying bird, but much easier to work with than a fish.

The including of *Chelonia* among proved open-sea navigators has indirect bearing on the troublesome question of sun orientation. Because of the absence in the watery landscape of any fixed reference for appraising sun azimuth angle, the sun-arc theory seems inapplicable in over-water orientation. Pennycuick (1960) has suggested that a bird might find its position by measuring (1) the altitude of the sun and (2) the rate of change in the altitude of the sun. Schmidt-Koenig (1961) nas shown experimental grounds for doubting that such a navigational grid is actually used. If this is the case, and if we are to retain a sun navigation theory to explain over-water guidance in the daytime, it would seem necessary to fall back on the idea that azimuth may be measured by using the trend of the waves as a reference. That sea turtles do their navigating from beneath or at the surface of the water would seem to make wave-trend unavailable to them as a reference, and to leave us with no theory to account for their feats of high-seas guidance.

In navigation testing, or in one sort of navigation test, a requisite almost as important as maintaining contact with the animal is knowing what it is trying to do, where it wants, so to speak, to go. Sea turtles present a considerable array of adaptive travel drives, and while the aim of some of these can be assumed by the observer, others, no less strong,

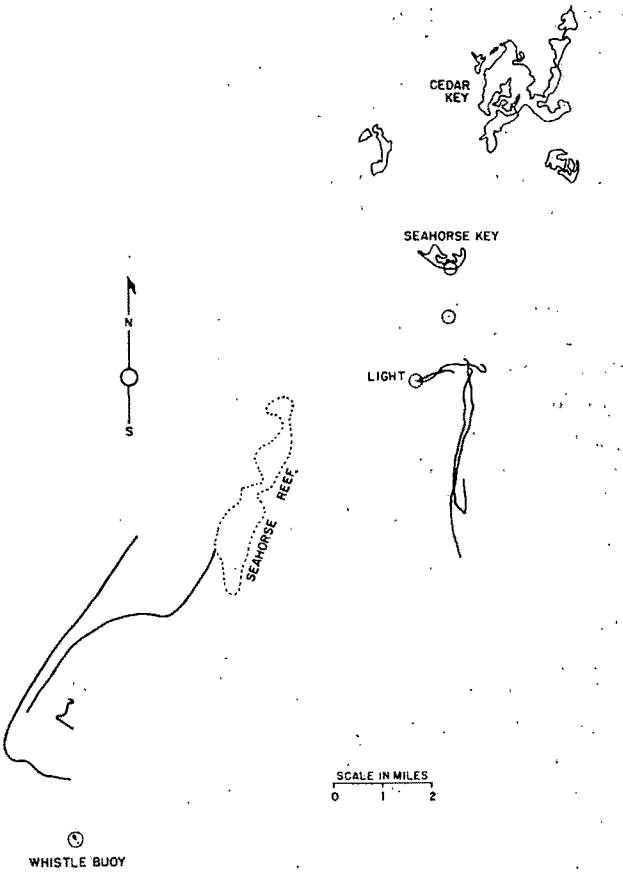


FIG. 4. The Cedar Key area of Florida, showing location of tracking tests made in 1960 with female loggerhead turtles hauled across the Peninsula from Fort Pierce, where they had just emerged to nest but had not nested. The three courses at left were six-hour runs tracked with two alidades, one located on a beacon, the other on a fifty-foot boat. The three course segments at right were tracked with transits on two day-beacons a mile apart, the two southerly courses on July 9, the other on June 26. The time involved in these is shown in Fig. 5.

are without evident aim. The so-called infantile frenzy of the hatchling is a good example. Newly hatched green turtles placed in a tank swim constantly and with frantic energy along and against the walls. If the tank is differentially lighted they congregate at the lighted end; but even there they keep up their desperate swimming for days or weeks. This may mean nothing; but more likely it is associated with the complete disappearance of all kinds of young sea turtles from human sight for at least a year following the crossing of the breaker line at the nesting

beach. The swim frenzy suggests that young sea turtles travel constantly for a long time, and their disappearance seems to confirm the suspicion. Keeping in touch with them would solve one of the most important remaining gaps in a complex life cycle. It seems likely that a latent compass sense is released with the culmination of the chain of events that takes them from the nest through the breakers. But little can be done experimentally until field tracking has shown what use the compass

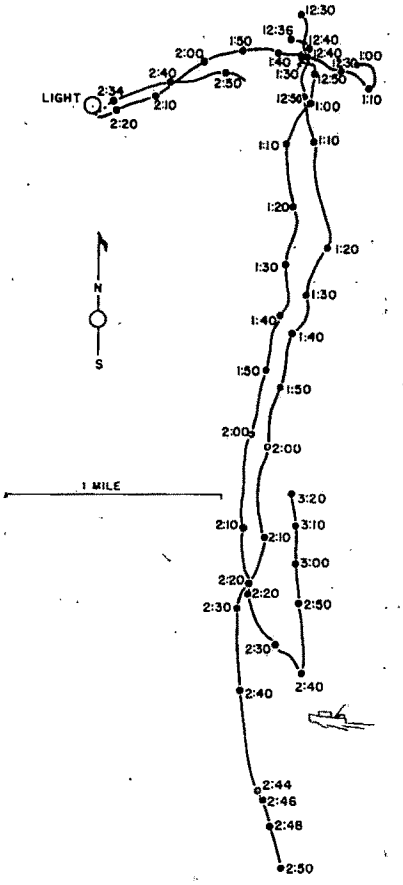


FIG. 5. Courses of three loggerheads tracked off Cedar Key (see Fig. 4). The turtles were marked with helium-filled balloons and tracked by triangulation with two transits, with sightings made at two-and three-minute intervals. The boat sketched at lower right interrupted the course of one of the turtles as shown.

sense is put to in nature, and an accurate forecast of goal-motivation can be made.

This unpredictability of the motivation of experimental material promises to be the most important obstacle in short-run tracking studies with sea turtles. The difficulties are illustrated by the results of six tracking tests we made off Cedar Key, on the Gulf Coast of peninsular Florida (see Figs. 4 and 5). The turtles were mature female loggerheads taken just after emerging to nest, but before actually starting the process.

The nesting drive is exceptionally urgent in marine turtles. At our Costa Rica camp they have come ashore dragging big buoy logs used to mark them for the coasting turtle boat that used to pick them up out of the sea where the hunters had released them. They also, as was said, show a strong site tenacity which, in the multiple nestings of a given season, may bring a turtle back to the same short section of a twenty-mile beach, or to one of many similar little coves around an island (Carr and Hirth, 1962); or to one among many small islands in a close knit system (Hendrickson, 1958). This combination of reproductive drive and site discrimination suggested using displaced ovigerous female turtles in heading and course-segment tests.

The six Cedar Keys trials were exploratory, and we soon saw that they were defectively arranged. They overcomplicated the navigation problem by placing the peninsula of Florida between the animal and its assumed goal. Release in the Atlantic off the nesting beach would have been more logical. The Cedar Key area was selected simply because the line of day beacons off the reef there made optical tracking more feasible than it would have been in the Atlantic, where it would have had to be done from small boats in rough water.

In tests such as these there are three separate questions to be asked: (1) whether the animal is navigating or moving at random; (2) whether the animal is homing; and (3) whether the animal is homing successfully, and if so by what route (i.e., whether if extrapolated, the course, with later corrections, would take the animal home. The alternative situation would be what Matthews called "nonsense" navigation).

In the Cedar Key experiments it was not possible to assume in advance what homing direction would be appropriate. How would a Daytona Beach sea turtle, hauled on her back in a covered truck at night to the other side of the Peninsula interpret her problem? Would she appraise her position as simply "out at sea," and head for the somehow-detected nearest shore? Or, reacting in a more sophisticated way to the same assumption, would she head due west—the way to go if she had indeed been displaced seaward in the Atlantic? Or, assuming that she had a sound familiarity with the celestial sphere but no inbuilt sense of detailed earth geography, would she head for home by the airline route—in this case straight across the Peninsula? Or, as some birds evidently can do, should the turtle be expected to show that most advanced of all orientation capacities: full map sense—earth-map as well as star-almanac—and either head south to circumnavigate the peninsula without seeing it, or move in and follow the shoreline around to her home beach. Obviously there is no knowing any of this *a priori*, and no sure way of extrapolating from short sample segments of courses taken. Such ambiguities are bound to complicate most heading tests with aquatic animals in shore waters.

Although, taken as a group, the six tests show little agreement in initial headings, there are, as Figure 4 shows, suggestions of nonrandomness in some of the individual routes. The two southerly courses, shown in detail in Figure 5, particularly, seem oriented. They were plotted from tracking observations at two-minute intervals and are probably quite accurate. Granting that they prove nothing about heading-motivation, it is still necessary to ask whether the turtles were moving at random, or were navigating or piloting. They were well out of sight of the mainland at all times. From their depressed angle of observation no fixed landmarks were visible for most of the period of their travel. A contour chart of the area shows no way in which isobaths might have been used for reference. The courses cut across a mosaic of water types and through current that varied in direction and strength along the way. Because there were extended periods when the turtles were not in visual contact with each other (they were always in sight of each other's balloons, but this cannot be assumed to have been a bond between them), the stature of the performance is enhanced by its having been done twice. The one conceivably available landmark was a fairly continuous rank of cumulus clouds over the hinterland fifteen or twenty miles away. That a navigating animal should fail to use such a source of guidance data appears unlikely. That it could have kept the turtles so precisely on their southerly heading, however, seems improbable. Perhaps the same can be said of sun-arc guidance in short runs such as these.

All that these and a few other balloon-tracking trials have done is suggest lines of investigation. It will be immensely profitable to know merely whether sea turtles can travel in non-random ways without reference to landmarks. The zoogeography of the group indicates that they can. Balloon tests seem unlikely to furnish solidly confirmatory data. This then is a clear case in which telemetric intervention seems promising.

Once the capacity for open-sea orientation has been experimentally demonstrated, tests can be devised to show how it is put to use in going from one place to another: first through familiar territory, then in homing or position-finding. It is here that sea-turtle natural history will make the greatest demand on telemetry. Throughout its travels the routes the animal follows are obscure. Baby turtles hatched at Ascension and adults returning from there to Brazil might drift passively with the current to within short distances of home territory. For the mature turtles going the other way, however—out to the little oceanic island from the mainland shore—there are at least three logically possible routes: the Gulf Stream system, the Brazil current-West Wind Drift-Benguela current circuit, and the airline upstream route against changing slants of the three-knot South Equatorial Current. The last seems by far the most likely, but it presents a highly complex navigation problem in terms of set and drift displacement.

In the Gulf-Caribbean area we know green turtles well at only three stages of development. Mature females come to Costa Rica to nest, and mature males accompany them to fertilize eggs for the next reproductive period. Hatchlings emerge from the eggs laid there. On the central Gulf Coast of Florida half grown (10–90 pound) green turtles show up in numbers in April and disappear in late October. All the rest of the life cycle is obscure. Consequently, there are few places in the range or life cycle where samples can be counted on to take predictable headings, and the significance of headings or short course segments must be appraised with the greatest care.

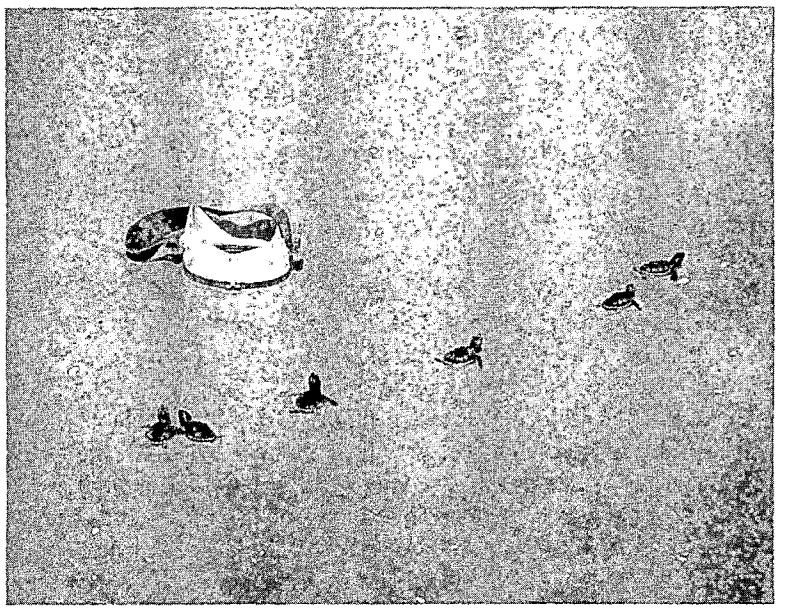


FIG. 6. Six hatchling hawksbill turtles oriented toward the sea (which lies closest at upper right), but showing momentary disorientation by a topical stimulus—the white face mask.

There is one aspect of the orientation life of sea turtles that is easily subjected to manipulatory study, if not easily understood. This is the short, eventful journey of the hatchling group from the nest to the sea. Navigation is evidently not involved in this, because hatchlings taken from a nest on a north-facing Atlantic beach and allowed to emerge on a southwest-facing Pacific shore go through the same behavioral stereotypes and reach the water with the same success as nestmates left at home. Nevertheless, the seafinding venture is a good model for the complex long range orientation feats of the adult. The most fundamental response in the sea-seeking travel appears to be a sort of telotaxis, a tendency to move toward better illuminated sky or toward horizons

free of obstacles. But at various points along the route this beacon is supplemented, or overridden, by other information. Shifts, confirmations, and reorientations occur all along the way. The hatchling journey thus furnishes insight into the nature of instinctive guidepost pathfinding, and supports the growing belief of navigation students that travel orientation is always a composite process.

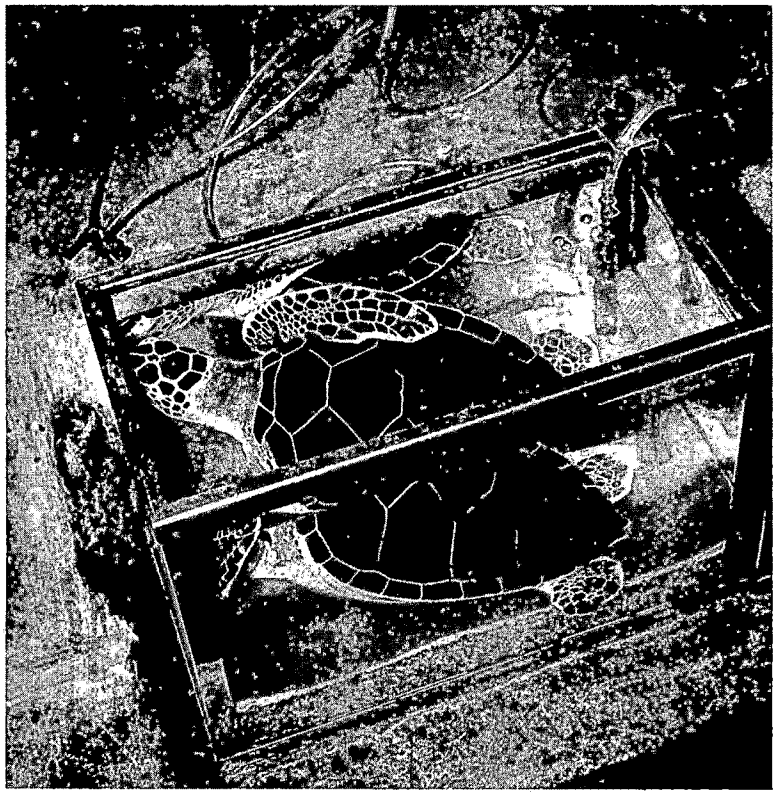


FIG. 7. Yearling green turtle in two-gallon tank in which it was reared. This is one of the turtles used in tests of retention of the hatchling sea-finding capacity shown in Figs. 8–11. This and the other yearling turtles used had not previously seen the ocean.

The trip to the sea begins some 28 inches beneath the sand, where the hatched-out turtles find themselves in a chamber in which collapse of the spherical eggs has given them a bit of headroom. It has been shown that the trip to the surface is not a climbing through the sand by separate individuals, each under the urge of negative geotaxis, as was once supposed (Carr and Hirth, 1961). There is some struggle upward, but the most effective factor in the emergence process seems to be the rising of the whole nest chamber, brought about by a proto-cooperative group thrashing, triggered by the downtrodden hatchlings on the floor of the

nest. The generalized thrashing of the bottom layer of turtles sends a pandemic of activity through the whole imprisoned group. Each burst undercuts the walls, saps the ceiling, and tramples sand into a rising floor. The group spasms are thus automatically converted into a crude division of labor, the net result of which is the upward movement of the roomful of turtles to the surface. Turtles hatching separately at similar depths show markedly reduced survival.



FIG. 8. Yearling green turtle properly oriented on a seaward course, about to go down a declivity separating high terrace and sloping tidal beach; Daytona Beach, Florida.

The time involved in the emergence process is generally about a week. Observations through a glass nest-side introduce artifacts: enough light to see by is an unnatural factor in the nest environment, and so is the glass replacing collapsible sand of the wall. For an adequate study of this interesting case of social facilitation, the need is for instrumentation to monitor the position and activity of individuals of the sibling group throughout the emergence period.

Once out on the surface the problem the hatchlings face is finding the

water, which more often than not is out of sight behind dunes, bushes, driftwood, or other obstacles. They peer about and then start moving, and after short false starts almost always go in the direction of the ocean. They climb slopes, go around logs and scramble through brush and trash, and, except for the freaks or weaklings among them, seem to have solid confidence in whatever beacon it is that tells them where the ocean



FIG. 9. Two yearling green turtles on seaward courses in orientation test; Daytona Beach, Florida.

lies. They can find it by day or by night. The sun or moon may be shining brightly, over the sea or over the land. The weather may be overcast or even raining.

In spite of considerable work by various people (summarized by Carr and Ogren, 1960) with both marine and freshwater turtles the basic guidepost is not thoroughly understood. Some of its characteristics and flexibility are shown in Figures 6–11. It has something to do with the illumination of the sky over the sea. If it were a positive light response in the ordinary sense—simple phototaxis—the hatchlings would be expected to go toward the sun or moon, but only very rarely does this

occur. On the other hand, when a little turtle moves out from behind an obstruction and the sea-sky comes into better view, he is usually stirred suddenly to increased effort. When a wave breaks white under moonlight, little turtles suddenly surge ahead; and it is the same when the surf shows fiery on phosphorescent nights. But even in a flat calm, sea turtle hatchlings seldom fail to find the ocean from wherever the nest may be located. There are occasional mass failures, but they are rare, and in themselves worthy of careful study.

After plodding through the soft dune sand the turtles reach the hard tidal flat. Here the light beacon appears to be supplemented by a local

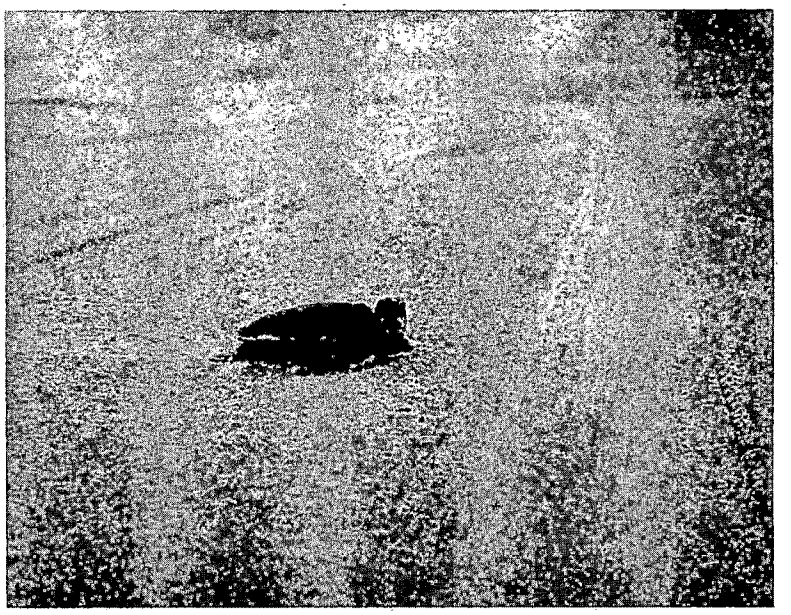


FIG. 10. Yearling green turtle half way across Daytona Beach in test trip to sea. Such pauses to peer about, as if in appraisal of guidance cues, are typical and frequent.

signal, probably the combination of hardness, flatness, and smoothness of the ground. In moving from horizontal to slightly inclined beach the turtles generally accelerate their pace noticeably. If a reverse change occurs they slow down. As Figures 8–11 suggest, however, no normal feature of beach topography completely disrupts the fundamental guidance response.

The next step comes when sand wet by the highest waves is reached. This brings on another surge of speed and "confidence" and some of the turtles may stumble over their flippers and break prematurely into short flurries of the flying strokes they use in swimming. The touch

of damp sand is evidently the cue that brings on the different gait that will be only of service when the water is reached. Then the farthest reach of a wave slides up the flat and lifts the foremost turtles in a quarter-inch of sheet-flow. The instant they are off the ground the flying swimstroke comes over the hatchlings in an explosion of energy. When the water slides out from under them they are caught with a new gait that is no good for walking, but as each wave wash comes back, they swim forward a little toward the surf.

This "learning" to swim, seems a clear case of releaser effect. It appears to require no practice period at all. It takes place in the tail

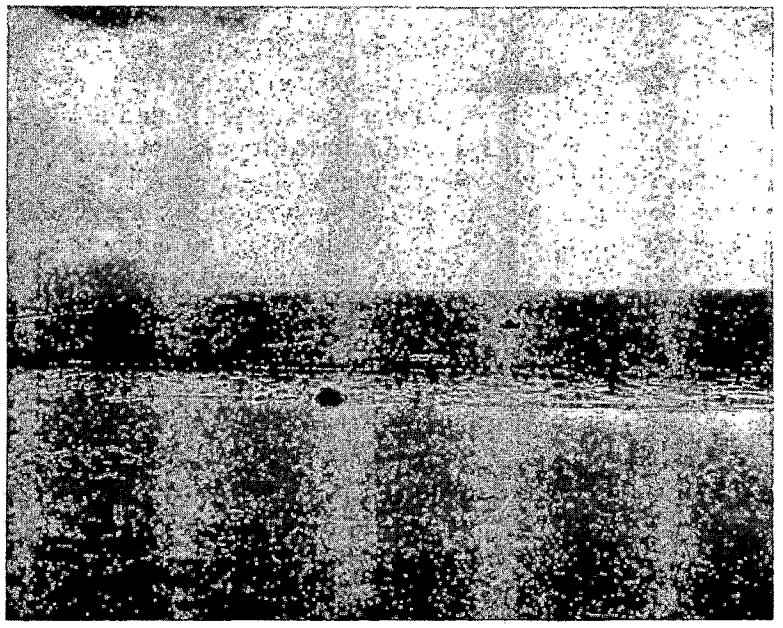


FIG. 11. Two yearling green turtles heading straight toward the sea, climbing a blind, landward slope after swimming across a tidal pool. Both in the pool and on the slope the view of the sea was hidden by the rise of the bar.

ends of a few waves and with it there comes a bipolar rheotaxis which causes the turtles to align themselves with the swash and backwash of the current, evidently told by occasional touches of the bottom that the water is in motion and that it goes first one way and then the other.

In any case they keep swimming straight seaward, dashing forward under water for a few feet, then emerging to breathe and look around, then going down and ahead again. When they reach the breakers one expects to see them picked up and thrown back through all of the way they have gone. But as the crest grows white the heads go down. The hatchlings presumably dive for the bottom and go under the plunge

of the breaker. This stage of the seaward journey is of course completely hidden but almost surely involves a variety of responses. In waves approaching shore, there appears to be a complex and variable relation between translation, plunge and backwash, and between these and the position of the step in the bottom. While it is not known what a turtle hatchling does inside a breaking wave there can be no doubt of the high survival stakes involved. There must be strong adaptive adjustments to wave dynamics. If the interplay of forces and responses could be monitored, it would not only help piece out our understanding of an extraordinary chain of orientation responses but might also shed light on the unaccountable absence of nesting colonies of sea turtles along thousands of miles of unused tropical beaches.

Thus, in going from the nest to the sea little turtles make a short, complicated journey toward a destination they have never seen, and use guidance information from a number of sources. Because it is almost surely the same with longer feats of oriented travel, spot assessment of cues seems unlikely to explain how any long course is established and held or where it leads. The ideal technique is radio tracking, or a continuous recording of the position of the migrant throughout entire round-trip journeys. That such an ideal can even be mentioned seriously is a sign of new times for natural history.

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MAGNETO-GASDYNAMICS: ITS SCIENCE AND TECHNOLOGY*

By ALI BULENT CAMBEL

DURING the last decade, a new science as well as technology has made itself felt, namely magnetohydrodynamics. Simply, magnetohydrodynamics comes about when an electromagnetic field is coupled with the flow field of an electrically conducting fluid such as mercury or an ionized gas. Originally developed by astrophysicists to study stellar phenomena, the subject is now claimed by engineers as well, because of its many potential technological applications. Among those we might mention the following: controlled thermonuclear fusion, magnetoaerodynamic drag for re-entry, thrust production for propulsive devices, pumping of liquid metals, high temperature resistant coating, cutting of high temperature alloys, and chemical synthesis. It is the purpose of this paper to review the concepts of magneto-gasdynamics as well as the underlying science which is necessary in bringing the applications closer to reality.

Depending on what medium is chosen, different names have been given to the subject. For example, the astrophysicists, who first observed the natural phenomena, labelled their study cosmical electrodynamics. Experiments with magnets and mercury yielded the name mercurydynamics. Physicists and electrical engineers have called it plasma physics or plasma dynamics; however, their descriptions do not necessarily imply the presence of an externally applied magnetic field. The word, magnetoaerodynamics, which implies experiments with air, has been used by aerodynamicists, while mechanical engineers have employed various descriptive terms depending on what turn their research took; for instance, the word magnetohydrodynamics describes the use of a magnetic field coupled with water or other liquids moving at a high rate of speed, while magneto-gasdynamics would imply that an electrically conducting gas or plasma is involved. Of course the subject finds application also in the case of very dense media or solids, and may be called magneto-elasticity. Probably the most general name would be magneto-fluidmechanics, and this has been suggested by Theodore von Kármán. In this present review, we shall emphasize primarily those applications in which a thermally ionized gas is subjected to the effects of a magnetic field. We shall mention also some analytical and experimental studies dealing with plasma physics and magneto-gasdynamics being pursued in our laboratory.

As a discipline, magneto-gasdynamics brings together a variety of

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classical disciplines such as classical thermodynamics, statistical mechanics, quantum mechanics, electromagnetism, and gasdynamics. We shall not go into their individual details here, because they are well known to the reader of this journal. However, we shall review briefly the highlights of the formulation of our unifying discipline. For a detailed treatment we refer the reader to an introductory treatise [1] by the present author. A brief review of the historical evolution of the subject has been reviewed in [2].

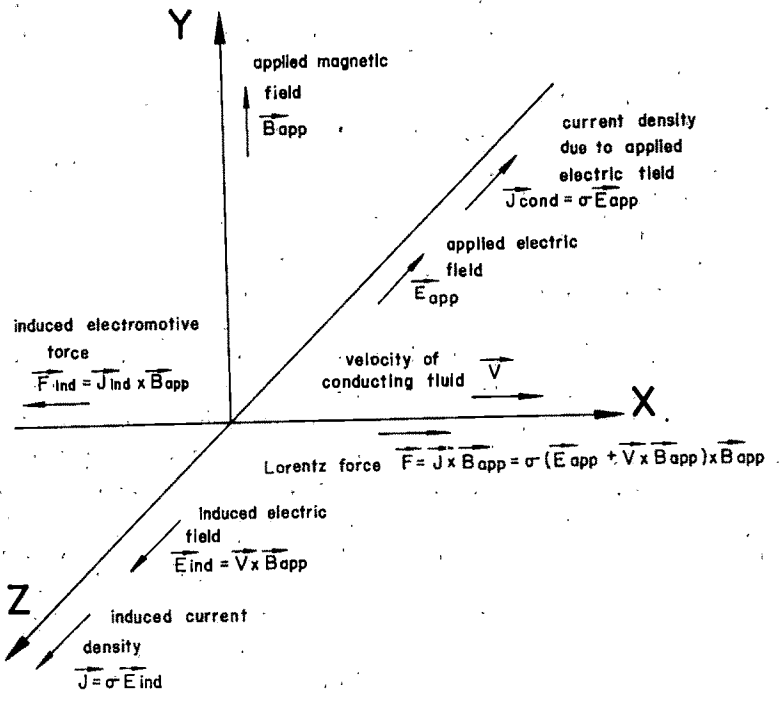


FIG. 1. Vector diagram of magneto-fluidmechanics.

In Figure 1 we see the coordinate system underlying magneto-fluidmechanic phenomena. In describing Figure 1, we imagine the steady flow of an electrically conducting fluid which travels with a global velocity **V**. At right angles to this we apply a magnetic field, the field strength of which is represented by the vector $\mathbf{B}_{app}$. Due to the interaction of the two fields an electric field denoted by $\mathbf{E}_{ind}$ at right angles to both **V** and $\mathbf{B}_{app}$ is induced. This electric field is given by the following equation:

$$\mathbf{E}_{ind} = \mathbf{V} \times \mathbf{B}_{app}$$

If we assume that the plasma is and remains isotropic in spite of the magnetic field, we can denote its electrical conductivity by the scalar

quantity σ. Then by Ohm's law, the density of the current induced in the plasma is

$$\mathbf{J}_{\text{ind}} = \sigma \mathbf{E}_{\text{ind}}$$

Simultaneously, occurring with the induced current is the induced ponderomotive force $\mathbf{F}_{\text{ind}}$ which is given by the following vector product:

$$\mathbf{F}_{\text{ind}} = \mathbf{J}_{\text{ind}} \times \mathbf{B}_{\text{app}}$$

This force occurs because as in an electric generator, the plasma cuts the lines of the magnetic field. Because this vector product yields a vector perpendicular to both $\mathbf{J}_{\text{ind}}$ and $\mathbf{B}_{\text{app}}$, the induced force is parallel to $\mathbf{V}$ but opposite in direction.

To make the configuration slightly more general, we now apply also an electric field $\mathbf{E}_{\text{app}}$, at right angles to both $\mathbf{B}_{\text{app}}$ and $\mathbf{V}$ but opposite in direction to $\mathbf{J}_{\text{ind}}$. The current density due to this applied electric field we denote by $\mathbf{J}_{\text{cond}}$ and call it the conduction current. The net current density $\mathbf{J}$ through the plasma and the one that we would be measuring with a suitably placed ammeter is then,

$$\mathbf{J} = \sigma(\mathbf{E}_{\text{app}} + \mathbf{V} \times \mathbf{B}_{\text{app}})$$

The ponderomotive or Lorentz force associated with this current is then

$$\mathbf{F} = \mathbf{J} \times \mathbf{B}_{\text{app}} = \sigma(\mathbf{E}_{\text{app}} + \mathbf{V} \times \mathbf{B}_{\text{app}}) \times \mathbf{B}_{\text{app}}$$

If $\mathbf{E}_{\text{app}} > \mathbf{V} \times \mathbf{B}_{\text{app}}$, we encounter an accelerator which may be used as a thrust-producing device.

Although generating plasmas requires very high temperatures which are generally difficult to produce and to contain, it is paradoxically this characteristic which makes plasma and/or magneto-gasdynamic engines so desirable technologically. The explanation is quite simple. Thus, the Carnot efficiency of an engine cycle increases with an increase in the range of the operating temperatures. The space technologist is interested in such engines for a second reason: In outer space there is no continuum air and thus disposing of the unavailable energy which any engine must reject in order to comply with the second law of thermodynamics poses a problem. However, the high exhaust temperatures make possible the removal of heat by radiation which is possible in a void. Finally, the specific impulse which such an engine might yield is considerably higher than that which chemical reaction motors can be expected to produce.

Before magneto-gasdynamic devices can become practical technologically, several problems will have to be solved. These are: (*a*) the availability of high temperature resistant materials; (*b*) the development of compact electric power supplies; (*c*) the perfection of compact electromagnets; (*d*) a better understanding concerning the thermophysical properties of plasmas in general, as well as the effects of electromagnetic

fields on the behavior of plasma particles; (*e*) better understanding of how electromagnetic fields influence the gasdynamics of ionized gases on a global or macroscopic level.

It is the purpose of this review to address itself primarily to the last two points, namely the chemistry, the physics and the dynamics of plasmas.

Plasma Physical Chemistry I—Kinetics

Even in the absence of fluidmechanic motion with or without applied electromagnetic fields, the study of plasmas constitutes a most fertile area for research. The scientist concerned with the physical-chemistry of plasmas will find challenging areas of study in equilibrium and non-equilibrium thermodynamics as well as in reaction kinetics.

Turning our attention to the kinetics first, let us describe in most simple terms the sequence of events which might take place in the ionization of a diatomic gas. We know that the molecules of such a gas may assume a variety of degrees of freedom and that with each degree of freedom there is associated an energy. Now let us consider air at relatively low temperatures and let us treat it as a mixture of nitrogen and oxygen. It is customary to represent such a diatomic gas by a "dumbbell" or "rigid rotator." At extremely low temperatures, namely in the neighborhood of absolute zero, there will be three translational degrees of freedom, namely in the x-, y-, and z-directions. At temperatures in the neighborhood of 10°K., the rotational degrees of freedom are excited, although the molecular bond remains quite stiff. As the temperature is raised further, the molecular collisions increase, and eventually the vibrational degree of freedom comes into play. This generally occurs around 1000°K. When the impacts become so intense that the molecular bond is stretched to the breaking point, dissociation results. At atmospheric pressure, oxygen starts to dissociate at about 3000°K., whereas nitrogen starts to dissociate at about 4500°K. At still higher temperatures, the electrons in the orbits around the atomic nucleus become excited to quantum states above the ground state. This excitation too contributes to the total energy, but the effect is negligible up to about 5000°K. Finally, at still higher temperatures, electrons leave their orbits and the gas becomes ionized. For example, at about 10,000°K. and at atmospheric pressure the ionization of the oxygen and the nitrogen in the air becomes sufficiently significant to be considered in engineering applications.

In the preceding discussion the various levels of excitation were explained in a consecutive order. Actually this is not necessarily the case and in some applications different levels of excitation may occur in the aforementioned sequence as well as concurrently. This may be explained because, during collisions, higher energy particles such as ions and

electrons impart their energy to the slower particles such as molecules and thereby excite them, so to speak, prematurely.

In the discussion so far, another simplification was made, namely that one deals with a single component diatomic gas *per se*, such as oxygen alone or nitrogen alone. Actually, in the case of air which is a mixture of many components, multifarious reactions must be taken into consideration. Although considerable research remains to be done in this connection, it has been suggested by Bortner [3] that the ionization of air proceeds in accordance with a number of shuffle reactions which may be summarized as follows:

Before ionization in air occurs, a number of dissociation reactions are important, namely,

$$O_2 + X \rightleftarrows O + O + X$$
$$O + N_2 \rightleftarrows N + NO$$
$$O + NO \rightleftarrows N + O_2$$
$$N_2 + X \rightleftarrows N + N + X$$

These reactions lead to an early ionization reaction, namely,

$$N + O + 2.8 \text{ e.v.} \rightarrow NO^+ + e$$

Subsequent ionization reactions are believed to be the following:

$$N + N + 5.8 \text{ ev} \rightarrow N_2^+ + e$$
$$O + O + 7.4 \text{ ev} \rightarrow O_2^+ + e$$
$$NO + X + 9.5 \text{ ev} \rightarrow NO^+ + e + X$$
$$O_2 + X + 12.3 \text{ ev} \rightarrow O_2^+ + e + X$$
$$O + X + 13.6 \text{ ev} \rightarrow O^+ + e + X$$
$$N_2 + X + 15.5 \text{ ev} \rightarrow N_2^+ + e + X$$
$$N + X + 14.5 \text{ ev} \rightarrow N^+ + e + X$$
$$\tfrac{1}{2} N_2 + \tfrac{1}{2} O_2 + 10.3 \text{ ev} \rightarrow NO^+ + e$$

It has been suggested that the mechanism controlling ionization in air is the forward-going oxygen dissociation reaction. It is because of this sequence of events that the relaxation time of the oxygen dissociation governs the ionization processes in air undergoing hypersonic flow.

From the aforementioned it is evident that we should treat the plasma as a multicomponent mixture. We might do this in many ways. For example, we note that we have different chemical components in air plasma, such as molecular oxygen and nitrogen, atomic oxygen and nitrogen, different oxides, ions and electrons. Of course, conducting a chemical analysis to specify the amount of each component is presently a tremendous task. However, even if we consider a chemically simple component gas such as hydrogen, for example, we must still treat the plasma as a multicomponent mixture. We come to this realization because experimental observations indicate that, when a plasma is accelerated electromagnetically, the difference in the behavior of the ions and the electrons is very significant. Thus, it has been noted that there is a strong coupling between the behavior of the electrons and the magnetic

field. On the other hand, the coupling between ions and the magnetic field as well as the coupling between ions and electrons are relatively weak. Therefore, it may be advisable to treat the plasma as a binary mixture of electron gas and ion gas. This calls for a readjustment in our thinking because the existing theory of magneto-gasdynamics is based primarily on the assumption of a single component gas, whereas in many engineering applications we have to reckon with multicomponent mixtures of a variety of gases making up air for example.

In our present discussion we have restricted ourselves to thermal ionization processes because, in energy conversion devices, these are the



FIG. 2. Laboratory simulation of plasma shock.

most important. Actually, ionization can occur by many other processes such as those due to radiation effects, high frequency fields, by electron attachment, etc. It is outside the scope of this paper to describe all of these, but the interested reader may find somewhat more detailed information in [4].

Another aspect of reaction kinetics of plasmas is interesting, namely the de-ionization and neutralization reactions. The importance of this may be demonstrated by a very practical application. Let us consider the bow shock which may develop in front of a hypersonic re-entry vehicle. In the laboratory, simulation of this case depicted in Figure 2 and reproduced here from [5] by Ziemer, we see the luminous shock

front due to the conversion of kinetic energy into thermal energy. As the ionized gas expands over the vehicle, it will experience a lowering of its thermal energy level and consequently will tend to become de-ionized thus liberating energy. Of course, the designer is concerned with the integrity of the vehicle structure and consequently must know the mechanism and rate of the neutralization process. Such neutralization or recombination processes too may take place in a variety of ways [4], but it is probably fair to suggest that we know even less about recombination kinetics than we do about the kinetics of ionization.

It is evident from the aforementioned that the researcher studying plasma reaction kinetics has his work cut out for him. Not only should he be familiar with the reactions which occur, but he should know also the reaction rate coefficients. Neither is easily measurable in the laboratory. However, the situation is further complicated because, in engineering applications, the plasma reactions will be taking place adjacent to some sort of a surface and therefore surface phenomena will have an important bearing. If, in addition, we consider the effect of the magnetic field, it is easy to surmise that the particle behavior and the reaction kinetics will be altered further.

The foregoing cursory survey indicates that the present scientific knowledge is not sufficiently complete to make the transition from basic scientific research to basic technological research. Our observation might suggest that we are returning to the type of era which might have existed in the days of Maxwell, Poincaré, or Minkowski, where experimental observations and technological glimmers of hope stimulated the formulation of new theories. We shall not debate whether such theories should be developed by the scientist or the engineer. The source is immaterial, although the need is clearly evident. However, we have no doubt that the challenge will be met and that, over the years to come, the presently blurred dichotomy between scientists and engineers will disappear completely.

Plasma Physical Chemistry II—Thermodynamics

In solving any fluidmechanical problem, it is necessary to satisfy the conservation equations for mass, momentum, and energy. In doing so, it is necessary also to know the properties of the fluid. Thus, before attempting to embark on a conclusive solution of magneto-gasdynamic problems, one must have available the pertinent equations of state from which the thermodynamics of the medium can be evaluated.

Developing equations of state which are valid for all ranges of temperature and density is an almost impossible task not only for ionized gases, but even for neutral gases. Therefore, the discussion here constitutes a relatively naïve approach, in which the plasma is approximated

to act more or less like a perfect gas. The discussion is aimed at conditions which may be expected in engineering applications. Thus, in general, it is assumed that the plasma, although electrically conducting, is electrically neutral and is also in thermal equilibrium. The discussion should be valid for electron densities up to 10^{16} per cm^3. We assume a hypothetical perfect gas plasma. It can then be shown [4] that the thermal equation of state for a singly ionized perfect gas plasma is

$$p = (1 + \alpha)\rho RT$$

where p denotes the pressure, α the degree of ionization, ρ the density, R the gas constant and T the temperature. Similarly, for a plasma which is singly as well as doubly ionized, the idealized thermal equation of state is

$$p = (1 + \alpha_1 + 2\alpha_2)\rho RT$$

where α_1 and α_2 denote the first and second degree fractions of ionization. The internal energy U for the case of simultaneous single and double ionization may be shown to be

$$U = \frac{3}{2}kT(1 + \alpha_1 + 2\alpha_2)n_H + \alpha_1 E_1 n_H + \alpha_2 E_2 n_H + (1 - \alpha_1 - \alpha_2)n_H E_0^{el} + \alpha_1 n_H E_1^{el} + \alpha_2 n_H E_2^{el}$$

where the Boltzmann constant is denoted by k, while n_H denotes the number of heavy particles in the plasma. In turn the E's denote the various ionization energies.

Because the extent of ionization is a crucial factor in defining the thermophysical properties of plasmas, we now seek to determine the per cent or fraction of ionization of a plasma consisting of neutral and electrically charged particles. Let us consider the general ionization reaction

$$A_i \rightleftarrows A_{i+1} + e - E_i \qquad (i = 0,1,2,\ldots)$$

where A_i represents the ith level ionized particle and E_i is the energy required to bring about ionization from the ith to the $(i + 1)$ level. The equilibrium constant K_{i+1} for this reaction is then

$$K_{i+1} = \frac{n_{i+1}n_e}{n_i} = \frac{Z_{i+1}Z_e}{z_i}\, e^{-E_i/kT}$$

If we then make the usual statistical thermodynamic substitutions, we find the following general Saha equation

$$K_{i+1} = V\left(\frac{2\pi m_e kT}{h^2}\right)^{3/2} \frac{2Z_{i+1}^{el}}{Z_i^{el}}\, e^{-E_i/kT}$$

where V is the volume and m_e is the electronic mass. Furthermore, h denotes Planck's constant and the Z's represent the electronic partition functions. Because the equilibrium constant can be defined in terms of

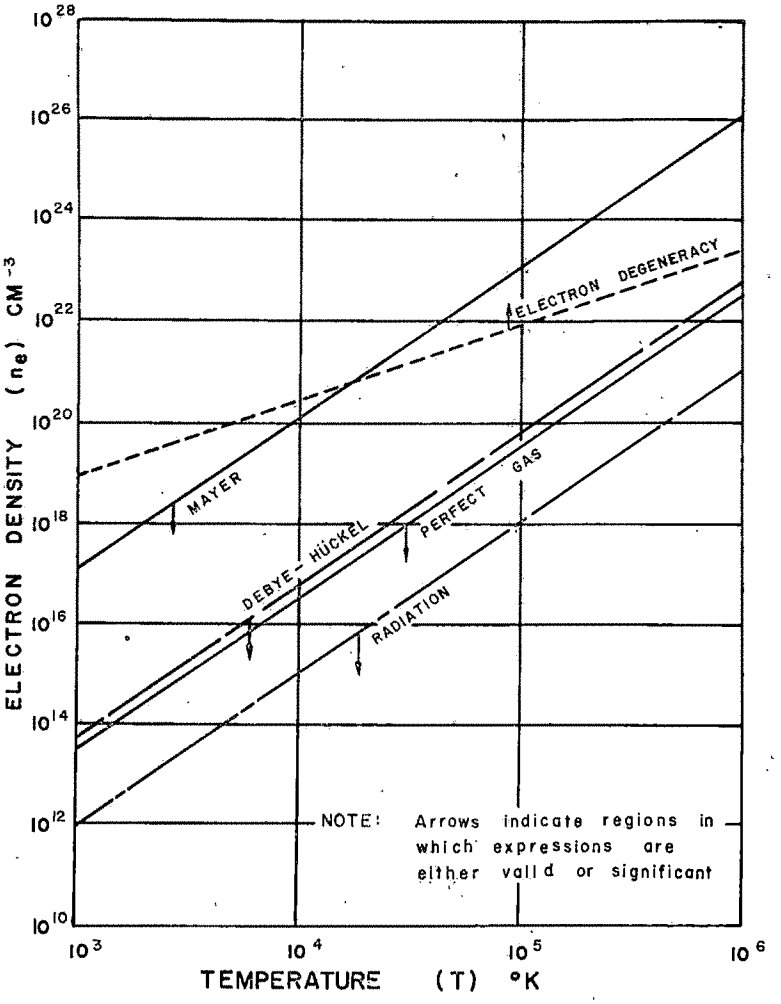


FIG. 3. Regions of applicability of equations of state for single ionization.

the fraction ionized, one can express conveniently the amount of ionization in any particular gas in terms of pressure and temperature alone. For example, for the case of single ionization, Saha's equation is

$$\frac{\alpha_1^2}{1-\alpha_1^2} = \left(\frac{2\pi M_e}{h^2}\right)^{3/2} \frac{(kT)^{5/2}}{p} \frac{2Z_1^{el}}{Z_0^{el}} e^{-E_i/kT}$$

It is evident from Saha's equation that the amount of ionization increases with increasing temperature and decreasing pressure. Because high temperatures are not easily come by, most plasma dynamic experiments are conducted at low pressures. Nevertheless, we must recognize that, technologically, high pressure plasmas have their attractive aspects.

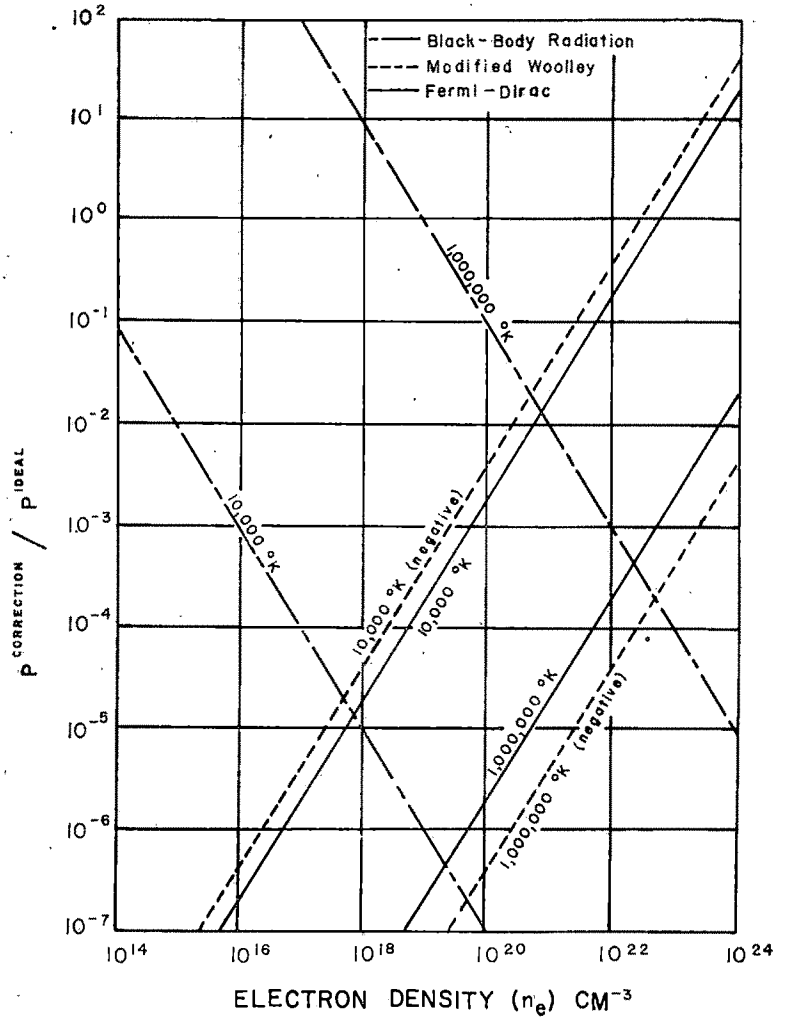


FIG. 4. Quantum pressure corrections.

As a word of caution, it should be noted that Saha's equation is applicable to homogeneous systems in thermal equilibrium, and therefore, in actual ionization processes it serves only as an approximation. However, in numerous applications equilibrium may not be taken for granted. For example, the region of ionized gas immediately behind strong shock waves may not be in equilibrium due to finite relaxation time. Deviations from equilibrium also exist in plasma arc jets and electromagnetic shock tubes so frequently used in research laboratories. Surfaces too affect the thermodynamics of the system as does turbulence and diffusion. All such effects tend to render less valid the Saha equation.

As a final comment concerning the Saha equation, we might add that, although we presented the case of single ionization only, appropriate Saha equations may be derived for higher degrees of ionization.

It should not be inferred that the simplified thermodynamics which we have presented here is conclusive. Indeed, a number of more sophisticated equations of state have been suggested. Unfortunately, it is outside the scope of the present review to discuss the numerous subtle points which should be taken into consideration. However, for the convenience of the reader we summarize in Figure 3 from [6] the regions of applicability of some equations of state. Similarly, in Figure 4 we show the quantum pressure corrections.

The calculation of the thermodynamic properties is very lengthy and may require the utilization of high speed electronic computers. To circumvent this, approximate equations of state suggest themselves. We are in the process of developing such equations.

Plasma Physical Chemistry III—Transport Properties

In magneto-gasdynamics, as in other flow problems, it is of urgent interest to be cognizant of the transport properties of the plasma. In general we define transport phenomena as rate processes in which deviations from equilibrium are quite slight. In such phenomena we are dealing with systems which are not in thermodynamic equilibrium but which nevertheless tend to move towards a state of thermodynamic equilibrium in accordance with the Boltzmann H-theorem. Indeed the trend towards equilibrium occurs by virtue of the transport mechanisms whereby mass, momentum, and energy are transported as a result of the motion and the collisions experienced by particles which carry with them the property being transported. We shall not enter into a discussion of radiative transport here because, in the phenomena with which we are particularly interested in this laboratory, the plasmas are optically transparent and the radiative phenomena are negligible compared with the other effects. However, it should not be inferred that radiative transport is unimportant generally. Quite the contrary is true.

The analysis of transport phenomena in gases may be conducted along either of two avenues of approach. The first, and indisputably the rigorous approach is to solve the Boltzmann equation. The second approach is to construct a physical model on the atomic level and to observe mean values of the free paths and velocities. Although less accurate than the analysis based on Boltzmann's equation, the phenomenological approach yields results which are acceptable in many engineering applications.

In classical gasdynamics it is customary to treat the gas as a continuum consisting of particles which do not exert any forces on one another. Because plasmas contain electrically charged particles, this simplification

is not a good one, and one must consider the forces between particles. Unfortunately, there does not exist at this time a generally valid and rigorous mathematical treatment. The present state of the art has been presented by Chapman [7]. Therefore, it is customary to deal with a variety of semiempirical equations which define the intermolecular potential functions.

As a matter of convenience we differentiate between two classes of intermolecular forces, namely, short range forces and long range forces. The short range forces come into play when the particles are so close to one another that their electron clouds overlap. Short range forces are found to be repulsive. When the distance between particles is sufficiently large, the forces are attractive, and we speak of long range forces. It is found that electrostatic, inductive, and dispersion effects are associated with long range forces. Although in principle the force between particles may be calculated simply if the potential function describing the potential energy of interaction can be defined, in actuality the question remains unanswered.

TABLE I DOMINANT COLLISIONS

Transport Process	Controlling Modes of Collisions
Electrical conduction	Electron—neutral
	Electron—ion
Thermal conduction	Electron—electron
	Electron—neutral
	Ion—neutral
	Electron—ion
Viscosity	Neutral—neutral
	Ion—ion
	Ion—neutral
Diffusion	Electron-ion
	Ion—neutral

Transport phenomena occur by virtue of random collisions among carriers which make up the system. The carriers which may be found in plasmas are molecules or atoms which we call neutrals, and ions and electrons. In slightly ionized plasmas, there are relatively few charged particles and thus the motion of the neutrals plays the dominant role in contributing to transport. However, if the level of ionization is relatively high, one must consider also the collisions among the charged particles as well as collisions among neutrals and charged particles. In Table I are summarized the dominant collisions in various types of transport phenomena.

In discussing collision phenomena, we differentiate between two types, namely, elastic collisions and inelastic collisions. Elastic collisions are those in which neither of the two particles involved in the

collision change their state of internal energy. However, a particle involved in an elastic collision may acquire a higher velocity. In an inelastic collision a particle does experience a change of its internal energy from a ground level to another. Any increase in the internal energy occurs at the expense of kinetic energy. Collisions leading to excitation or ionization are inelastic. In speaking of collisions, it should not be inferred that there must occur a collision in the literal sense. Indeed, because electrostatic forces between charged particles are long ranged, particles may be influenced by one another without coming into actual contact. It is hypothesized that, when two charged particles pass one another in a distant encounter, each particle will move along a path which appears as a hyperbola relative to the center of mass of the two particles. A more rigorous description has been presented by Spitzer [8].

The transport properties of particular interest to the plasma physicist are the electrical conductivity, the thermal conductivity, the viscosity and the diffusivity. Among these, the electrical conductivity has probably received the greatest amount of attention, and we shall review briefly the state of its science. The kinetic theory of ionized gases goes back to Boltzmann, Maxwell, and Lorentz, but the impetus to real gas theory was imparted by Chapman and Cowling in their now so well-known monumental book [9] on non-uniform gases. In this classical volume may be found the solutions of the Maxwell-Boltzmann distribution which Sydney Chapman and David Enskog had analyzed independently and in different detail in 1916–1917. Although Chapman and Cowling made profound contributions, their assumption of binary collisions is not correct in the strictest sense for ionized gases. One of the best known papers in which the assumption of binary collisions alone was not made is that by Spitzer and Härm, who derived relatively simple expressions for the electrical conductivity and the thermal conductivity of a fully ionized gas. They assumed that the collision term on the right side of Boltzmann's equation is the sum of two non-coupled effects, one accounting for the close encounters and the other for the distant encounters. They then cut off the close encounter effects at a so-called critical impact parameter. Their relation when all appropriate constants are evaluated numerically is as follows:

$$\sigma = \frac{1.56 \times 10^{-4} \times T^{3/2}}{\ln\left(\frac{1.23 \times 10^{4} \times T^{3/2}}{n_e^{1/2}}\right)} \text{ mho cm}^{-1}$$

If one compares, as in Figure 5, the predictions of the above equation with the experimental data of Kantrowitz and his co-workers, [10], it is found that there may occur a discrepancy of ±15 to 30 per cent. In the present state of the art and science of magneto-gasdynamics, this is excellent verification.

In the previous section we discussed the case of fully ionized gases. The relations for fully ionized gases cannot be adjusted simply for slight ionization, because the transport mechanism is different. Chapman

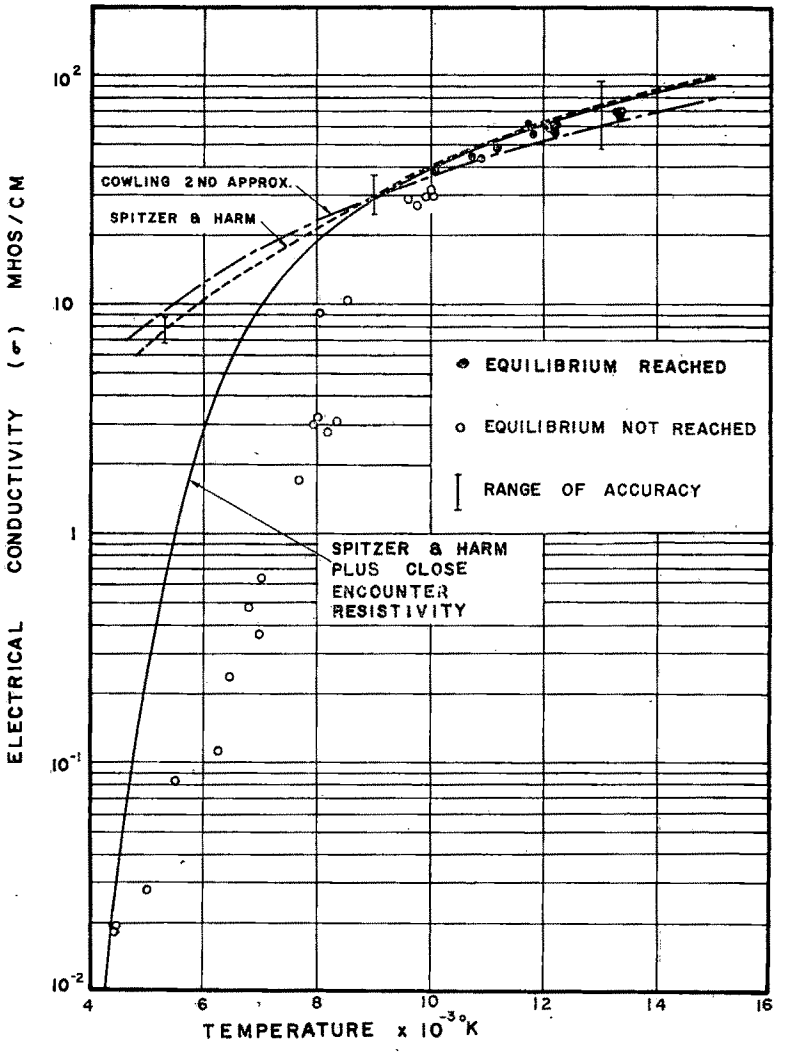


FIG. 5. Comparison of theoretical predictions for electrical conductivity with experimental data.

and Cowling developed a convenient theory on the assumption that the gas may be treated substantially like a neutral one. They showed that the distribution function in the Boltzmann equation is a function of the diffusion coefficient and that there is therefore a coupling between charge

and mass diffusion. The expression for the electrical conductivity of a slightly ionized gas when the various constants are evaluated is as follows:

$$\sigma = 3.9 \times 10^{-12} \frac{\alpha}{QT^{1/2}} \text{ mho cm}^{-1}$$

where Q is the collision cross section.

In many engineering problems we encounter plasmas which, although not fully, are strongly ionized. To a first approximation, the electrical conductivity of such gases may be found by recalling that, when an electron moves through a plasma system, it is influenced by two types of retarding forces which are due to short range collisions and to long range collisions. Kantrowitz and his associates suggested that because the retarding forces act like resistances, and hence inhibit the flow of electric current, they should be treated like resistances in series. Because the resistivity is substantially the reciprocal of the conductivity, we may then write

$$\frac{1}{\sigma} = \frac{1}{\sigma_L} + \frac{1}{\sigma_H}$$

where σ is any arbitrary conductivity, σ_L is the conductivity of the slightly ionized gas as computed from the Chapman-Cowling theory, and σ_H is the conductivity of the fully ionized gas calculated from Spitzer's formulation. Depending on the peculiarities of the case under consideration, $\sigma_L \gtrless \sigma_H$. Therefore, when $\sigma_L \ll \sigma_H$, $\sigma \cong \sigma_L$ while when $\sigma_H \ll \sigma_L$, $\sigma \cong \sigma_H$.

The second approach of determining the conductivity for arbitrary ionization is the free path theory. In this one does not concern himself with the forces but rather considers the mean time τ elapsed between close encounters. The expression for the conductivity thus found is

$$\sigma = \frac{ke^2n^2}{m_e}\tau$$

where the mean time τ, is defined as the ratio of the mean free path to the mean velocity, namely,

$$\tau = \frac{(\pi m_e)^{1/2}}{(8kT)^{1/2}\sum_i n_i Q_i}$$

To assure the successful design of engineering devices useful in power generation and flow control, it is necessary to assure a satisfactory coupling between the flow field of the conducting fluid and the electromagnetic field. This requires a reasonably high electrical conductivity. Of course, the electrical conductivity can be increased by using very high temperatures, but this would have deleterious effects on the life span of the device being considered. However, the effective electrical

conductivity may be enhanced so to speak artificially, by injecting into the plasma additives which ionize at relatively low temperatures. This technique is called seeding. The primary requirement for a good seeding

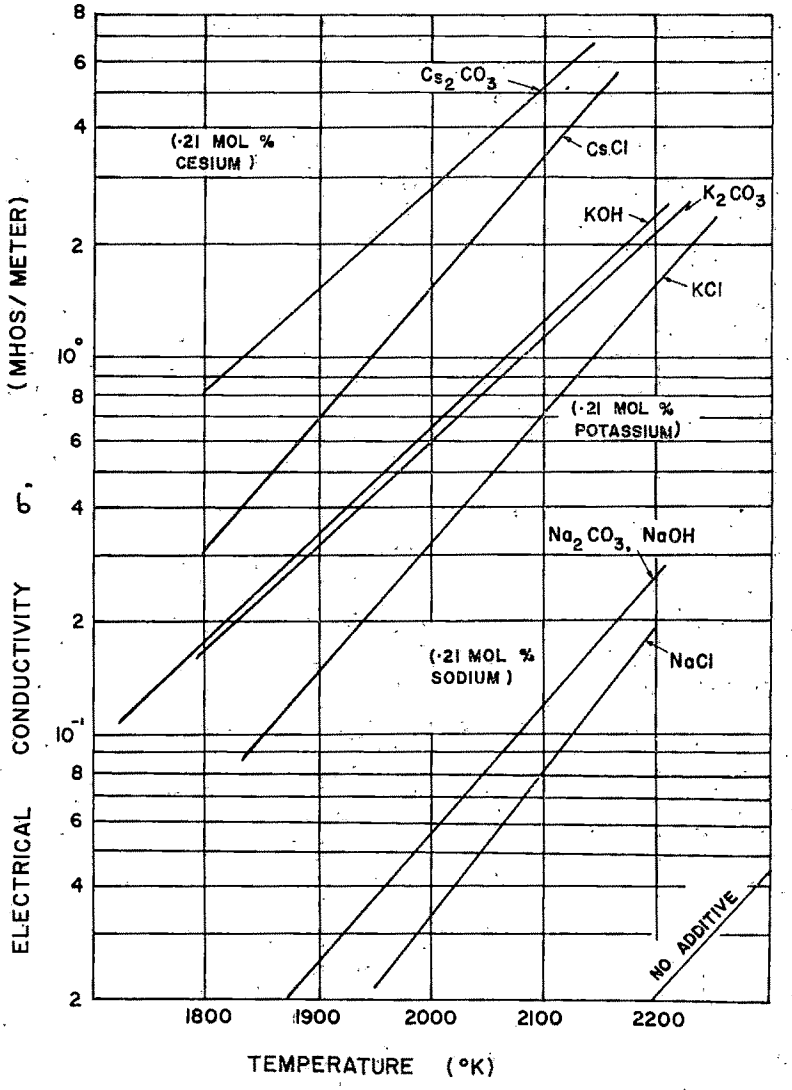


FIG. 6. Electrical conductivity of seeded propane-air flames.

material is that it is ionized easily, i.e., the seed should have a low ionization potential. Generally alkali metals and alkaline earths have relatively low ionization potentials. In Figure 6, from [11], may be seen the electrical conductivity of propane-air flames seeded with potassium

chloride spray. Our measurements indicated that compounds of potassium which are very cheap are almost as effective as compounds of cesium, which are quite expensive.

If a magnetic field is imposed on the plasma and the plasma becomes anisotropic, the conductivity must be treated as a tensor and Ohm's law must be modified suitably. The interaction of the various fields will result in a variety of modes of charge transport and a number of currents will thus arise with each of which there will be associated an individual conductivity. For the successful operation of any magnetohydrodynamic devices which we may wish to design, it is necessary that we know how to account for the various conductivities.

First we recognize again the scalar conductivity σ which is attributed to the transfer of charges due to the imposed electric field. This conductivity is the one which exists in the absence of a magnetic field or when the applied magnetic field is parallel to the electric field. The scalar conductivity is the only conductivity which is a property of the fluid and it has of course no directions.

The second type of electrical conductivity of interest is the so-called Hall conductivity, already well known in solids. Indeed, in ionized gases the Hall conductivity σ_H may be much more important than in solids. The Hall effect is associated with charge transfer caused by the electromagnetic field, and it can be attributed to the difference between the charges of the ions and the electrons.

The third conductivity is the transverse conductivity σ_T. It is due to the charge transfer when the electric and magnetic fields are perpendicular to one another. The transverse conductivity σ_T is defined

$$\sigma_T = \frac{\sigma}{1 + (\omega_c \tau)^2}$$

where ω_c is the cyclotron frequency.

The tensor conductivity $\bar{\bar{\sigma}}$ for an anisotropic plasma incorporates the various directional components already described. It is defined as follows:

$$\bar{\bar{\sigma}} = \begin{bmatrix} \sigma_T & -\sigma_H & 0 \\ \sigma_H & \sigma_T & 0 \\ 0 & 0 & \sigma \end{bmatrix}$$

Clearly the effect of the magnetic field on the electrical conductivity is to reduce it. Measurements in this laboratory [11] wherein propane-air flames seeded with various compounds and subjected to a transverse magnetic field of 4.1 kilogauss indicated that the transverse conductivity of propane-air flames is about 75 per cent of the scalar conductivity. We see this in Figure 6. Although much more experimental research remains to be conducted, indications are that the reduction in conductivity may not be as severe as theory would predict.

The scope of this paper does not permit a study of the other transport properties, but we hope that the present discussion will serve as an in-

troduction to the subject.

The Conservation Equations

In order to solve a problem in classical gasdynamics, it is necessary to satisfy the conservation equations and to specify the appropriate equations of state. The same approach is necessary in magneto-gasdynamics, but some additional factors must be taken into consideration. First, we must realize that in many magneto-gasdynamic problems the composition of the plasma does not remain constant because ionization and de-ionization processes are likely to occur. Therefore, it is necessary to satisfy not only the global continuity equation, but also the species continuity equation. Second, the magneto-gasdynamic momentum equation must incorporate the magnetic pressure. Taking into consideration a variable body force may be a new experience to the classical gasdynamicist who normally neglects body forces. Third, the energy equation in magneto-gasdynamics should include the magnetic energy term and it should also take into account the radiation which may be associated with the high plasma temperatures.

First of all we wish to review briefly the electromagnetic equations which play such an important part in magneto-gasdynamics.

The well-known electromagnetic field equations were deduced by Maxwell from the experimental laws of Ampère and Faraday. In mks units these are

$$\nabla \times \mathbf{H} = \mathbf{J} + \frac{\partial \mathbf{D}}{\partial t}$$

$$\nabla \times \mathbf{E} + \frac{\partial \mathbf{B}}{\partial t} = 0$$

$$\nabla \cdot \mathbf{B} = 0$$

$$\nabla \cdot \mathbf{D} = \rho_e$$

In free space the vectors **H**, **B**, **E**, and **D** are related by the constitutive equations, namely $\mathbf{B} = \mu\mathbf{H}$ and $\mathbf{D} = \epsilon\mathbf{E}$.

For a stationary Ohm's law is written as follows:

$$\mathbf{J} = \sigma(\mathbf{E} + \mathbf{V} \times \mathbf{B}) + \rho_e \mathbf{V}$$

where ρ_e is the excess charge density. If transport phenomena are taken into consideration, a generalized Ohm's law may be written as follows:

$$n_e\mathbf{E} + (1 - g\beta)\nabla P_e = n_e\left(\sigma_{ei} + \frac{\mathbf{B}}{\sigma_{in}}\right)\mathbf{J} + (1 - 2g\beta)\mathbf{J} \times \mathbf{B} + \frac{g^2}{n_e\left(\frac{1}{\sigma_{en}} + \frac{1}{\sigma_{in}}\right)}[\nabla P_e \times \mathbf{B} - (\mathbf{J} \times \mathbf{B}) \times \mathbf{B}]$$

The continuity of charge equation is

$$\frac{\partial \rho_e}{\partial t} + \nabla \cdot \mathbf{J} = 0$$

The ponderomotive force for a plasma is defined

$$\mathbf{F}_{em} = \rho_e \mathbf{E} + \mathbf{J} \times \mathbf{B}$$

The electromagnetic energy u_{em} is defined

$$u_{em} = (\rho_e \mathbf{E} + \mathbf{J}_c \times \mathbf{B}) \cdot \mathbf{V}$$

The Joule heating is

$$Q_J = (\mathbf{E} + \mathbf{V} \times \mathbf{B}) \cdot \mathbf{J}_c$$

The fundamental equations of magneto-gasdynamics may be derived in either of two ways. Thus, one may combine Boltzmann's equation with the electromagnetic equations or one may combine the convected conservation equation with the electromagnetic equations. For a purely macroscopic formulation the latter approach is sufficient and quite convenient. However, for a microscopic formulation, it is necessary to start with Boltzmann's equation from which one arrives at the species conservation equations. These when summed over all species further yield the macroscopic equations.

The Boltzmann equation is well-known and hence will not be repeated here. However, because the convected equation is less widely found in the literature, we wish to recall it. Thus,

$$\frac{D}{Dt} \int_v f dv = \int_v \left[\frac{\partial f}{\partial t} + \nabla \cdot (f \mathbf{V}) \right] dv$$

In this equation v represents an arbitrary control volume, while f is some function $f(x,y,z,t)$ which has continuous derivatives and may be any pertinent variable such as momentum for example.

If we incorporate into either the Boltzmann or the convected equation the electromagnetic equations, we arrive at the following conservation equations:

Species Continuity Equation:

$$\frac{\partial n_i}{\partial t} + \nabla \cdot n_i \mathbf{V}_i = \dot{w}_i$$

where n_i is the number of particles per unit volume, $\mathbf{v}_i$ is the velocity of the particles and $\dot{w}_i$ is the reaction rate coefficient. The subscript i refers to the ith species. It is evident that for a multicomponent plasma undergoing chemical reactions, we must write a species continuity equation for each species i. It should be remembered that there are very little data about $\dot{w}_i$.

Global Continuity Equation: The global continuity equation may be found by summing the species continuity equation over all species.

Thus one obtains,

$$\frac{\partial \rho}{\partial t} + \nabla \cdot (\rho \mathbf{V}) = 0$$

Global Momentum Equation:

$$\rho \frac{D\mathbf{V}}{Dt} = -\nabla p + \nabla(n \nabla \cdot \mathbf{V}) + \frac{1}{3} \eta \nabla(\nabla \cdot \mathbf{V}) + \rho_e \mathbf{E} + \mathbf{J} \times \mathbf{B} + \rho \mathbf{F}_g$$

Global Energy Equation:

$$\rho \frac{D(c_p T)}{Dt} = \nabla \cdot (k \nabla T) + \nabla \cdot Q_{\text{Rad}} + \frac{J_e^2}{\sigma} + \frac{DP}{Dt} + \Phi$$

where Φ is the so-called dissipation function.

It is evident that while the species and global continuity equations of magneto-gasdynamics are the same as in classical gasdynamics, the momentum and energy equations must include terms to account for the electromagnetic phenomena and the high temperature radiation effects.

In addition to the aforementioned three equations it will be convenient to have an equation which defines the interaction between the flow field and the electromagnetic field. Such a relation can be obtained quite simply by combining the global momentum equation with Ohm's law, thus

$$\frac{\partial \mathbf{H}}{\partial t} = \quad \times (\mathbf{V} \times \mathbf{H}) + \nu_m \nabla^2 \mathbf{H}^2$$

where ν_m is the magnetic diffusivity, which is defined $\nu_m = (\mu\sigma)^{-1}$.

Having formulated the governing equations that are applicable our natural desire would be to seek a solution. Unfortunately, we cannot expect to arrive at a general solution because from a mathematical viewpoint the equation of motion is a higher order, non-linear equation to which at the present there exists no general solution. Therefore, the only recourse we have is to seek solutions for various individual cases. This of course requires (*a*) a knowledge of the appropriate boundary conditions; (*b*) a knowledge of the physical properties; and (*c*) dexterity in handling non-linear equations. It is at once evident why the magneto-gasdynamicist should be inclined both mathematically and experimentally.

The scope of this review does not permit our presenting the solutions to different problems. However, it may be helpful to impart a physical feeling by discussing briefly a number of similarity conditions in magneto-gasdynamics.

The hydromagnetic equation will remind the hydrodynamicist of the vorticity transport equation which is generally expressed as follows:

$$\frac{\partial \boldsymbol{\Omega}}{\partial t} = \nabla \times (\mathbf{V} \times \boldsymbol{\Omega}) + \nu \nabla^2 \boldsymbol{\Omega}$$

Because in steady flow classical hydrodynamics and in magneto-hydrodynamics we expect $\nabla \cdot \mathbf{V} = 0$, it follows that the analog of Maxwell's equation $\nabla \times \mathbf{H} = 0$ is $\nabla \times \Omega = 0$.

For the case when the electrical resistivity of the fluid is negligible, $\sigma \to \infty$ and the hydromagnetic equation becomes,

$$\frac{\partial \mathbf{H}}{\partial t} = \nabla \times (\mathbf{V} \times \mathbf{H})$$

This equation is reminiscent of Helmholtz' vortex theorem which prescribes that in an inviscid compressible fluid, a fluid element which originally has no rotationality cannot subsequently acquire rotation. In other words, the vortex lines move with the fluid, i.e., they are locked into it. Similarly, one may envisage the case when the magnetic lines are locked into the fluid or the magneto-hydrodynamic flow is frozen. This condition will occur when there is no leakage of the magnetic field. Hence $\partial \mathbf{H}/\partial t = 0$, and consequently $\nabla \times (\mathbf{V} \times \mathbf{H}) = 0$.

It can be shown then that for one-dimensional frozen flow

$$d(\rho H) = 0$$

From classical, one-dimensional gasdynamics it is known that

$$d(\rho V) = 0$$

Combining the last two equations, one can show that

$$\frac{dH}{d\rho} = \frac{H}{\rho} = \text{constant}$$

Although the assumption of frozen flow makes easier the analytical solution of many problems, from a practical viewpoint this mathematical convenience is not realistic because most engineering plasmas have finite and indeed very small conductivities. We are therefore interested in establishing the criterion which would determine to what extent the magnetic flux lines may be locked into the flow field. Thus, we re-write the hydromagnetic equation as follows:

$$\frac{\partial \mathbf{H}}{\partial t} = \nabla \times \left[(\mathbf{V} \times \mathbf{H}) - \frac{\mathbf{J}_c}{\sigma \mu} \right]$$

It is evident that for frozen flow to exist, it is necessary that the second term on the right-hand side must vanish. It is easy to see that $\mathbf{J}_c/\sigma\mu \to 0$ as $\sigma \to \infty$. However, because from an engineering viewpoint the electrical conductivity can hardly approach infinity, this term will be non-zero and finite. Therefore, we can surmise that there takes place a diffusion or leakage of the magnetic field through the medium. This may then be expected to have a smoothing out effect on small irregularities in the magnetic field. It is evident that whenever $\mathbf{J}_c/\sigma\mu \neq 0$, neither $\partial \mathbf{H}/\partial t$ and $\nabla \times (\mathbf{V} \times \mathbf{H})$ can equal zero, and therefore frozen flow becomes questionable whenever the fluid has resistivity which is of course a manifestation of dissipative effects.

It seems logical to define the scalar ratio $VH/J_c/\sigma\mu$ as a criterion for the extent of relative motion between the magnetic field and the flow field. Thus whenever $\sigma\mu VH/J_c$ is very large compared to unity, we can justify the absence of relative motion between the two fields and the flow will tend to be frozen. Conversely, when the ratio $\sigma\mu VH/J_c$ is small compared to unity, then there will be appreciable relative motion between the two fields and we should be aware of the leakage of the magnetic effects through the medium. Elsasser [12] ascribed to this ratio a physical meaning and named it the magnetic Reynolds number. This is defined

$$Re_m = \mu\sigma VL = \frac{VL}{\nu_m}$$

Accordingly, $Re_m \gg 1$ corresponds to magnetically frozen flow, whereas $Re_m \ll 1$ corresponds to leakage flow. It is now easy to see why, in cosmical situations, the decay of the magnetic field is bound to be small. Not only are the characteristic distances extremely large, but as a rule the electrical conductivities are very high. Therefore, the magnetic Reynolds number can easily assume values which are in the thousands. On the other hand, in laboratory apparatus both L and σ are relatively small. Hence even a magnetic Reynolds number of only slightly greater than unity is most welcome.

There are a number of very useful similarity parameters and these are listed in Table II.

TABLE II MAGNETO-GASDYNAMIC SCALING PARAMETERS

Name	Definition
Hartmann number	$Ha = \mu HL\sqrt{\sigma/\eta}$
Magnetic Reynolds number	$Re_m = \rho VL\sigma = \frac{VL}{\nu_m}$
Cowling number	$C = \frac{V_A}{V}$
Kármán number	$Ka = \frac{V}{V_A}$
Magnetic Mach number	$M_m = \frac{V}{\sqrt{a^2+V_a^2}}$
Electromagnetic field number	$N_{EM} = \frac{HE}{\rho V^3}$
Charge density number	$N_q = \frac{VL\rho_e}{H}$
Lundquist number	$Lu = B\sigma L\sqrt{\mu/\rho}$
Interaction parameter	$N = \frac{\sigma B^2 L}{\rho V}$
Hall coefficient	$N_H = \omega_c\tau$

In Table II, V_A signifies the all important Alfven wave propagation velocity. In 1942, Alfven showed [13] that when a conducting liquid such

as mercury is subjected to a magnetic field, there will arise waves which travel in the direction of the applied magnetic field. These waves will carry with them an induced magnetic field as well as a velocity field. Considering an incompressible fluid having no resistivity, Alfven showed that the waves now named after him propagate with a velocity V_A defined as follows:

$$V_A^2 = \frac{\mu H^2}{\rho}$$

The Alfven waves are thus analogous to shallow surface waves found in classical hydrodynamics or to sound waves encountered in gasdynamics. A number of useful scaling parameters incorporating the Alfven wave velocity may be defined, and these are cited in Table II.

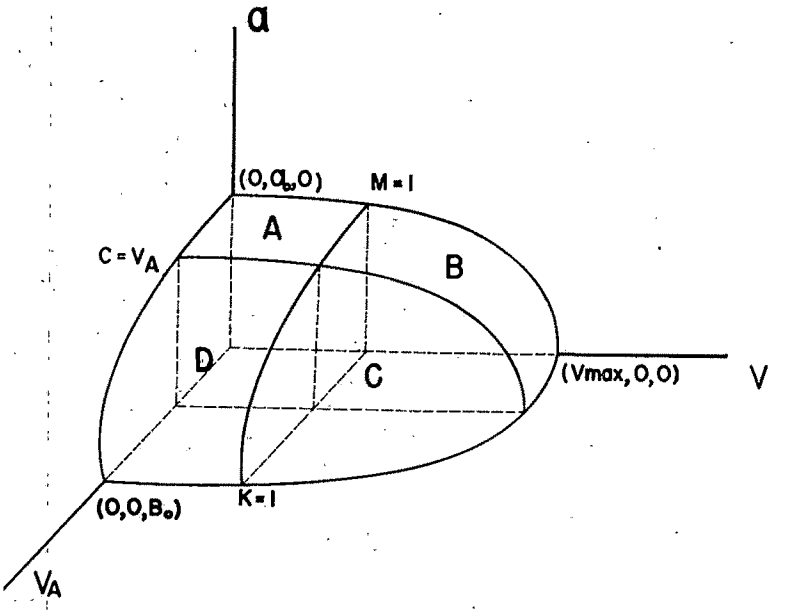


FIG. 7. Regimes of magneto-gasdynamics.

In some of our studies yet to be published we have found that two such numbers are very useful in the analytical prediction of flow regimes. For example, the energy equation for steady flow may be written

$$V^2 + \frac{2}{\gamma - 1} a^2 + V_A^2 = \text{const.}$$

We can represent this equation geometrically as in Figure 7 in which several magneto-gasdynamic regimes are recognized. Thus in regime A we have subsonic flow with no appreciable magnetic effects, whereas in regime B the flow is supersonic, though still without important magnetic

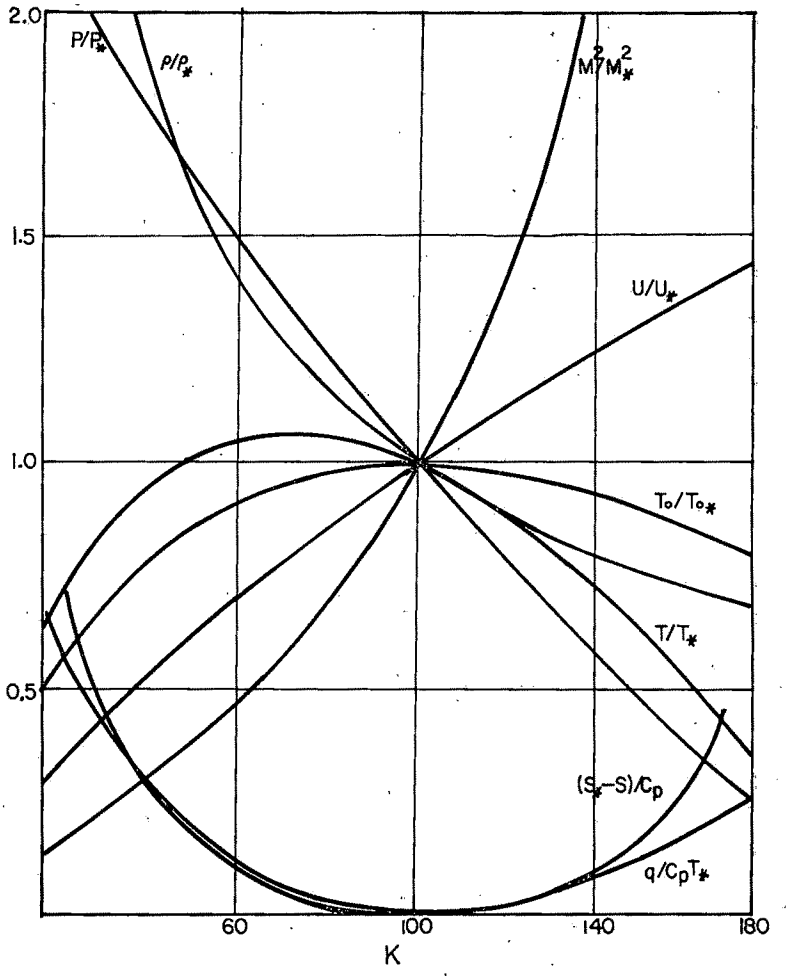


FIG. 8. One-dimensional, adiabatic, magneto-gasdynamic flow with friction.

effects. In regimes C and D the magneto-gasdynamic phenomena are important. The flow in regime C is supersonic; in D it is subsonic.

It can be shown further [14] that we will encounter compression when

$$\frac{2dM}{2M + (\gamma - 1)M^3} < \frac{dK}{K}$$

Similarly, expansion occurs when

$$\frac{2dM}{2M + (\gamma - 1)M^3} > \frac{dK}{K}$$

Two classes of problems are important in flow devices, namely, heat transfer and friction. Because the mathematics of such problems becomes intractable, we found [15] that a one-dimensional approach may

give useful insights into the phenomena. Thus, we defined a critical Kármán number in terms of the Mach number

$$K_*^2 = \frac{M_*^2}{M_*^2 - 1}$$

Because an imaginary number has no meaning in this consideration, we must have $K_* \geqq 1$ and $M_* \geqq 1$.

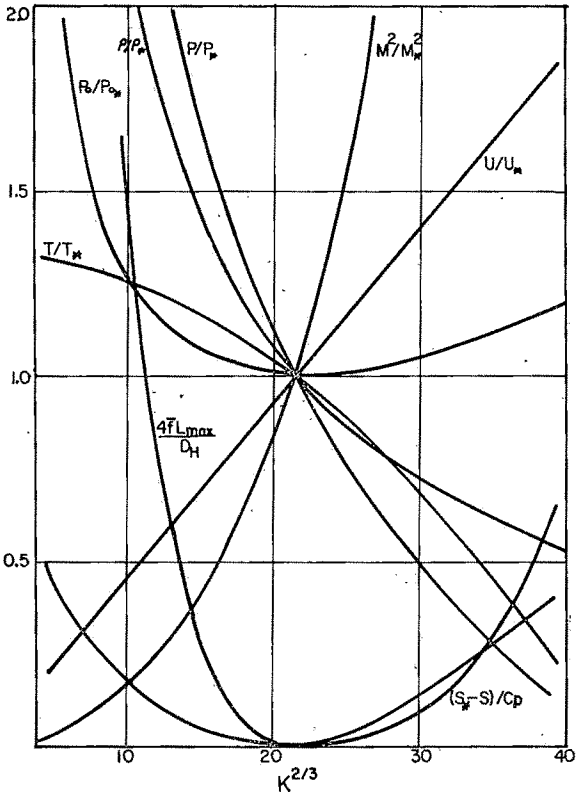


FIG. 9. One-dimensional, frictionless, magneto-gasdynamic flow with heat transfer.

We could then show that, for adiabatic flow with friction in a constant area duct, the maximum permissible duct length between K and K_* may be given as follows:

$$4f\frac{L_{max}}{D_H} = \frac{\xi}{\gamma}(K^{-4/3} - K_*^{4/3}) + \frac{2(\gamma+1)}{3\gamma}\ln\frac{K}{K_*} + \frac{2(2-\gamma)}{3\gamma}(K^{-2} - K_*^{-2})$$

Because the mathematical manipulations leading to the Fanno relations for temperature, density, etc., are elaborate, we shall not discuss them here. However, in Figure 8 we show the results of an electronic

digital computer solution for adiabatic flow with friction in the presence of a magnetic field. Of course, similar design charts for different values of K_* can be computed.

Similarly, we derived one-dimensional equations for frictionless flow with heat transfer and some of the results of our computer solution are summarized in Figure 9. It is evident that there occurs a singularity when the magnetic Mach number is equal to unity. Furthermore, when there is no magnetic field, $\mathbf{B} = 0$ and $K \rightarrow \infty$. For this special case, the magneto-gasdynamic equations describing one-dimensional heat transfer reduce to the equations of classical gasdynamics which is a pleasant observation because as yet there is no experimental work to confirm our analytical predictions.

Experimental Observations and Diagnostic Techniques

It is evident from our previous discussion that there exists a substantial body of profound theories and imaginative hypotheses which describe the physics and behavior of plasmas. On the other hand, experimental work has not progressed as much. The experimental studies are needed in measuring greatly needed data, in observing the flow phenomena and in serving as the origin of practical applications.

Experimental studies, although challenging, pose a number of complexities. The reasons for this are multifold. First, a plasma encountered in most engineering problems consists of a mixture of neutral atoms and/or molecules, electrons, positive and possibly even negatively charged ions. Thus, it is necessary to know the chemical composition and the respective concentrations of the constituents. Second, in many practical cases, the plasma experiences physico-chemical reactions making it necessary to take extreme precautions in measurements which rely on equilibrium considerations. For example, the neutral gas entering the arc chamber of a plasma generator will be in equilibrium but, in the immediate vicinity of the electrodes, the ionized gas will probably not be in equilibrium. However, downstream of the arc chamber the plasma will likely return to equilibrium. Third, any measurements of the plasma will have to realize that the medium is electrically conducting and thus many measurement techniques perfected for high speed, high temperature neutral gases must be properly modified. Fourth, the very high temperatures encountered in plasmas make difficult the choice of materials which have to withstand high thermal energy fluxes. Fifth, the very high temperatures associated with plasmas suggest that radiation effects cannot be neglected without due deliberation. Sixth, most engineering devices will utilize high-velocity, turbulent flow, which inherently complicates measurements and, further, any probes introduced into the flow will disturb and alter the flow pattern. Seventh, when a

magnetic field is applied to the plasma, the properties will no longer be scalar quantities but will assume directional characteristics.

A number of relatively simple, yet most illuminating studies may be conducted with mercury. For example, one might repeat, with a magnetic field, Reynolds' well-known experiment showing the transition from laminar to turbulent flow. Among the earliest and most significant experiments performed with mercury are those by Lundquist [16] who demonstrated the existence of magneto-hydrodynamic waves in an oscillating cylinder. The principal advantage of using mercury in laboratory studies is that it is a fine electrical conductor at room temperature.

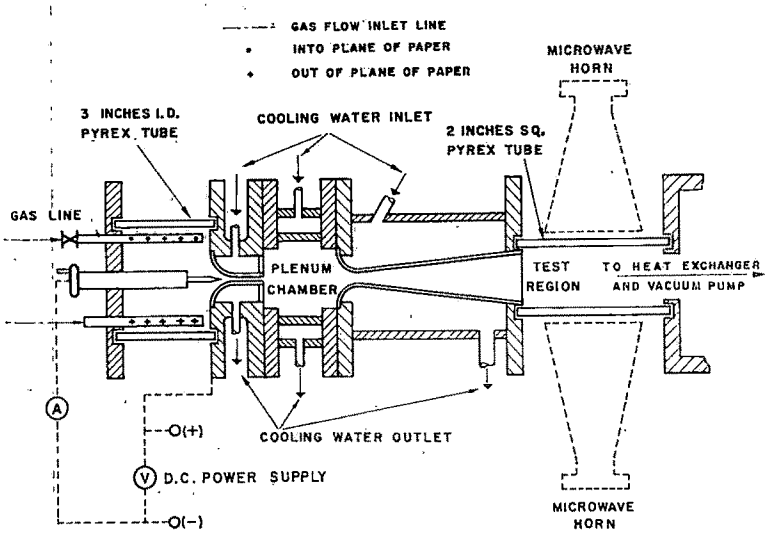


Fig. 10. Schematic of Gas Dynamics Laboratory, Northwestern University plasma arc tunnel.

Therefore, the experimenter does not have to concern himself with the rather severe design complexities which are involved in the construction of plasma equipment.

Those who wish to experiment with plasmas have a variety of configurations at their disposal. In studying the properties of stationary plasmas or gas discharges, one can resort to using discharge tubes which are readily available commercially. It is also possible to generate streams of cool plasma by using low currents and high voltages. Plasma phenomena which are not due to high thermal inputs can be achieved further by means of high frequency oscillators. At the other extreme of complexity are the various facilities which have been developed in connection with thermonuclear fusion processes. We shall not discuss these further because they fall outside the scope of our activities.

Probably the two most versatile devices are the electromagnetic

shock tube and the plasma arc jet. The arc jet configuration (also called the hyperthermal arc) is a very popular configuration because, in essence, it represents many of the technological devices being contemplated. The arc jet appeals to the laboratory scientist further because it is a continuous flow facility and hence the timing of measurements is not so crucial. There are various ways of designing plasma arc devices depending on the needs and indeed tastes of the researcher. In Figure 10 may be seen the schematic diagram of one of the plasma generators

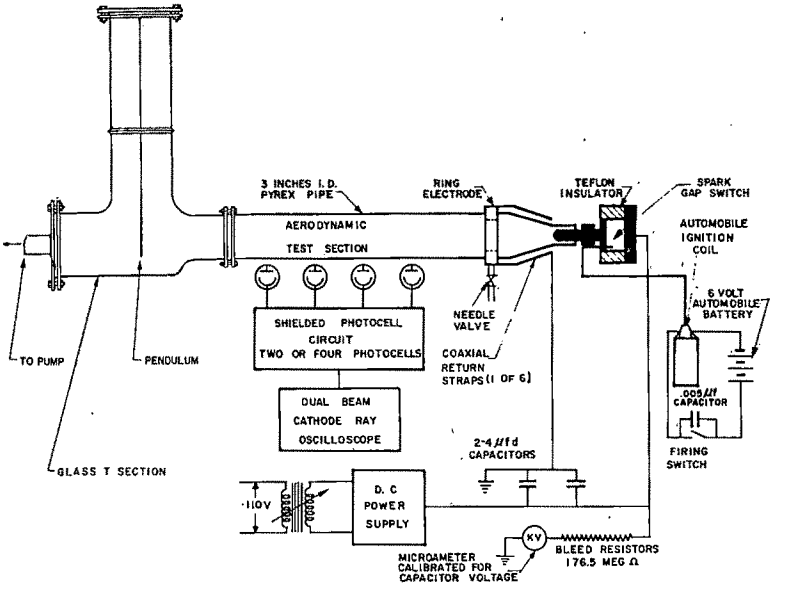


FIG. 11. Schematic of Gas Dynamics Laboratory, Northwestern University conical driver electromagnetic shock-tube.

designed in our laboratory. This facility is very flexible and allows for the interchanging of test sections. Thus it may be used in studies relating to power generation, magneto-aerodynamic drag, chemical synthesis, propulsion motors, and in the diagnostics of plasma properties. Because the arc jet is a continuous flow device, the temperatures and velocities which may be achieved are somewhat limited. For example, the unit shown in Figure 10 is limited in its present form to Mach numbers of about 10 and temperatures of about 20,000°K.

The experimenter who is interested in still higher speeds and temperatures often finds the electromagnetic shock tube an appealing piece of apparatus. Here, too, there are a variety of configurations to satisfy the scientist's tastes. In Figure 11 may be seen the schematic of the conical discharge section electromagnetic shock tube used in our laboratory, while Figure 12 is a photograph of this facility. The advantage of the

conical drive tube over the so-called T-tube is that cleaner plasmas can be obtained, and, furthermore, the attenuation of the shock is minimized. Shock velocities of several hundred times the speed of sound and temperatures of several hundred thousand degrees Kelvin can be achieved in shock tubes of this type. Still higher temperatures can be achieved by placing two tubes facing one another so that their respective shock waves collide with one another. The main disadvantage of the electromagnetic shock tube is the extreme rapidity with which the events take place. On the other hand, shock tubes have earned themselves the devoted loyalty of the experimentalist because the data are repeatable.

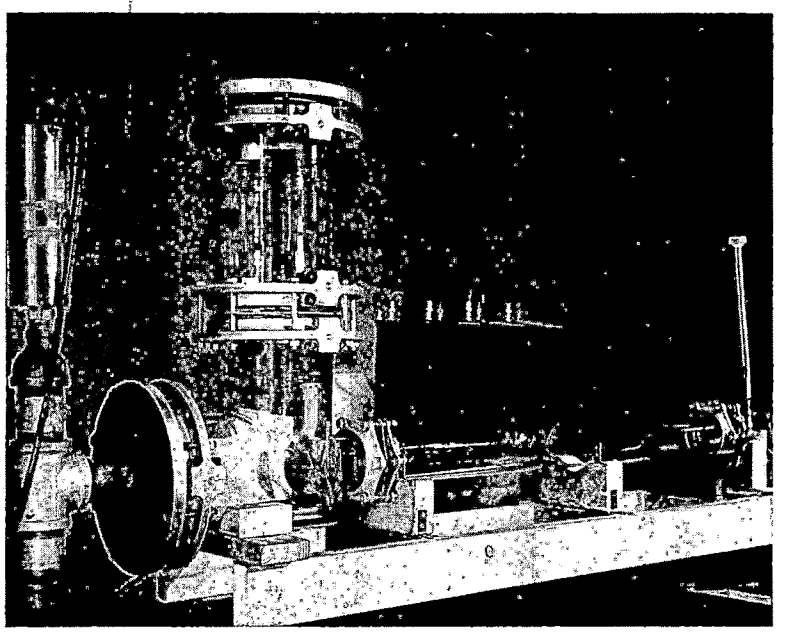


FIG. 12. Gas Dynamics Laboratory, Northwestern University electromagnetic shock-tube facility

The laboratory studies which might be conducted in the realm of magneto-gasdynamic energy conversion techniques may be classified into any of three categories, namely (*a*) diagnostics of the plasma properties, i.e., their electrical, thermal and transport properties; (*b*) observation of the flow and measurement of the flow parameters; (*c*) performance evaluation of energy-converting devices. There are numerous plasma diagnostic techniques. In some of these one uses ion diagnostics, while in others electron diagnostics may be preferred. The advantages and the disadvantages of the various methods have been studied and a careful evaluation has been presented by Wharton [17].

It was mentioned already that the chemical composition of plasmas

and the ionization and de-ionization rates are of great interest. Spectroscopy serves as a very powerful tool, and suitable techniques have been developed [18] in this laboratory. It must be admitted that our technique pre-supposes that the plasma is optically transparent.

Another diagnostic technique, and one which is particularly well-suited in diagnostics leading to the electrical properties, utilizes microwave instrumentation. Although in theory microwave techniques should be quite straightforward, the researcher faces numerous complexities when he starts to conduct actual experiments. Thus, he must be cognizant of the geometry of the plasma. For example, he must avoid applying the conclusions reached in utilizing the planar approximation to cylindrical plasmas. Furthermore, he might encounter difficulties in obtaining microwave components because the microwave signal frequencies must

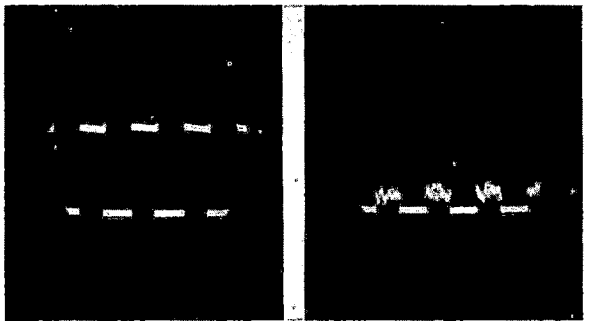


FIG. 13. Transmission of x-band microwave signals through argon plasma.

be appreciably higher than the plasma frequencies. In developing our microwave instrumentation [19], we have experimented with 3 cm to 3 mm wavelength equipment. Furthermore, we have made transmission and interferometer measurements. The transmission measurements indicated so much randomness that the data could not be interpreted rationally. For example, in Figure 13a we see the case in the absence of plasma, while in Figure 13b may be seen the pattern when X-band signals are passed through a high velocity argon plasma jet.

When the plasma is subjected to a magnetic field, there will occur so-called "windows" in the plasma because its dielectric constant may assume two values, the ordinary and the extraordinary. Therefore, by applying a magnetic field, we can propagate electromagnetic waves which have lower frequencies than the plasma. This offers some interesting potentialities to the diagnostician.

In studying the fluid mechanics associated with continuous flow arc jets, one may use Langmuir probes, photographic techniques, and optical instruments such as the Toepler-schlieren system or the Mach-

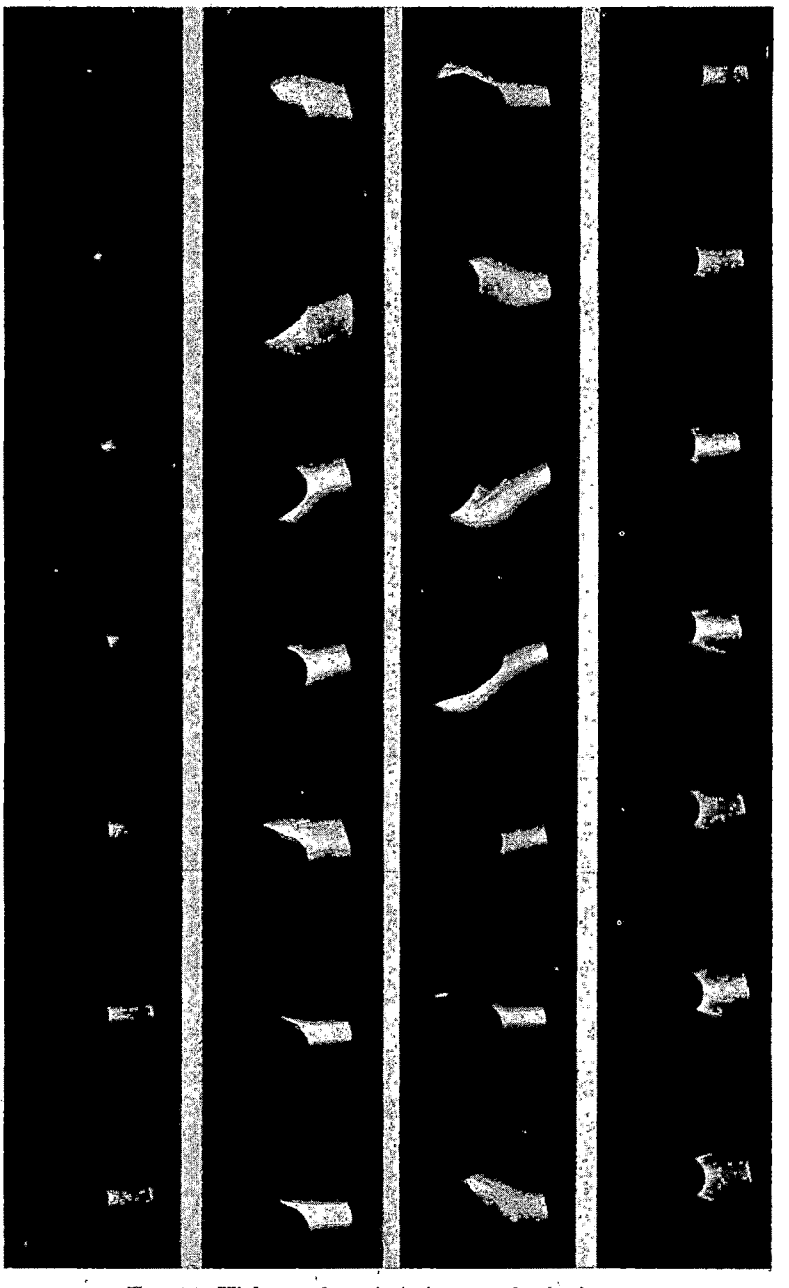


FIG. 14. High speed motion pictures of spinning arc

Zehnder interferometer. In Figure 14 may be seen film strips from a high speed motion picture showing the establishment of a stable argon plasma arc in our plasma arc tunnel. There are two complications which the experimentalist with a liking for schlieren and interferometric techniques encounters. These are the low gas densities and the high level of luminosity of the plasma. However, these can be overcome by careful adjustment of the viewing apparatus.

The determination of the fluidmechanical phenomena in electromagnetic shock tubes is generally based on observing the luminous front. Such measurements can be carried out conveniently by using photomultiplier tubes located at strategic points along the shock tube or by using streak cameras. By relying on photomultipliers, we have studied [20], the attenuation of the shock wave proceeding down the shock tube when air was used, and this is shown in Figure 15. In this

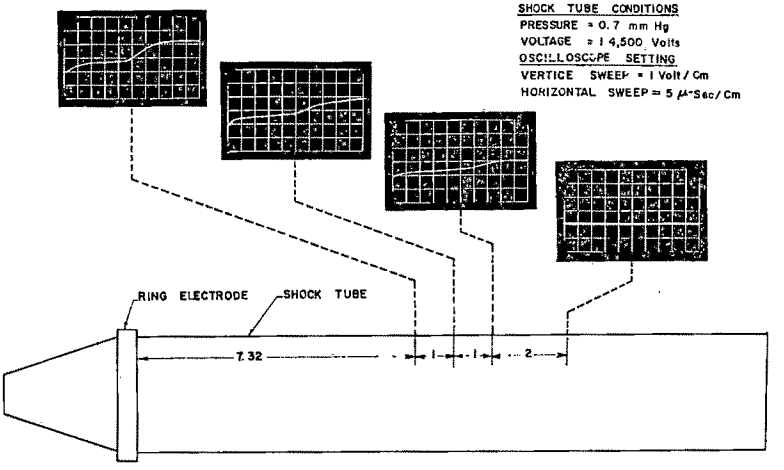


FIG. 15. Oscilloscope trace showing decrease in intensity of luminosity behind shock wave.

connection it was of interest to find out if radiation may enhance the attenuation. Our studies indicated that this is indeed the case in the close proximity of the discharge and when the pressures are high. The results of these studies are summarized in the topological diagram depicted in Figure 16.

Closure

The aforegoing discussion indicates that it would not be an overstatement to suggest that magneto-gasdynamics is a renaissance science and that in the words of von Kármán it constitutes "happy hunting grounds" for the inquisitive scientist. The importance of the subject has been confirmed by the President's Scientific Advisory Board which

recommended that magneto-hydrodynamics be incorporated into engineering and science curricula.

In ending our presentation, it is probably fitting to quote Michael Faraday, the originator of magneto-hydrodynamics: "It is the great beauty of our science that advancement in it, whether in a degree great or small, instead of exhausting the subject of research opens the doors to further and more abundant knowledge, overflowing with beauty and utility."

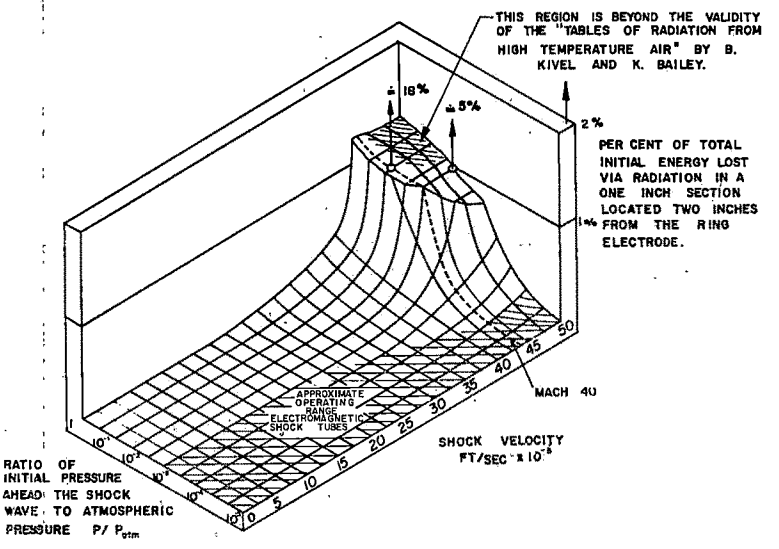


FIG. 16. Effect of shock wave parameters on radiation losses in electromagnetic shock tube.

Acknowledgments

It is a distinct pleasure for me to extend my thanks to my students without whom much of this work would have been impossible. It must be recognized also that our research would be curtailed drastically without the financial aid of various groups. Thus, I wish to acknowledge the support of the USAF, Arnold Engineering Development Center, which makes possible our analytical studies of the kinetics and physics of real gases under Contract No. AF 40(600)-748. The USAF, Office of Scientific Research has been generous in allowing us to pursue our experimental program in magneto-gasdynamics under Contract No. AF 49(638)-879. The National Science Foundation is making it possible for us to develop prototype educational experiments under their grant NSF-G17692. Last, but not least, I wish to express my appreciation to various industrial companies and to Northwestern University for their support and encouragement.

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SPACE PROPULSION ENGINES—THEIR CHARACTERISTICS AND PROBLEMS*

By M. J. ZUCROW

THE technical problems associated with the exploration of space lie in many fields. To name a few, there are problems in the fields of aeromedicine (including protection against radiation), communications, vehicle guidance, biology, bacteriology, astronomy, environmental control of space vehicles and space stations, electric power supply, and propulsion. It is generally acknowledged that "propulsion is the key to space exploration" [1], and it is no exaggeration to state that the *advances made* in propulsion will exert the *rate-controlling-influence* upon the specific programs of space exploration that can be undertaken.

Current conceptions for conducting flights into outer space are based, in general, on missions comprising two or more phases. First, there would be a *"boost" phase*, during which a "booster" vehicle carries a separable payload from the surface of the earth and places it in an "assembly" orbit located from 300 to 1000 miles from Earth's surface; the booster

TABLE I

SOME FUNCTIONS TO BE PROVIDED BY PROPULSION SYSTEM

(1) Acceleration or boost thrust
(2) Deceleration thrust
(3) Satellite orientation thrust
(4) Efficient transfer of a payload to a given destination
(5) Electric power for operating the space vehicle or the space station
(6) Control of internal atmosphere of the space vehicle or the space station

vehicle would then return to Earth and either be discarded or possibly be re-employed for transporting other payloads to the "assembly" orbit. The actual journey into space would start from the orbit, from an artificial satellite "space station," after the spaceship had been assembled and equipped for its flight, by rendezvous techniques [1,2,3]; these have yet to be established. Other flights are based on boosting a payload, the last stage of a multistage vehicle, to the required flight velocity.

Table I presents the major functions which must be provided by propulsion engines for space exploration.

* A Sigma Xi-RESA National Lecture, April 1962.

Items 5 and 6, in Table I, are not truly propulsion functions. They are included for two reasons:

(1) The exploration of space, especially manned exploration, requires that an adequate supply of electric power be available in space; and
(2) The technical problems pertinent to developing powerplants for supplying electric power in space, are similar to those encountered in developing certain propulsion engines for space exploration.

In general, the characteristics desired for a space propulsion engine are determined in large measure by the space mission being considered; the word "mission" denotes the specifications defining the objectives

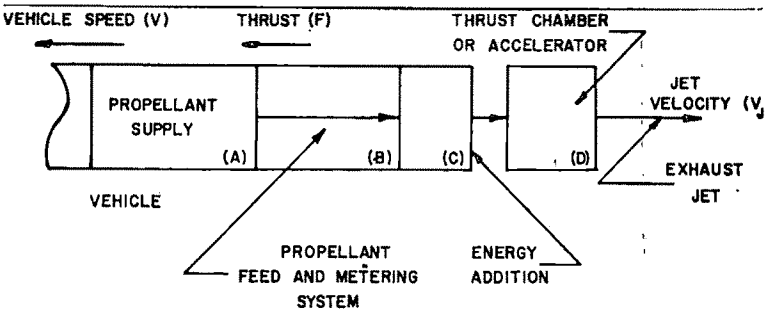


FIG. 1. Essential features of a rocket engine.

of the space flight. The contemplated and conceivable space missions differ widely in objectives, and consequently, there is a wide variation in the characteristics desired for space propulsion engines [2,3,4]. Some missions, such as boosting a payload from Earth to either the *escape velocity* or to the velocity for placing an artificial satellite into an orbit around Earth, require *large-thrust engines;* that is, engines developing thrusts up to several millions of pounds. On the other hand, there are

TABLE II

SOME APPLICATIONS FOR SMALL-THRUST ENGINES

(1) Adjustment of the orbit of a low altitude satellite to increase its lifetime
(2) Raising or lowering a satellite orbit
(3) Changing the orientation of a satellite orbit
(4) Propulsion of instrumented probes to Mars and Venus
(5) Propulsion of manned spaceships from one Earth orbit to another, or from one planetary orbit to another

missions requiring *small-thrust engines,* engines developing thrusts ranging from very small fractions of a pound to at most tens of pounds. Table II presents some typical missions requiring *small-thrust engines* [12,50,53].

Experience has amply confirmed the predictions of the pioneers in rocketry, that, from a propulsion standpoint, all of the space missions so far conceived can be achieved by properly applying the *rocket jet propulsion principle,* hereafter termed *rocket propulsion.* Accordingly, the discussions here are concerned with the characteristics and the research and development problems pertinent to rocket engines for space propulsion.

Figure 1 illustrates the essential elements of a rocket engine. Three main components are involved:

(1) a supply of propellant material carried in the rocket-propelled vehicle;
(2) a propellant feed and metering system; and
(3) a thrust chamber; also called a rocket motor, thrustor, or accelerator.

In all rocket engines, regardless of type, there must be provided means for adding energy to the propellant as it flows through the thrustor.

Refer to Figure 1. Propellant material from the supply is metered and fed to the thrust chamber wherein energy is added to it. As a consequence of the energy addition, the propellant is ejected from the thrustor in the form of a high velocity jet of material, termed the *exhaust jet.* The ejection speed of the exhaust jet is termed the *jet velocity,* and is denoted by V_j. A propulsive force, termed the *thrust,* denoted by F, is thereby produced acting in the direction opposite to that of the jet velocity, V_j; that is, the thrust acts in the same direction as the *vehicle velocity;* the latter is denoted by V [5,6].

In the abstract, at least, there is no restriction upon the kind of matter that can be used as the *propellant* material comprising the exhaust jet. It may be solid particles, liquid particles, gaseous particles, a plasma, ions, electrons, and combinations thereof. The term *rocket engine* refers to the operating assembly of all of the components needed for producing the aforementioned *exhaust jet.*

Classification of Rocket Engines

To date we have obtained an inkling of the possibilities for exploring space. It is evident, however, that to achieve space exploration in its fullest sense, use must be made, wherever possible, of all of the forms of energy known to man. Consideration must, therefore, be given to the application of (1) solar energy, (2) chemical energy, (3) nuclear energy, and (4) electrical energy.

Calculations indicate that because of the large amounts of power required by a space propulsion engine, the application of solar energy leads to impractically large and heavy propulsion engines. Rocket en-

gines may, accordingly, be grouped in accordance with the forms of energy they use in their functioning. Thus, there are three major groups:

(*a*) chemical rocket engines;
(*b*) nuclear heat-transfer rocket engines; and
(*c*) electric rocket engines.

Brief descriptions of the different types of rocket engines will be presented at this point, primarily for identification purposes. More detailed discussions of the more important engines will be presented later.

(*a*) *Chemical Rocket Engines:* All chemical rocket engines have two common characteristics [5,6].

(1) They utilize chemical reactions in the thrust chamber as the means for producing a high temperature *propellant gas* at the entrance plane of a converging-diverging exhaust nozzle; the nozzle is the *propulsive element* of the rocket engine.

(2) The high temperature propellant gas expands as it flows through the converging-diverging nozzle and is ejected to the surroundings as a high velocity gaseous exhaust jet. The expansion in the exhaust nozzle is a *thermodynamic process* by which a portion of the thermal energy (enthalpy of reaction), released by the combustion of one or more propellants in the thrust chamber, is converted into the kinetic energy associated with the high velocity gaseous exhaust jet.

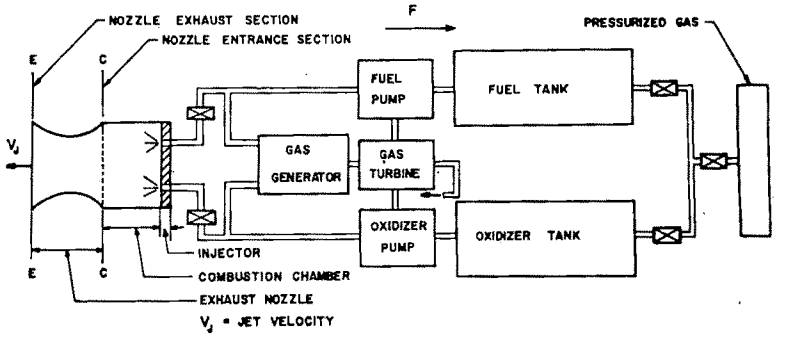


FIG. 2. Essential features of a liquid bipropellant rocket engine.

All of the components of a chemical rocket engine, except the exhaust nozzle of the thrust chamber, form essentially a *hot gas generator* for supplying a gas, at a high temperature and pressure, to a suitable exhaust nozzle.

Chemical rocket engines may be grouped into four main categories:

(i) liquid bipropellant rocket engines;
(ii) liquid monopropellant rocket engines;
(iii) solid propellant rocket engines; and
(iv) hybrid chemical rocket engines.

(I) LIQUID BIPROPELLANT ENGINE. Figure 2 illustrates the essential features of a liquid bipropellant rocket engine employing a turbopump for feeding two propellants (an oxidizer and a fuel) to a rocket motor.

(II) LIQUID MONOPROPELLANT ENGINE. Figure 2 also illustrates the essential features of a monopropellant engine if one removes the oxidizer tank, oxidizer pump, oxidizer lines, etc. Since a monopropellant contains the oxygen for combustion in its chemical or physical structure, an oxidizer supply with its associated feed and metering system is unnecessary.

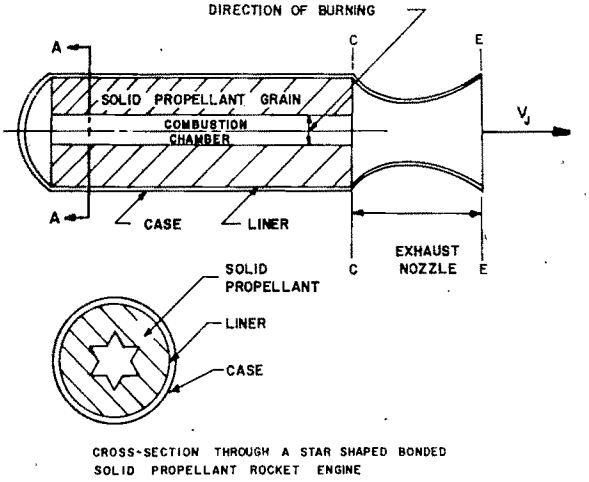


FIG. 3. Essential features of an internal-burning case-bonded solid propellant rocket engine.

(III) SOLID PROPELLANT ROCKET ENGINE. Figure 3 illustrates schematically a solid propellant rocket engine employing an internal-burning *case-bonded grain;* the latter burns radially outward at a substantially constant rate. A solid propellant contains both its fuel and the requisite oxidant for burning it, either in the solid propellant molecule (*double-base propellants*) or as an intimate mechanical mixture (*heterogeneous* or *composite propellants*). The chemical reaction is initiated by means of an electrically-fired igniter.

(IV) HYBRID CHEMICAL ROCKET ENGINE. Figure 4 illustrates schematically the essential features of a hybrid (chemical) rocket engine. The solid propellant, which may or may not contain a small amount of oxidizer, is reacted with a liquid oxidizer and burns in the radially outward direction at a substantially constant rate.

Of the chemical rocket engines presented above only the liquid bipropellant and the solid propellant engines have attained an operational state of development. Thus, the Atlas and the Titan (ICBM)*

* ICBM—Intercontinental Ballistic Missile. IRBM—Intermediate Range Ballistic Missile.

missiles employ liquid bipropellant engines, and the Minuteman (ICBM) and the Polaris (IRBM) employ solid propellant rocket engines. Consequently, the discussion of chemical rocket engines will be limited to liquid bipropellant and solid propellant rocket engines.

(*b*) *Nuclear Heat-Transfer Rocket Engine:* Figure 5 illustrates schematically the essential features of a nuclear heat-transfer rocket engine employing a solid core reactor. In such an engine, the heat generated by the fissions of the uranium nucleus is utilized for heating a gaseous propellant, such as hydrogen, to a high temperature of approximately 4000R (2200°K) at the entrance section of the exhaust nozzle. The hot

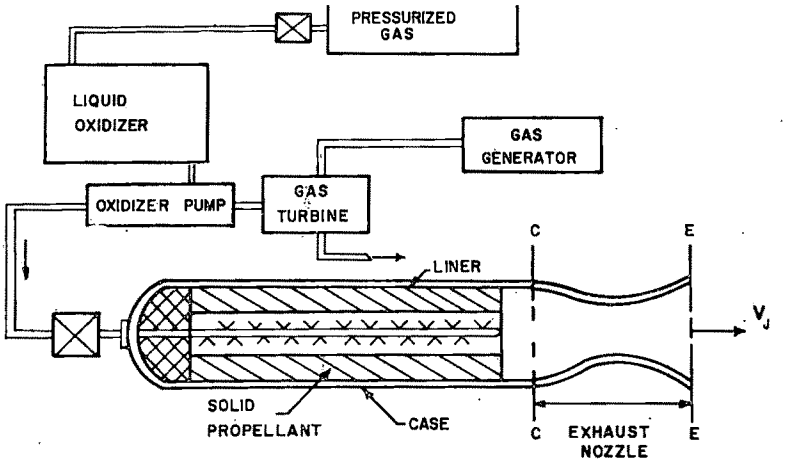


FIG. 4. Essential features of a hybrid chemical rocket engine.

gas is then ejected to the surroundings after expanding in a converging-diverging exhaust nozzle [7]. The basic difference between the operating principles of the nuclear heat-transfer and chemical rocket engines is that, in the former, the energy for heating the propellant gas is obtained from the fission of uranium instead of a chemical reaction. Since the exhaust nozzle is the propulsive element, the remaining components of the nuclear heat-transfer rocket engine constitute the hot gas generator.

The propellant in a nuclear heat-transfer rocket engine functions basically as a fluid for cooling the solid core nuclear reactor. Consequently, its selection is not based on energy considerations but on such properties as its specific heat, latent heat, molecular weight, liquid density, and other practical considerations.

(*c*) *Electric Rocket Engines:* These engines use electric power for either heating or accelerating a propellant [4,9,10,11,17].

Figure 6 illustrates schematically the basic features of an electric rocket engine. It comprises, in addition to the requisite control devices, the following principal components:

(1) a nuclear power source;
(2) an *energy conversion unit* for converting the energy supplied by the nuclear power source into the desired form of electrical energy;
(3) a propellant feed and metering system; and
(4) an electrically operated thrustor or accelerator.

The combination of the nuclear power source and the energy conversion unit, and their associated equipment, will be termed a *nuclear electric powerplant.*

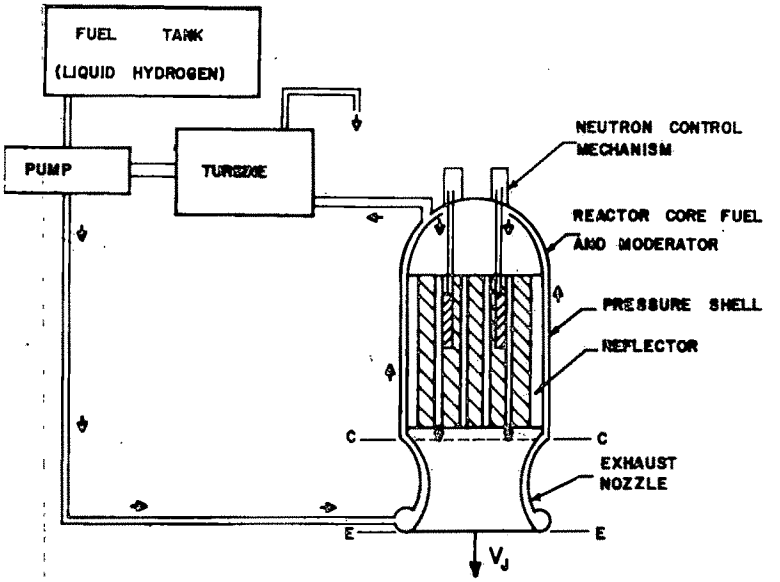


FIG. 5. Schematic arrangement of a nuclear heat transfer rocket engine.

Electric rocket engines may be segregated into three principal groups:

(i) electrothermal rocket engines;
(ii) electromagnetic rocket engines (also called plasma, magnetofluidmechanic, or magnetohydrodynamic rocket engines);
(iii) electrostatic rocket engines.

(I) THE ELECTROTHERMAL ROCKET ENGINE. In general, an electrothermal rocket engine uses electric power for heating a gaseous propellant to a high temperature by the time it reaches the entrance of an exhaust nozzle.

Figure 7A illustrates a form of electrothermal rocket engine known as a *thermal arc-jet engine.* It uses an electric arc for heating a propellant gas. It should be noted that the chemical, nuclear heat-transfer, and electrothermal rocket engines possess the common feature of being *thermodynamic engines;* that is, they produce a hot gas in some form of

hot gas generator and then expand it in a converging-diverging exhaust nozzle to produce a high velocity gaseous jet.

The thrust of a thermodynamic rocket engine is produced by the impacts of atoms and molecules of the propellant gas striking the walls of the thrust chamber and exhaust nozzle. The jet velocity, V_j, obtained from a thermodynamic thrustor is proportional to $\sqrt{T_c/\bar{m}}$ where T_c is the temperature of the gas entering the exhaust nozzle and $\bar{m}$ is its molecular weight. The problem of cooling the exhaust nozzle of a thermoelectric thrustor becomes a serious one when the jet velocity, V_j, exceeds approximately 50,000 fps.

(II) ELECTROMAGNETIC ENGINE. Figure 7B illustrates schematically the essential features of an electromagnetic or plasma rocket engine. There is a wide variety of such engines [51] but all of them utilize the same operating principle. A plasma (a neutral ionized conducting gas) is accelerated by means of its interaction with either a stationary or varying magnetic field.

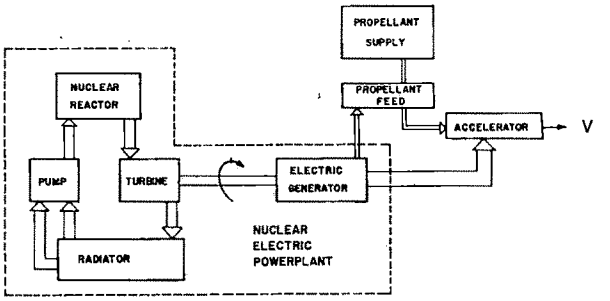


FIG. 6. Essential features of an electric rocket engine.

Basically, the difference between an electromagnetic thrustor and a conventional electric motor is that the *moving armature* of the motor is replaced by a *conducting plasma*. By employing the analogy between an electromagnetic thrustor and an electric motor, electromagnetic thrustors may be segregated into three principal groups: (1) *shunt* thrustors, also termed *crossed-field* or $i \times B$ thrustors; in their operation the electrical current flowing through the plasma and that used for exciting the magnetic field are not the same; (2) *series thrustors*—also called *pinch* or *pulsed* plasma thrustors—wherein the same electrical current flows through the plasma that excites the magnetic field; and (3) *induction* thrustors—also termed *electrodeless* plasma thrustors—which have no electrodes in contact with the plasma; the electrical currents are *induced* in the plasma by the action of *time-varying* magnetic fields and they in turn, by interaction with the applied magnetic fields, achieve acceleration of the plasma [51].

(III) ELECTROSTATIC ENGINE. Figure 7C illustrates schematically the essential features of an electrostatic or *ion rocket engine* [8,11,13]. It comprises the following principal assemblies:

(1) a nuclear electric powerplant;
(2) a propellant supply;
(3) ionization apparatus;
(4) an ion accelerator; and
(5) an electron emitter for neutralizing the ion beam ejected from the accelerator.

Its operating principle is based on utilizing electrostatic fields for accelerating and ejecting electrically charged particles, from an accelerator, with extremely large velocities. The over-all objective is to transform

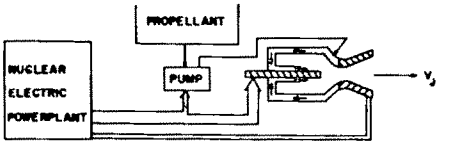


FIG. 7A. Electrothermal rocket engine.

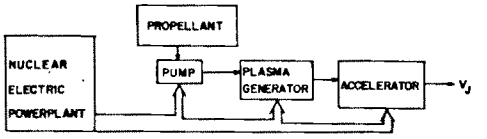


FIG. 7B. Electromagnetic rocket engine.

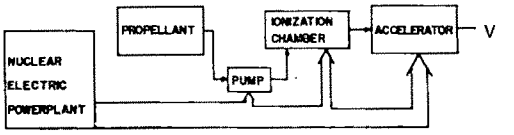


FIG. 7C. Electrostatic (ion) rocket engine.

thermal (nuclear) energy into the kinetic energy associated with an extremely high velocity stream of electrically *neutral* beams ejected from a thrustor [15].

In theory, at least, the charged particles flowing through and ejected from the accelerator may be ions, colloids, droplets, dust, etc. Because of the large background of experience obtained on constructing and operating ion accelerators for high-energy particle research, the current concepts pertinent to electrostatic rocket engines are based on employing some form of ion accelerator as the thrust-producing element of the engine. Most of the current developments are based on using heavy *positively-charged* atomic ions, such as cesium, that are produced by surface ionization. Electrons produced by thermionic emission are usually

proposed for neutralizing the positively charged ion beam ejected from the accelerator.

In a chemical rocket engine, the energy for propulsion as well as the mass ejected through the exhaust nozzle are provided by the propellants, so that no additional equipment is needed for the energy addition process. The energy which can be added to a unit mass of the propellant gas, however, is a fixed quantity limited by the nature of the chemical bonds of the reacting materials [35]. For that reason, chemical rocket engines are said to be *energy-limited rocket engines.* It should be noted that a nuclear heat-transfer rocket engine is also *energy-limited,* the limitation, in that case, being imposed by the amount of energy which can be added per unit mass of propellant without exceeding the maximum allowable temperature for the materials employed in fabricating the solid-core reactor.

In an electric rocket engine, on the other hand, the energy added to the propellant must be furnished by the nuclear electric powerplant. Since the power available for that purpose is limited by the maximum power output of the nuclear electric powerplant, electric rocket engines are said to be *power-limited rocket engines;* they are also termed *separately-powered rocket engines.* In any electric rocket engine the nuclear electric powerplant contributes 90 per cent, or more, of the total weight of the engine [47].

Rocket Performance Parameters

There are several parameters which must be considered in the application of a rocket engine to a specific mission. The more important ones will be defined and discussed briefly [9,10,11].

(*a*) *Thrust* (*F*): The thrust developed by a rocket engine, irrespective of type, is given by

$$F = \dot{M}_{pr} V_j = \dot{W}_{pr}/g_0 V_j \qquad \text{(lb)} \qquad (1)$$

where

$\dot{M}_{pr}$ = rate at which mass is expelled from the exhaust nozzle in unit time.
V_j = effective velocity of exhaust jet, fps.
$\dot{W}_{pr} = g_0 \dot{M}_{pr}$ = weight rate of flow of propellant gas, (lb/sec).
g_0 = 32.174 ft/sec^2.

Equation 1 demonstrates the fundamental characteristic of rocket propulsion; that is, to develop thrust, a mass of propellant M_{pr} must be carried in the propelled vehicle, and ejected to the surroundings at the rate $\dot{M}_{pr}$; the latter depends on the *burning time* t_p.

(*b*) *Specific Impulse* (*I*): By definition,

$$I = \frac{F t_p}{W_{pr}} = \frac{F}{g_0 \dot{M}_{pr}} = \frac{V_j}{32.174} \qquad \text{(sec)} \qquad (2)$$

The specific impulse I, is a measure of the effectiveness with which the *propulsive energy* is converted into exhaust jet kinetic energy. Equation 2 shows that either the specific impulse I or the jet velocity V_j may be employed as a criterion of performance [6].

The specific impulse or jet velocity measures the capability of a rocket engine burning specified propellants to produce "static" thrust [14]. When propelling a vehicle, however, the propulsion problem is a dynamic one having as its goal the acceleration of a payload mass, M_{pay}, to a desired vehicle velocity, V. Consequently, the determination of the required vehicle acceleration depends on other variables in addition to specific impulse [14].

Mission analyses show that, to achieve a large payload-carrying capacity with an energy-limited rocket engine (chemical, nuclear), the specific impulse I should be as large as possible. For a power-limited rocket engine, since the energy source does not reside in the propellant material but in the nuclear electric powerplant, there is an optimum specific impulse, denoted by I_{opt}, for a given flight time and total mass [11], as will be shown later.

(*c*) *Propellant Weight:* The propellant weight, denoted by W_{pr}, denotes the weight of propellant required for achieving a mission, requiring the operating time t_p. Thus

$$W_{pr} = \frac{Ft_p}{I} \tag{3}$$

Equation 3 shows that for specified values of thrust, F, and burning time, t_p, the propellant weight, W_{pr}, required for achieving a given mission decreases as the specific impulse, I, is increased.

The acceptable value for the burning time, t_p, depends upon the characteristics of the mission. If the thrust, F, and the powered flight time, t_p are fixed, then the propellant load required for the mission, W_{p2} is proportional to $1/I$.

(*d*) *Jet Power* (P_j): The power associated with the propulsive jet ejected from a rocket engine, is termed the *jet power*, P_j. Thus

$$P_j = \tfrac{1}{2}\dot{M}_{pr}V_j^2 \qquad \text{(ft-lb/sec)}$$

or

$$P_j = FI/45.8 \qquad \text{(kw)} \tag{4}$$

Equation 4 shows that for a given value of the jet power, P_j, an increase in the specific impulse, I, is obtained only at the expense of a decrease in thrust, F, and vice versa. Because of the inefficiencies which are unavoidable in transforming electrical energy into jet kinetic energy, the power output capacity of the nuclear electric powerplant, called the *required power*, denoted by P, must be larger than the jet power, P_j.

Hence, the required power, P, is given by

$$P = FI/45.8\eta \tag{5}$$

where η (less than 1) is the *power-conversion efficiency*. A large value for η is desirable.

(*e*) *Specific Powerplant Weight* (α): By definition, the specific powerplant weight, denoted by α, is given by

$$\alpha = W_{PP}/P_j \quad \text{(lb/kw)}^* \tag{6}$$

where W_{PP} is the weight of the powerplant.

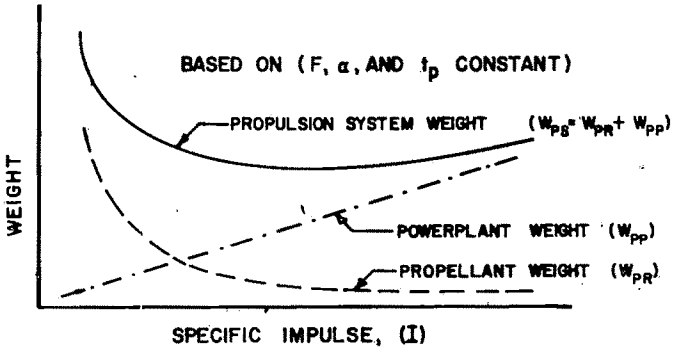


FIG. 8. Propellant, powerplant, and propulsion system weights as a function of specific impulse.

The parameter, α, is applicable to all types of rocket engines, but is particularly convenient in the study of electric rocket engines.

(*f*) *Powerplant Weight* (W_{PP}): It is desirable to express the powerplant weight, W_{PP}, in terms of the thrust, F, the specific impulse, I, and the specific powerplant weight, α. Thus

$$W_{PP} = \alpha P_j = \alpha FI/45.8 \quad \text{(lb)} \tag{7}$$

If F, t_p, and α are fixed, then the powerplant weight, W_{PP}, is proportional to the specific impulse, I.

(*g*) *Propulsion System Weight* (W_{PS}): The toal weight of a propulsion system, denoted by W_{PS}, is given by

$$W_{PS} = W_{pr} + W_{PP} = F\frac{I\alpha}{45.8} + \frac{t_p}{I} \tag{8}$$

Figure 8 presents the propellant, powerplant, and propulsion system weights as functions of the specific impulse, I, for fixed values of the thrust, F, powered-flight time, t_p, and specific powerplant weight, α. It is seen that the powerplant weight increases directly with the specific impulse, while the propellant weight decreases as the specific impulse is increased.

* Ordinarily the specific weight of a rocket engine is defined by the ratio W_{PP}/F

Figure 8 shows that there is a value of specific impulse (assuming fixed values for F, t_p, and α) which makes the propulsion system weight a minimum. That value is the *optimum specific impulse*, I_{opt}, referred to earlier [10,11].

If the specific impulse is *larger* than the optimum value, a heavier powerplant will be required. Conversely, if the specific impulse is *smaller* than the optimum value a larger propellant load will be required.

Payload Ratio

It is desirable that the ratio of the payload, W_{pay}, carried by the spaceship to its initial weight W_0, be as large as possible. If W_{str} denotes structural weight of the vehicle, then [11]

$$W_0 = W_{str} + W_{pay} + W_{PP} + W_{pr} = W_{str} + W_{pay} + W_{PS} \tag{9}$$

Hence

$$\frac{W_{pay}}{W_0} = 1 - \frac{W_{str}}{W_0} - \frac{F}{W_0}\left(\frac{\alpha I}{45.8} + \frac{t_p}{I}\right) \tag{10}$$

Let $W_{str}/W_0 = 0.05$, then Equation 10 becomes

$$\frac{W_{pay}}{W_0} = 0.95 - \frac{F}{W_0}\left(\frac{\alpha I}{45.8} + \frac{t_p}{I}\right) \tag{11}$$

The problem of matching a propulsion system to a mission is ultimately the problem of *maximizing* the payload ratio, W_{pay}/W_0; that is, of *minimizing* the weight of the propulsion system W_{PS}. Hence, a spaceship propelled by an electric rocket engine will transport its maximum payload when the engine is operated to give the optimum specific impulse throughout the journey. In that case the payload ratio is given by

$$\left(\frac{W_{pay}}{W_0}\right)_{max} = 0.95 - 87\, F/W_0 \sqrt{\alpha t_p\,(\text{days})} \tag{12}$$

In equation 12, the ratio F/W_0 is termed the *thrust-to-weight ratio.*

If the electric rocket engine is operated at the optimum specific impulse, then the weight of the propellant load at take-off (from an Earth orbit) must be approximately equal to the weight of the nuclear electric powerplant. Hence, the minimum weight that must be *boosted* from the surface of Earth to the Earth orbit is more than *twice* the weight of the nuclear electric powerplant.

The specific powerplant weight, α, exerts a predominant influence upon the maximum value of the payload ratio, W_{pay}/W_0, and the range of useful values of specific impulse, I.

Figure 9A presents the payload ratio, W_{pay}/W_0, as a function of the specific impulse, I, for a case where the thrust-to-weight ratio, F/W_0, is equal to 10^{-4}.

Figure 9B presents the same information for the case where $F/W_0 = 10^{-3}$.

It is apparent from Figure 9A and 9B that the specific weight, α, of the nuclear electric powerplant exerts a predominant influence upon the maximum value for the payload ratio, and the range of useful values

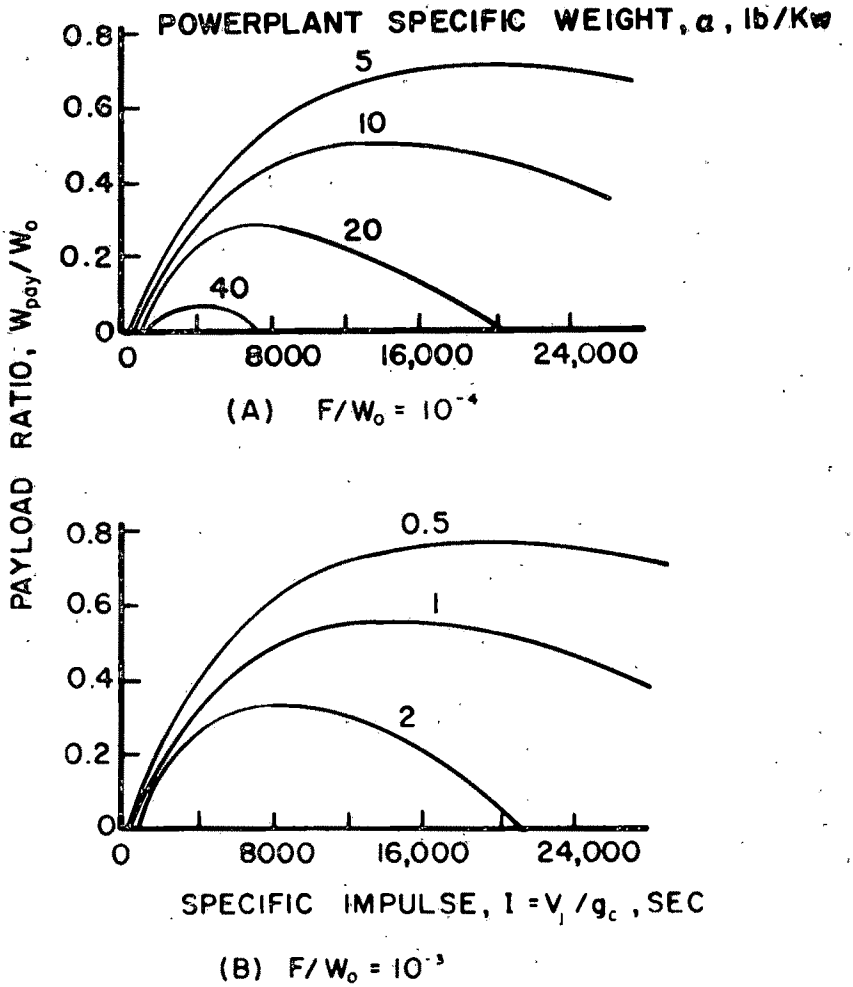


FIG. 9. (A) Payload ratio vs specific impulse for different values of specific powerplant weight ($F/W_0 = 10^{-4}$) (Based on Reference 11). (B) Payload ratio vs specific impulse for different values of specific weight ($F/W_0 = 10^{-3}$) (Based on Reference 11).

of specific impulse. For space exploration to become feasible, reliable nuclear electric powerplants with small values of specific powerplant weight are essential. Without them electric rocket engines cannot be applied effectively to deep space explorations [2,15].

General Characteristics of Nuclear Electric Powerplants for Space Applications

Since the major component of an electric rocket engine, irrespective of type, is its nuclear electric powerplant, it is worth examining briefly the state of the art and the problems to be solved in developing a reliable nuclear electric powerplant for space applications. It must be realized, as noted earlier, that, without an adequate supply of electrical power in space for communications, control, instrumentation, and furnishing an environment for supporting human life in space, it is doubtful that manned exploration of space will be achievable.

The electric power requirements will depend, of course, upon the mission [2,11,15,33]. For missions such as satellite sustaining, satellite

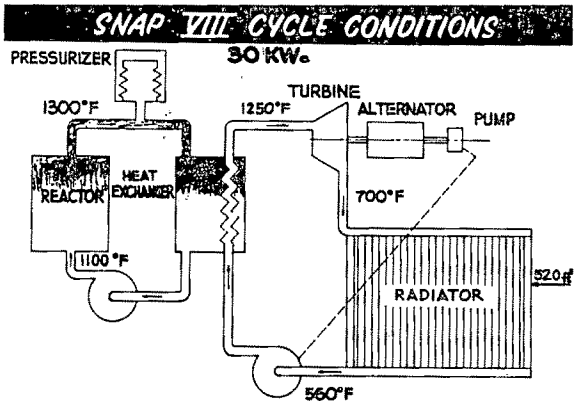


FIG. 10. SNAP VIII power cycle (Taken from Reference 23).

orientation, or for furnishing the electric power for communications, instruments, and the like, the electric power demands will be relatively small; ranging from a few watts to possibly 30 kw. Estimates indicate that, for unmanned lunar and Mars missions, the power requirements will range from 30 to 300 kw, and that for manned missions they may reach 10,000 to 30,000 kw. For electric power demands exceeding approximately 300 kw the nuclear electric powerplant, including its appurtenances, is by far the heaviest portion of an electric rocket engine.

(*a*) *Essential Features of a Nuclear Electric Powerplant:* Figure 10 illustrates diagrammatically the essential features and the cycle conditions for the SNAP VIII* which is currently under development [22,23].

* SNAP—Systems for Nuclear Auxiliary Power.

In general, a nuclear turboelectric powerplant for space applications comprises three major components, in addition to its control system:

(1) A nuclear reactor which, by atomic fission, serves as a *high temperature source* of thermal energy.
(2) An *energy conversion unit* wherein a portion of the thermal energy supplied by the high temperature source (the nuclear reactor) is converted into the desired type of electric power.
(3) A *low temperature sink*, or *radiator*, which dissipates the non-useful thermal energy rejected to it by the energy conversion unit.

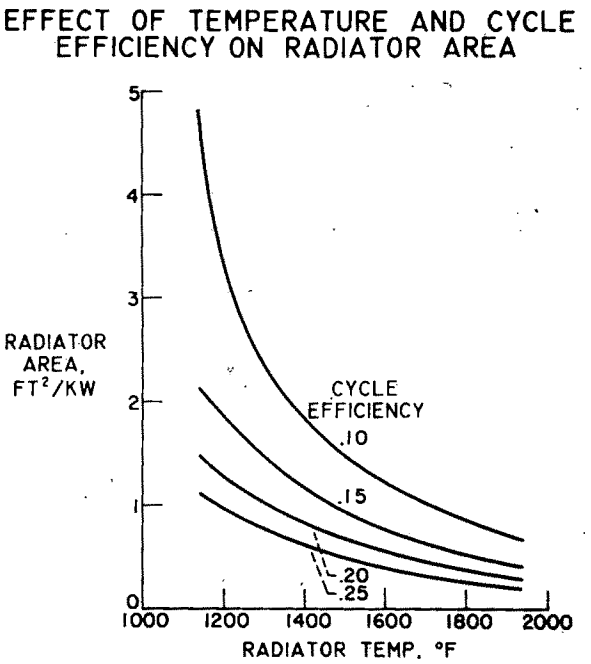


FIG. 11. Radiator area in (sq ft/kw) as a function of radiator temperature for different values of power cycle efficiency (Courtesy Reference 62).

The energy conversion unit may be a *direct conversion device* (thermionic or thermoelectric) or a *dynamic conversion device* (turboelectric generator). Studies indicate that, for nuclear electric powerplants which must operate at power levels exceeding a few hundred kilowatts, the dynamic conversion unit is the logical choice [22,26].

In a turboelectric power unit, thermal energy from the high temperature source is first converted into mechanical energy in some form of turbine which drives an electric generator. Since only a portion of the thermal energy supplied the turbine can be converted into useful work, the balance

is rejected to the radiator. Because of the absence of an atmosphere in space, the energy rejected to the radiator must be dissipated by thermal radiation. Since the rate at which heat can be rejected from the radiator is directly proportional to the fourth power of the absolute radiation temperature, the transfer of heat by radiation is effective only if the absolute radiation temperature, termed the *rejection temperature*, is relatively high.

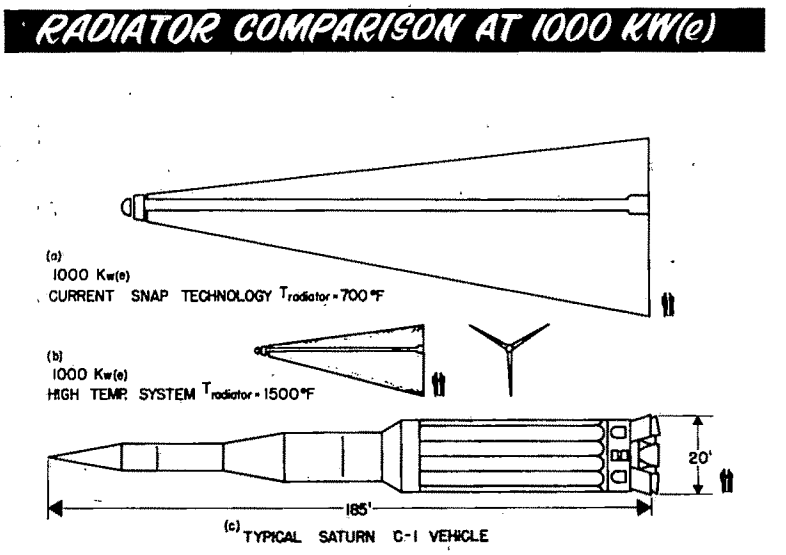


FIG. 12. Physical significance of raising the cycle temperatures (Taken from Reference 11).

(*b*) *Importance of High Cycle Temperatures:* The radiator of a nuclear electric powerplant for space application is in any case a large piece of apparatus, and it becomes a dominant factor in the designs of powerplants for power outputs exceeding a few hundred kilowatts. The dimensions of the radiator are important for the following reasons:

(1) It introduces problems of packaging the powerplant in the launch vehicle;
(2) Its size affects the dimensions and the weight of the radiation shield; and
(3) It must be protected from damaging collisions with micrometeorites.

To achieve the minimum powerplant weight and size for given high temperature source, the radiation temperature should be selected so that the radiator size will be a minimum[22,23,24].

Figure 11 presents the radiator area as a function of the heat rejection temperature, for different values of cycle efficiency [62].

Figure 12 illustrates the physical significance of raising the cycle temperatures for two 1000 kw nuclear turboelectric powerplants [22,23].

(c) *Powerplant Weight:* If one is optimistic and assumes that the nuclear turboelectric powerplants of the 10,000 to 20,000 kw capacity can be developed having specific weights of approximately 2 lb per kw, which is well beyond current technology, the accelerations which can be imparted to a spaceship will be smaller than 0.01 g. For that reason one may conclude the following regarding electric rocket engines:

(1) They are incapable of propelling a vehicle from the surface of Earth;
(2) They are useful only for small-acceleration missions, in space;
(3) They must operate continuously for long periods of time if large vehicle velocities are to be achieved;
(4) A spaceship propelled by such an engine is a single-stage vehicle; it cannot discard the engine; and
(5) They possess the advantage that the specific impulse can be varied by varying the electric power furnished the thrustor.

It is not unreasonable to state that the application of electric rocket engines to space propulsion will be paced almost entirely by the progress made in developing highly reliable, light weight, nuclear electric powerplants. The successful development of such a powerplant will require solving a large number of extremely difficult engineering problems. There are materials problems associated with using liquid metals such as lithium, rubidium, potassium, and the like, at the high temperatures involved (2000°F (1360°K)) or more. The development of a turbine which will operate efficiently and reliably for long periods of time, without attention, in an atmosphere of high temperature wet liquid-metal vapor is a most challenging problem [15,25,63].

General Characteristics of Rocket Engines

The relatively recent interest in electric rocket engines stems from the fact that chemical rocket technology is now so advanced that it is becoming feasible to place heavy payloads, such as a vehicle equipped with an electric rocket engine, into an Earth orbit. Consequently, a propulsion system which is virtually inoperable terrestrially, can be positioned in space where it can operate effectively because of the absence of aerodynamic drag and strong gravitational fields. That possibility was demonstrated by the first artificial Earth satellite. Consequently, by first placing a vehicle propelled by an electric rocket engine in an Earth orbit, the low thrust electric rocket engine can serve a useful purpose, since, under the conditions in space, an exceedingly small thrust applied to a vehicle will accelerate it to a large vehicle velocity if the operating time for the engine is sufficiently long. Mission

studies indicate that, for missions starting from low altitude orbits around Earth, power-limited (electric) rocket engines have a superior payload capability than energy-limited (chemical and nuclear) rocket engines. There are useful missions where the long flight times are acceptable [2,3,12].

Table III presents some of the general characteristics of the different types of rocket engines.

Of the rocket engines listed in Table III only the chemical types have attained practical realization. To date, no electric rocket engine has been operated in space so that the practicable feasibility of electric rocket engines is an unknown quantity. Undoubtedly, it will be more than a decade before manned flights in electric rocket-propelled-spacecraft will become feasible.

TABLE III

GENERAL CHARACTERISTICS OF DIFFERENT ROCKET ENGINE TYPES

Type of Engine	I_{sp}*, *sec*	*Thrust-to-Weight Ratio*	*Typical Duration of Powered Flight*
Chemical	200–420	10^{-2}–10^{2}	Minutes (high thrust) Hours (low thrust)
Nuclear	500–1000	10^{-2}–10	Minutes (high thrust) Hours (low thrust)
Electric			
Electrostatic (ion)	7,500–60,000	10^{-5}–10^{-3}	Months
Electrothermal (arc-jet)	700–2500	10^{-4}–10^{-2}	Days
Electromagnetic (MFM)	2,500–10,000	10^{-5}–10^{-3}	Weeks

* Vacuum expansion.

Based on G. P. Sutton, "Rocket Propulsion Systems for Interplanetary Flight," *Jour. Aero/Space Sci.*, October 1959.

Development Status of Nonchemical Rocket Engines

Because of space limitations most of the discussion will deal with the general characteristics, development status, and technical problems of the nonchemical rocket engines.

(*a*) *The Nuclear Heat-Transfer Rocket Engine:* The attractiveness of the nuclear-rocket engine stems from the enormous energy per unit mass obtainable from the fission of uranium; approximately 10^7 times that obtainable from the most energetic chemical propellants [36,37, 38,39,40,41,42,43].

A nuclear heat-transfer rocket engine equipped with a solid-core reactor capable of heating hydrogen gas to a temperature of 4000 R at the entrance to the exhaust nozzle will have a specific impulse of approximately 750 sec; that is, almost twice the best value obtainable from a chemical-rocket engine [35]. During 1959, a successful test was made of the Kiwi A reactor, as a part of the Rover project for demonstrating the

feasibility of the nuclear heat-transfer rocket engine. The information obtained from that test has been incorporated in the design of new reactors for checking design concepts, materials, and operating characteristics as a part of the NERVA program.*

The maximum operating temperature of a nuclear-rocket engine is limited by the structural integrity, at high temperatures, of the solid materials containing the fissioning uranium. Currently, the operating temperature of a solid-core reactor appears to be limited to approximately 5000 R. If the operating temperature could be raised to 7000 or 8000 R so that a hydrogen gas temperature of approximately 6000 R were attainable, the specific impulse would be raised to approximately 900 seconds.

Currently technology as evidenced by the NERVA program is directed to applying the information obtained from the KIWI reactor test program to a flyable NERVA engine. A large number of difficult scientific, engineering, and manufacturing problems must be solved to achieve that goal. Among them are those concerned with reducing reactor weight, developing a reactor core structure capable of withstanding stresses due to acceleration and thermal shock, fast neutronic start-up, control, destruction of reactor core after flight is completed, and achieving the extremely high degree of reliability so essential for a space propulsion engine.

(*b*) *Electric Rocket Engines:* Electric rocket engines are in their infancy, and most of the research and development work pertinent to them has been in connection with developing the thrustors of the different types of engines. As pointed out earlier in this presentation, the application of these thrustors to space exploration will be governed by two important considerations:

(1) The availability of nuclear electric powerplants of large power output capacity, having a low specific powerplant weight and capable of reliable continuous operation, without attendance, for long time periods, possibly a year or two.
(2) The availability of large booster engines, developing 5 to 20 millions of pounds of thrust, for placing the heavy payloads represented by electric rocket propelled vehicles into an Earth orbit.

1. The Electrothermal Rocket Engine: Figure 13 presents the essential features of a thermal arc-jet thrust chamber. An electric arc is used for heating a propellant, such as hydrogen, ammonia, or mixtures of hydrogen and lithium. The temperature inside the electric arc is estimated to be as high as 90,000 R while the temperature of the heated propellant will range from 5400 R to 22,000 R [47]. The heated propellant is ejected to the surroundings after expanding in a converging-diverging

* NERVA—Nuclear Engine for Rocket Vehicle Application.

exhaust nozzle. Hence, the jet velocity depends upon the portion of the enthalpy of the hot working fluid that is transformed into exhaust kinetic energy.

Experimental thermal arc-jet rocket engines using either hydrogen or ammonia as the propellants have been operated successfully for extended periods with gas temperatures of 3000°K, the specific impulse being 1000–1200 sec [25]. The merit of a propellant is determined not only by its available enthalpy at the design thrust chamber temperature, but also by its long time storability and capability for regeneratively cooling the exhaust nozzle and the electrodes [44,46,49].

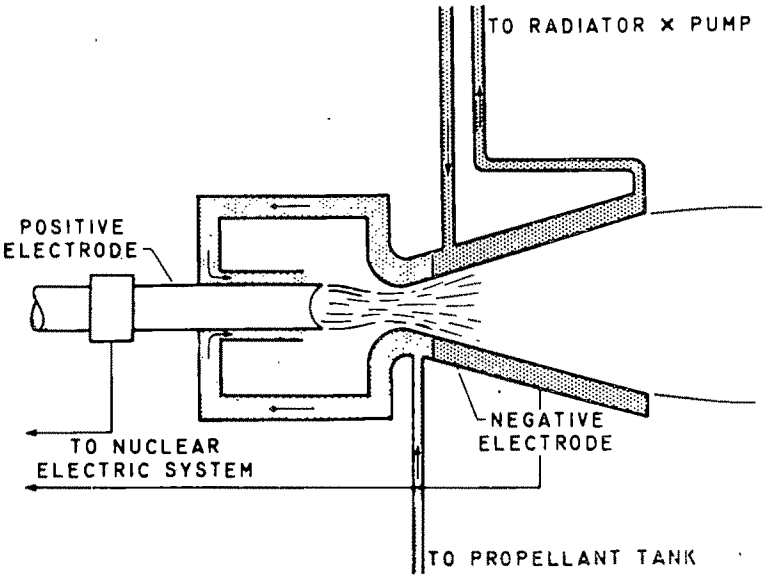


FIG. 13. Essential features of thermal arc-jet thrustor.

The electrothermal rocket receives considerable interest as a simple, practical propulsion unit for satellite orientation, orbit adjustment, and satellite raising missions [50]. Flight tests by NASA on a 1 kw engine are planned in 1962 [25].

(*2.*) *Electromagnetic Rocket Engines.* The thrustor for the *plasma* rocket engine is in the early stages of research and development. Its operation is based on accelerating a plasma, an ionized neutral gas, to a large ejection velocity by means of electromagnetic fields [51, 52, 53].

The thrustors employing electrodes use an arc-jet for supplying a partially ionized hot plasma (approximately 7000°K) to a flow channel in the thrustor. A magnetic and an electric field are impressed upon the channel so that they are perpendicular to each other and also to the flow

direction for the plasma. The resulting electromagnetic force then accelerates the plasma and it is ejected from the thrustor with a large jet velocity. In some cases the electrical conductivity of the plasma is increased by "seeding" the plasma with an alkali before it enters the electromagnetic field.

The type of accelerator just discussed is a steady state device, termed a *crossed-field accelerator.* Experimental devices have delivered specific impulses of 1400 sec or less, with thrusts varying from 0.3 to 3 lb, at power efficiencies ranging from 30 to 70 per cent [22]. The cross-field accelerator is inherently a simple method for producing and accelerating a highly conducting plasma.

The electromagnetic rocket engine has not fully emerged from the conceptual and feasibility stages, and a large number of schemes are being studied. Research conducted to date has not delineated with clarity which of the several approaches is the most promising.

Although electromagnetic rocket engines are still in an early stage of research and development, the results obtained to date indicate that they will perform well in the specific impulse range above 1200 sec, thereby filling the gap between the electrothermal rocket engines and the electrostatic rocket engines. In addition, in theory at least, the plasma-rocket engine is capable of providing a larger thrust per unit area than the ion rocket.

(*c*) *The Ion Rocket Engine:* The thrustor for the electrostatic or ion rocket engine has received more research and development attention than the thrustors for any of the other types of electric rocket engine. Theoretical predictions indicate that, in its present conceptual form, it will operate efficiently only at specific impulses exceeding approximately 2000 sec. As indicated in Table III, the ion rocket engine has the potential capability of achieving larger values of specific impulse than any other type of rocket engine without introducing severe heat transfer problems. Its thrust-to-weight ratios are, however, the smallest. Although the basic operating principle of an ion thrustor is relatively simple, a large number of complicated problems are as yet unsolved.

The propellant must be a readily ionized material and is fed to an *ionizer* wherein electrical power is used for producing positive ions, that is, atoms stripped of one or more electrons. The positive ions are separated from the electrons and flow through the *accelerator,* wherein they are accelerated by an electrostatic field established between the ionizer and *grid electrode,* called the *accelerating grid.* To produce a thrust the electrostatic field must act on a region where there is but one type of electric charge [59].

For no electric charge to be built up on the propelled vehicle there must be a balance between the number of positive charges (ions) and negative charges (electrons) ejected from the vehicle. A means must,

therefore, be provided for injecting electrons into the ion beam and neutralizing it [32, 37, 63].

The ion-rocket engine is usually considered for application to missions ranging from transfers between geocentric orbits to complex interplanetary missions requiring very large values of specific impulse. From a general standpoint, the different types of ion-rocket engines differ primarily in the means utilized for ionizing the propellant [25, 27, 30, 31, 33].

Currently, the neutralization of the ion beam appears to be the major and most controversial problem. There is no general agreement on the effectiveness of the techniques for accomplishing beam neutralization.

The question regarding whether or not neutralization of an ion beam can be achieved in the environment in space is not known for certainty, although recent laboratory experiments on Earth have stimulated confidence that beam neutralization can be achieved in space. Nevertheless, that question will not be resolved until an ion rocket engine is tested in space itself.

Regardless of the type of ion source, a high ionization efficiency is essential [55]. Otherwise, unionized (neutral) propellant will leave the ionizer and will not be accelerated. Moreover, there will be collisions between the neutral and the charged particles, with the result that the trajectories of the charged particles will deviate from their design values, causing some of them to impinge upon the electrodes [10, 56, 60].

If a high speed ion impinges on an electrode it may cause "sputtering," a term employed for describing the removal of atoms from a solid surface by high velocity particles impinging on the surface. Sputtering of electrodes by "wayward" ions from the ion beam erodes the electrodes, thereby reducing electrode life; a long electrode life is a prime requirement for missions requiring long powered-flight times. Consequently, the research for solving the problem of sputtering is extremely important.

One of the problems encountered in developing the ion rocket engine arises in connection with simulation on Earth of conditions in space. Since conditions in space cannot be simulated exactly on Earth, a method is needed for correlating simulated performance on Earth with actual performance in space.

Chemical Rocket Engines

Of the rocket engines discussed, only the liquid bipropellant and the solid propellant chemical rocket engines have achieved operational realization, and have a broad well-developed technology. Based on the experience gained with existing high-thrust chemical rocket engines, there is reasonable assurance that the development of new and larger thrust engines can proceed with confidence. Potentially, at least, chemical rocket engines can be developed for boosting any desired mass of payload into an Earth orbit, or to the escape velocity [16, 18].

Solid propellants in use today give a specific impulse of approximately 240 sec, but propellant research indicates that it may be possible to develop a solid propellant having a specific impulse of approximately 290 sec by about 1965 [57]. Experimental solid propellant rocket engines have been fired giving thrusts of 500,000 lb and 600,000 lb.

Table IV presents calculated values of the maximum specific impulse obtainable from different liquid high-energy propellants. Currently, the specific impulses of typical liquid bipropellant systems exceed those of the solid propellant systems. For the liquid systems, as indicated earlier, the maximum attainable specific impulse is approximately 400 sec.

In recent years there has been increased emphasis on the need for applying storable liquid propellants to existing ICBM engines. For example, the Titan II engine has been operated on storable propellants. In addition, larger thrust units are being developed. To illustrate, the Rocketdyne F-1 engine has been fired at over 1,000,000 lb thrust, and, in the first NASA firing (April 29, 1960) of all eight first stage rocket engines for the Saturn vehicle, a thrust of 1,300,000 lb was developed [58]. Considerable effort is currently being expended on design changes for increasing the reliability of liquid bipropellant rocket engines. It is interesting to note that "liquid rockets presently in service have demonstrated a reliability of approximately 90%" [58].

TABLE IV

PERFORMANCE OF HIGH-ENERGY CHEMICAL BIPROPELLANT SYSTEMS

Propellant System	*Oxidizer to Fuel Ratio*	*Specific Enthalpy, B/lb*	*Molecular Weight of Gases*	*Specific Impulse I, sec*	*Exhaust Velocity V_j, fps*	*Per Cent Converted Per Mole*
LOX–RPI	2.6	2,700	26.3	299	9,600	50.0
N_2O_4–N_2H_4	1.4	2,860	22.1	292	9,400	61.5
LOX–N_2H_4	0.9	3,390	21.7	315	10,150	66.5
F_2–N_2H_4	2.4	4,540	21.8	366	11,800	62.3
LOX–H_2	7.0	5,660	16.12	376	12,100	51.5
F_2–H_2	15.0	5,680	17.9	404	13,000	59.3

High specific impulse by itself is insufficient as a criterion for selecting a chemical propellant system. One must also consider (and this applies also to the propellants for electric rocket engines) certain important practical factors [6]. Consequently, the most appropriate propellant system is the one that satisfies the propulsion requirements with the lowest over-all cost, and also gives the rocket engine the greatest operating reliability.

Conclusion

We, as a nation, have embarked upon the study and exploration of space. It is no exaggeration to state that the scientific and engineering

problems to be overcome in achieving space exploration are of unusual difficulty. For example, we are confronted with the problems of fabricating components which must operate reliably for long periods of time, months and possibly years, under conditions not encountered on earth, and some of the conditions cannot be simulated and studied in laboratories on earth. A logical program of space exploration involves many different space flight missions, each having different propulsion requirements. Consequently, there are many types of rocket engines which must be studied, and practically all of the newer types are in the development stage. Since extremely high degree of reliability is an essential requirement for space flight engines, it will be several years before the newer and the more complicated types of engines can be developed into reliable operating hardware.

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THE UNIQUENESS OF PLANTS*

By A. G. NORMAN

IT IS AXIOMATIC that the growth and development of any organism is influenced by its environment. Less obviously, it is also true that the characteristics of organisms reflect the evolutionary pressures of their ordinary environments. A marine organism does not thrive in a freshwater lake; a polar bear would do poorly in the tropics. Plants have been and are confronted with environments that involve fluctuating conditions of light intensity, temperature, and moisture, superimposed on seasonal climatic patterns. If one examines the capabilities of higher plants in relation to their environment, one cannot but be impressed by certain features of their behavior and responsiveness that are unique to plants. These fall into two categories. The more obvious relate to their capacity for growth and to the acquisition of the raw materials and energy for cell synthesis. The less obvious relate to inductive mechanisms whereby a certain series of events is put in train, sometimes as a result of a sequence of happenings rather than a single incident. These involve both perceptive and control systems of great sophistication, not unlike some of the remarkable electronic devices made by man. It is my intention to review and interpret some of these unique characteristics of plants. What follows is in no sense a personal research report; it is a fractional distillation from the work of many men in many laboratories through many years.

If one attempted to define for an engineer the specifications of a chemical plant to carry out simultaneously all the things that the crop plant can do, one would soon run into difficulties, not the least of which would be the incredulity of the engineer that anyone could ask for such complicated interacting systems. One might be reasonable and specify as end products only those major plant constituents that are the primary products of the economic crops which we use, such as sugar in sugar beet, or starch and protein in cereal grains, or oil and protein in soybeans. However, these would have to be synthesized from carbon dioxide and water as raw materials with small quantities of nitrate and other inorganic salts. One would have to ask for some sensitive control mechanisms; some that would regulate the rate of starch, oil, or protein production and maintain the balance between them, others that would perceive light within a certain range of wavelengths, measure the length of the night and respond to changes therein, sense the trend in fluctuating temperatures in the environment and modify the synthetic operations accordingly, bringing them to a close at the proper season before the onset of cold weather, and "winterizing" the plant so that it would be put in "stand by" condition and not be damaged by severe frosts. The

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plant physiologist has cloaked the operation of these sensing and control mechanisms in words which have descriptive meaning to him, such as photosynthesis, biosynthesis, phototropism, photoperiodism, hardening, dormancy, etc. To a degree, however, the use of these plant physiological terms detracts from the full realization of the remarkable character of these responsive mechanisms, which, if expressed in physical or engineering terms, would be more impressive. We scientists often delude ourselves by believing that when we have coined a fine word to describe a new phenomenon, we have somehow explained it. The semantics of science are not always precise.

The Stuff of Plants

It is somewhat unfortunate that we have allowed the phrase "plant nutrients" to mean those inorganic elements that are essential for plant growth, because this causes us to forget the real substances from which the bulk of the stuff of plants is synthesized. The chemical engineer of whom we spoke earlier might be a little taken aback at being told that his only raw materials would be carbon dioxide and water, but perhaps not so much so as before the days of the polyethylenes and polypropylenes which are manufactured from relatively simple starting materials. If one added, however, that the carbon dioxide would be available only at a concentration of 3 parts in 10,000 by volume, diluted with an excess of nitrogen and oxygen, he might appear troubled, because to him that would mean the handling of vast quantities of air to extract the carbon dioxide needed. The plant can do this. Let us not underestimate the stupendous quantities of carbon dioxide that are incorporated into vegetation. Figures of the order of 1.5×10^{10} tons annually have been quoted for incorporation into terrestrial plants (excluding the oceans). Perhaps one-thirtieth to one-fiftieth of this may be accounted for by economic crops, or plants used by man. Now there may be considerable error in these estimates, but, even the last figure for crop plants exceeds several fold the yearly carbon dioxide output of all industrial operations on earth.

Perhaps it is easier to grasp figures based on an acre. To grow a good field of corn which will yield 100 bushels, about 20,000 pounds carbon dioxide is needed to provide about 5500 pounds carbon for the organic structures of the crop. During the growing season, therefore, the corn plants on one acre must deplete an enormous volume of air to meet their needs for carbon dioxide. No less than 21,000 tons of air are needed to supply 20,000 pounds of carbon dioxide. This is a startling calculation because it points up the astonishing ability of plants to do what an engineer might describe as "processing" these many tons of air to recover and utilize about two and one-half tons of carbon.

Some agronomists have been concerned that the carbon dioxide con-

tent of the ambient air in a heavy stand of a crop such as corn or cane might at times become sufficiently depleted to reduce growth rate. However, this could only happen in still air, which is a rare circumstance in the field. It has been established experimentally that increasing the carbon dioxide content of the air above normal under certain circumstances can result in an increase in carbon assimilation. There was at one time the suggestion that greenhouse atmospheres might profitably be enriched with carbon dioxide, but not much came of this because light intensity rather than carbon dioxide was thought to be more usually limiting to plant growth in greenhouses. Recently, this suggestion has been revived and a number of carbon dioxide installations injecting supplementary carbon dioxide have been installed for winter use in some commercial greenhouses raising specialty horticultural crops. On bright cold days, when the ventilators must remain closed, depletion of carbon dioxide may take place.

Although the generally accepted view is that all the carbon of plants is derived from atmospheric carbon dioxide through the process of photosynthesis, the possibility that some plant carbon may enter through the root system has occasionally been raised since the time of Liebig, when this issue was presumed to be settled. In the soil atmosphere, and because of the respiratory activities of soil organisms, the carbon dioxide content may at times be substantially higher than in the atmosphere above, though the oxygen content is reduced but little. One possibility is that the bicarbonate ion may enter roots as do other anions and then participate in some dark fixation reaction. Certain Soviet workers have studied the uptake of radioactive carbon dioxide ($C^{14}O_2$) and bicarbonate by roots and the subsequent appearance of C^{14}-tagged constituents in the plants. In some pot and field experiments with several crops they estimated that the amount of carbon dioxide absorbed by the roots may be as much as one-fourth of that assimilated through the leaves. They report field experiments in which crops treated with combinations of soluble carbonates and conventional fertilizers outyielded those with fertilizer only. Other investigators agree that there may be some dark fixation of carbon by roots but do not believe that carbonate fertilization is beneficial or that any significant part of the total carbon of plants is derived in this way. The biochemical reaction involved may be the carboxylation of phosphoenolpyruvate to give oxalacetate, which is a dynamic cell constituent. Roots, of course, rather actively respire, so that plant roots in soil may at the same time be a source of some of the carbon dioxide in the soil atmosphere and of bicarbonate in the immediate vicinity of root surfaces. There is the possibility here of a certain measure of cycling, perhaps similar to that which takes place in the leaves of succulents. Furthermore, there is abundant evidence that soluble organic compounds may enter plant roots and, if metabolizable,

may contribute to the substance of plants, though under ordinary circumstances not to any significant extent. It appears, therefore, that the textbook dogma about the source of carbon for plant growth needs some amplification.

The carbon assimilated by plants finds its way into a long list of chemical substances, some structural, some reserves, some of obvious importance in the economy of the plant, some of great usefulness to man, and some for which the role, if any, is quite obscure. The synthetic accomplishments of the green plant are impressively diverse. The elucidation of many of the pathways of synthesis and oxidative degradation in plants has been one of the great achievements of recent biochemistry, and with this has come recognition of the fact that there are many crossover points and alternative paths. This started with the recognition of the existence of diverse respiratory routes in microorganisms. It soon became apparent that some intermediates in the respiratory process are also intermediates in synthesis, and that the biochemical webs of synthesis and respiration are connected at several points. Realization of this has come about in part also from experiments involving radiocarbon in the form of carbon dioxide enriched with carbon 14. If green algae or higher plants are exposed to radioactive carbon dioxide ($C^{14}O_2$), the radioactivity can be followed into products originating from those molecules of CO_2. These are the studies for which Melvin Calvin received the Nobel prize for 1961 in chemistry. It was very early recognized by Calvin and others that these so-called "path of carbon" experiments had to be of surprisingly short duration if they were to give unambiguous information. In experiments of longer duration the radioactivity was found to be widely distributed in all types of compounds, because of these multiple crossover points in synthesis, and the fact that the basic building units are small and dynamic. It is now frequently the case that, in photosynthesis and transport experiments, the labeled carbon dioxide is supplied for only a few seconds and even then, when the tissue is instantly killed, radioactivity is detectable in several constituents.

At one time investigators were most anxious to identify the primary carbohydrate formed in photosynthesis, and a good deal of controversy surrounded this issue. If there is a definable primary product it is presumably phosphoglycerate, but this is quickly transformed into other compounds, so that it cannot be said that there is any accumulation of a primary product. Indeed, in the investigation of photosynthesis, views as to the nature of the key reaction have changed as new knowledge has been gained. For a while it was held that the photolysis of water providing the hydrogen to reduce the phosphoglycerate to form a triose was the vital step. Currently, some would vote for photophosphorylation, whereby light energy is converted into chemical energy in adenosine triphosphate (ATP) to supply the energy for the synthetic reactions.

Requirements for Water

Much of what has been said up to this point has related to carbon and to the synthesis of the substance of plants from carbon dioxide and water. Earlier, we spoke of water as a major plant nutrient, which it is, but the water actually utilized in photosynthesis together with that in the tissues of the plant form but an infinitesimally small fraction of the total water requirements. There is currently a good deal of interest in soil water and in soil-plant relationships, and a general recognition of the essentiality of an ample supply of water to the plant, but, even so, most of us would not see the water requirement in quite the same light as the engineer who is used to dealing with processes that require lots of water. He would not be surprised by the statement that, to meet the water needs of the same acre of corn, 4 to 5 million pounds of water might be needed, or in volume more than half a million gallons. When I quote such figures to students, I am always met by incredulity. They find it less shocking or more understandable, if expressed as 20 acre-inches.

A plant, therefore, may be regarded in engineering terms as a water-still or evaporator, and through every plant there passes in a season a very large volume of water. A single corn plant which weighs one pound at harvest time will have absorbed, transported, and transpired 300 to 400 pounds of water or 40 to 50 gallons.

Although all of us recognize the close relationship between water supply and plant growth, the large volumes of water passing through vegetation is less generally realized, and is another aspect of the uniqueness of plants.

At one time physiologists occupied themselves with determining the "consumptive use" of water by plants on the assumption that each species had a minimum requirement per unit of dry matter produced. Figures in the range of 200 to 900 were quoted, meaning that for every pound of dry matter 200 to 900 pounds of water passed through the plant. However, the current view is that the plant has only a minor role in determining the amount of water lost per unit of land area per season. The amount is largely determined by the supply and the energy balance at the earth's surface. Where water is ample, 85% of the net radiation may be dissipated in the vaporization of water. The meteorologist makes no distinction between water evaporated from the surface of the soil and water transpired by the leaves but groups them together as evapotranspiration.

Evapotranspiration = Evaporation (from soil) + Transpiration (from leaves)

Some indeed are inclined to the view that the nature of the plant cover makes very little difference to the total water lost by evapotranspiration, if the soil surface is moist and well covered by vegetation. This may be true for shallow-rooted annual plants, but is less likely when

one considers deep-rooted plants that may derive some part of their water from the lower horizons of the soil.

The Productivity of Plants

When one speaks of a plant growing, one has in mind the easily recognized features of increased size and weight. One can think of plant growth, however, as an example of the operation of compound interest because the growth increment in any period adds to the total capital available for subsequent growth. As leaves increase in size and number, they add to the photosynthetic capability of the plant. The compound interest law is not completely applicable because, as the plant grows, the root system also is enlarged, and while the larger root system may add to the ability of the plant to take up water and nutrients, only the photosynthetic areas can provide the raw materials for growth. One is really concerned with the daytime gain from photosynthesis, less the respiratory loss, which goes on both by day and by night, and which may amount to as much as one-third of the former. In assessing the growth potential and productivity of a plant it would be helpful to have quantitative information about the photosynthetic efficiency of leaves in terms of the net assimilation rate, which represents the balance available for growth.

Growth (weight increase) = Photosynthetic Input − Respiratory Loss

Crop physiologists have determined this indirectly by measuring the increase in dry weight per unit of time. The net assimilation rate

TABLE 1

REPRESENTATIVE VALUES FOR NET ASSIMILATION RATE

	g/m²/*week**				g/m²/*week**		
Cotton	55	42		Potato (Watson and Wilson, 1956)	47	43	46
Cucumber	34	41		Barley (Watson, Thorne, and French, 1958)	22	33	14
Tomato (Heath and Gregory, 1938)	61	47		Barley	40	23	
Barley	42	36		Potato	73	45	
Potato	34	75		Sugar beet	79	80	
Sugar beet	75	41		(Thorne, 1960)			
Wheat (Watson, 1946)	39	32					

* Units are g dry weight increase per m² photosynthetic surface per week.

therefore has such dimensions as 50 g per sq. in. leaf surface per week, or, with a shorter sampling period, 70 mg per sq. dm. per day. In Table 1 are given some net assimilation rates for major crop plants. In general these will be seen to be rather similar in magnitude, though for any particular crop there are trends with time that reflect the changing light and temperature conditions and changing condition of the leaves as the season progresses. The net assimilation rate is genetically controlled, but

varietal differences are apparently outweighed by response to nutritional status and immediate environmental factors.

The direct determination of net assimilation rates can be made non-destructively for relatively short sampling periods by gas analysis, but of course this involves inclosing either single leaves, the top of a plant, or the plants on a unit area which, in each case, may introduce a change in the environment.

TABLE 2

REPRESENTATIVE VALUES FOR LEAF AREA INDEX*

Barley	2.3
Wheat	2.0, 4.1
Potato	2.3
Sugar beet	4.3, 5.8

* Photosynthetic area per unit area of land.

To be meaningful in terms of productivity, or agricultural yield, which, except in the case of orchard trees, is always expressed on an area basis, it is necessary to combine the net assimilation rate term with a value which expresses the foliage area per unit field area. This has a value substantially greater than 1 for the larger, leafy crop plants, such as corn

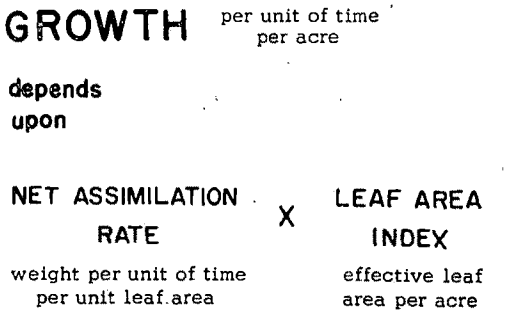


FIG. 1. Components of crop productivity.

or sugar beet (Table 2). In effect, this permits us to calculate the size of the photosynthetic production plant, and to say, for example, that the corn plants growing on one acre of land attain an effective leaf surface of 3.5 acres. At present, several groups are actively attempting to refine these techniques of growth analysis in order to be in a better position to recognize the components of yield and productivity. The goal of the agronomist is to maximize the productivity of the crop per unit area. He can examine the opportunities for improvement both in net assimila-

tion rate and in leaf area index, as combined in the statement shown in Figure 1. He may scrutinize the available germ-plasm for lines with significantly higher net assimilation rates, or having greater "leafiness." He may examine the management practices which affect leafiness and the length of time during which the leaves remain in prime photosynthetic condition. There will be other applications for these sophisticated techniques of growth analysis; some of the ecological problems of plant adaptation, plant succession, and climax communities may similarly be better understood when these quantitative methods of growth analysis are applied in their study.

Perceptive Mechanisms

Among living organisms, plants possess to a unique degree the capacity to respond to components of their environment, both climatic and nutritional. The light, temperature, and moisture regimes are always fluctuating, but may also be exhibiting a seasonal trend. Plants may respond to these changes in several ways. The *direct* effects that occur promptly are not difficult to understand. Changes in light intensity cause immediate changes in photosynthetic rate; changes in temperature cause immediate changes in respiratory rates. Parenthetically, one might mention that both ends of a plant are rarely at the same temperature and that, at times, there may be a substantial temperature differential between the leaves and the roots. However, it is the *indirect* or inductive responses that exhibit unique features. These may be set in train by a sequence of environmental happenings, such as the changing length of the day, the night temperatures, or exposure to low temperature. This means that plants contain perceptive mechanisms or sensing systems for these subtle components of the environment, and some biochemical means for translating the message into an action response.

Light in relation to plant growth presents the physiologist and biochemist with many intriguing problems. The essentiality of light energy in the photosynthetic process was easily recognized. It was more slowly realized that plants exhibit other responses to light, and to the relative lengths of the light and dark periods. Later, it developed that the same perceptive mechanism could lead to one of several different responses.

The time of flowering of many plant species, though not all, is determined by the day length, or more correctly by the length of the dark intervals (Fig. 2). Fall flowering species, such as chrysanthemums or asters, do so in response to the lengthening nights of late summer. These plants would fall on the left side of Figure 2. Many summer flowering species—the common summer annuals of our gardens—must have long days and short nights to flower and mature. They would fall on the right side of Figure 2. This response to the relative lengths of the day and night is known as photoperiodism. It is a genetic character, but ex-

perimentally the response can be invoked artificially by extending the dark period or shortening it by exposure to light of relatively low intensity but the correct spectral quality. It has been established recently that the induction of the photoperiodic response depends on a reversible photoreaction. The perceptive mechanism is a pigment, happily termed phytochrome, which exists in two interchangeable forms. One form absorbs red light in the vicinity of 6500 Å and is thereby converted into the other form which absorbs in the far-red or near infrared region of 7300 Å, and brings about the reverse reaction. This enables one exposure to

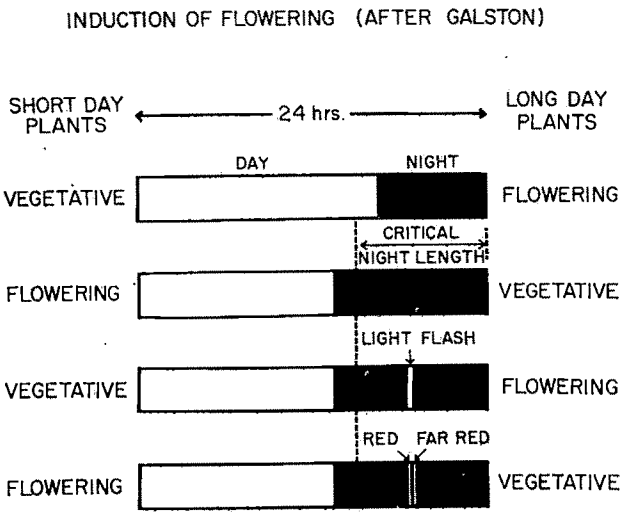


FIG. 2. Induction of flowering by photoperiodic responsive plants.

cancel the other experimentally—any response induced by exposure to red light (or white light containing red) can be nullified by prompt exposure to far-red light, or vice versa. However, in one direction this reaction also proceeds slowly in the dark. The far-red absorbing form (P730) slowly reverts to the red-absorbing form (P660) and it is this slow reversion which accounts for the ability of some plants to measure the length of the night. In the case of a plant such as the chrysanthemum which is induced to flower by a long night, interruption of the dark period by a brief flash of light or brief exposure to red light will prevent the flowering response. It is this type of experiment that tells us that it is the length of the night to which the plant responds rather than the length of the day. Interruption of the light period by an interval of darkness is without effect. At the end of the day the phytochrome is all in the far-red absorbing form and the slow reversion process starts. A light flash or red light exposure reconverts it all into the far-red absorbing form, and it is as though the night was starting all over again.

The same sensing system has been demonstrated to be present in certain seeds, such as lettuce which require light for germination. With all other conditions favorable such seed can be caused to germinate or prevented from germinating by alternate exposure to red or far-red light. The last exposure determines the response—if red the seed germinates, if far-red germination does not occur. The light exposures must follow each other fairly promptly for cancellation to occur. If delayed, some processes will have started that cannot be reversed.

The phytochrome system is also the perceptive mechanism that brings about changes in the growth of a developing seedling as its shoot emerges from the soil. Below ground in the dark there is great elongation and no opening of the leaf buds. Above ground the stem internodes are short and the leaves expand. It makes no difference at what depth a bean seed is planted, within reason, the shoot will elongate until the hook breaks the surface whereupon the hook opens and the leaves unfurl. This same response will follow exposure to red light; similarly it may be cancelled by prompt exposure to far-red.

The phytochrome system then is a sensing mechanism, responsive to light of particular spectral quality, triggering one of a number of responses—either flowering or the prevention of flowering, or the germination of some seeds, or shoot elongation and the expansion of leaves. The pigment phytochrome has not yet been isolated in pure condition. Its absorption spectrum is quite different from that of the chlorophylls. The far-red absorbing form presumably is active in initiating the response, perhaps as an essential component or coenzyme for the biochemical events that result in the response.

TABLE 3

SPECTRAL RANGES OF LIGHT RESPONSES BY PLANTS

	Range	*Peaks*
Photosynthesis (Chlorophylls)	4000–7300A	4400
Phototropism (Flavin ?)	3000–5000	4400
		4700
Photoperiodism (Phytochrome)	5500–7000	6500

Another light response familiar to all is the curvature developed by young plants or rapidly growing plant parts towards a unilateral light source. The wave lengths that induce this response are not identical with those involved either in photosynthesis or photoperiodism. The maximum is found in the blue at about 4500 Å with a minor peak at 4800 Å, but curvatures can be demonstrated in response to much shorter wave lenghts far into the violet. In this case, it is probable that the photoreceptive pigment is a flavin.

As one considers, then, the several light-induced responses of plants

it is clear that the spectral quality is important, and that the plant contains perceptive pigments responsive only to light of particular wave lengths (Table 3).

Another perceptive system present in plants, in this case not involving light, most remarkably detects the gravitational force. In whatever position a seed is placed in soil, or out of it in darkness, or in omnidirectional light, the root grows down and the shoot grows up. The response is a powerful one; it can be cancelled on a turntable or centrifuge. The main root or tap root may be strongly geotropic. Later roots and laterals may not be responsive. A spreading fibrous root system is indicative of a very feeble geotropic response. Organs other than the young root and shoot may assume a consistent position. The horizontal growth of rhizomes below ground or of runners on the surface involve a positional response. If their position is changed, the new growth will assume a near horizontal position. There is some recent evidence which indicates that leaf positions and the angle which the petiole makes with the stem may also be determined in part in reference to the perpendicular. Workers at North American Aviation recently showed that there was a linear relationship between the petiole angle of *Xanthium* and *g* forces from 0.2 *g* to 1 *g*. We have the situation therefore that some plant organs exhibit a strong geotropic response whereas others are neutral. Some compounds which result in disorientation are known, but these inhibitors may be affecting the response mechanism not the sensing system, about which little that is helpful has been discovered.

Other plant characteristics that have elements of uniqueness are manifest in the phenomena of hardening and dormancy and in the flowering of biennial species. In each of these the response follows exposure of the plant or plant part to low temperatures and in most situations several cold exposures rather than a single treatment are needed to induce the response. As yet, the physiological and biochemical explanations of these phenomena are most inadequate; there is no information as to the nature of the perceptive mechanism.

In the "winterizing" process, as a result of exposure to declining mean temperature, tissues which initially would have been injured by cold exposure will survive the severe frosts of winter. All gardeners are familiar with the losses that occur when a sharp frost follows unseasonably warm fall weather. In woody species, buds which form in one season will not open until they have been exposed to a sufficiently long cold period. Here the presence of inhibitors is postulated, accumulated during bud formation and slowly dissipated while growth is suspended, until, with the warm days of spring, there can be resumption of cell division. However, the tissues then have lost the resistance to cold injury which they possessed through the winter, and may be severely damaged by mild freezing temperature.

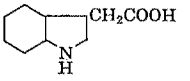
The seeds of many species exhibit a form of dormancy that can be broken only by cold exposure; in some there are even different requirements for root and shoot development. Then also there is the flowering stimulus for many biennials, which involves cold exposure. Certain minimum requirements have to be met if the plant is not to remain infinitely vegetative. Some northern latitude biennials will never flower if grown in the tropics. Time does not permit further discussion of these and other temperature-induced responses which are of great complexity and in some cases are confounded with light effects.

Control Mechanisms

For the complete expression of responses to the environment whether involving light or temperature, a response mechanism or control mechanism triggered by the former is needed. One of the most fascinating aspects of plant behavior centers around the internal hormonal systems that appear to regulate certain features of organ growth and development. These phytohormones or auxins seem to have a key role at every stage from seed germination to senescence. Sometimes their action is inhibitory, in the sense that a continuing supply of auxin from the

TABLE 4

NATIVE INDOLE AUXINS



Indole-3-Acetic Acid

Indole-3-acetaldehyde
Indole-3-acetonitrile
Ethyl indole-3-acetate
β-(Indole-3) propionic acid
γ-(Indole-3) butyric acid
Indole-3-pyruvic acid
Indole-3-pyruvaldehyde
Indole-3-glycollic acid
Indole-3-glyoxylic acid

rapidly growing tissues in the shoot prevents something from happening—the growth of axillary buds for example. Sometimes their action is stimulatory as in causing internode elongation. The compound indoleacetic acid (IAA) occupies a special place in the family of auxins which is now known to consist of a number of indolyl derivatives (Table 4). Indoleacetic acid is a compound producing unique responses in plants. Comparative biochemistry gives us no help at all, because even though the cellular metabolism of plant cells, animal cells, and bacterial cells possess many features in common, indoleacetic acid appears to be only of significance in plants, and then perhaps more at the organ level than the cellular level. Some investigators regard the indoleacetic acid family as being the ubiquitous and only growth-controlling system in higher plants. These compounds are present and operative at extremely

low concentrations, or alternatively are enormously active per unit weight. By a bioassay procedure it is easily possible to demonstrate the presence of 0.01 milligram IAA per liter.

When the activity of indoleacetic acid was recognized, chemists provided or synthesized many compounds with structural similarities, and somewhat surprisingly, many of these were found to bring about responses in plant tissues and organs resembling those invoked by indoleacetic acid. In some cases and in some responses there was even higher activity, and in others less. Many important horticultural applications of these auxin-like chemicals have been perfected, but the full potential of regulatory or control chemicals will only be realized when the biochemistry and physiology of the unique plant auxins is known.

TABLE 5

AUXIN-INVOLVED RESPONSES (AFTER GALSTON AND PURVES, 1960)

Auxin	Induces cell elongation
	Promotes root initiation
	Inhibits root elongation
	Delays leaf abscission
	Inhibits lateral buds
	Induces callus formation
	Promotes leaf epinasty
	Affects flower initiation
	Affects fruit set
	Causes morphological changes

Despite rather intensive studies in the past 20 years, the primary mechanism of action of auxins is not understood; worse than that, there are no very convincing theories. Some physiologists believe that, in young actively growing cells, a change in auxin concentration is followed by changes in the properties of the cell wall so that it is more extensible and, directly or indirectly, water entry is enhanced. Although this might account for the cell elongation response and various types of curvatures such as occur in response to light or gravity there must be other basic biochemical reactions that bring about multiple effects on growth and development. Certain of these are summarized in Table 5. Here then is a system controlling growth, operative in a number of different ways sometimes repressing, sometimes promoting, depending upon the organ involved.

Epilogue

In developing this theme of the uniqueness of plants, there are other examples which might have been given. But perhaps enough has been said in defense of the statement that plants indeed are uniquely responsive to components of their environment. Furthermore, man is still

heavily dependent on plants for subsistence. Although in this country there is an ample supply of food for all, this is not true on a global basis, and, with the rapidly growing world population, further improvement in the productivity of our economic crops will be vital. Scientific agriculture has made great gains in the last two decades, but the technology of agriculture is now advancing, not through empiricism, but by the application of principles uncovered in the basic sciences upon which it rests. It is essential to the future development of the agronomic and horticultural sciences that the growth processes and environmental responses of plants be explained and understood. This knowledge can then be utilized in the improvement of crops and in their efficient management. Real control is not possible without real understanding. It will, therefore, be a significant contribution to the welfare of man to have comprehension of the unique characteristics of plants.

STRUCTURAL REORGANIZATION THROUGH LIGAND INTERCHANGE*

By JOHN R. VAN WAZER

CHEMISTRY is the science which deals with the interactions between atoms, particularly those interactions which give molecules. For over a century, descriptive chemistry has been more or less arbitrarily divided into two categories: organic chemistry, which treats of the compounds of carbon, and inorganic chemistry, which treats of the compounds of the hundred-odd other elements. Organic chemistry involves millions of well-described different molecules; but not nearly as much is known about the chemistry of elements other than carbon. As can be seen from the data presented herein, this is not surprising since many molecules based on elements other than carbon very readily exchange parts (or "ligands") thereby undergoing structural reorganization, a knowledge of which is obviously important for the development of the chemistries of elements other than carbon.

The Temperature Range of Chemistry [1]

Let us consider chemistry from the viewpoint of temperature. At extremely high temperatures, there is no chemistry since even atoms are no longer stable. As the temperature is reduced to the range of several thousand degrees absolute, simple so-called spectrographic molecules (such as aluminum monoxide) begin to appear. Even this is not much chemistry and one must go to somewhat lower temperatures of one or two thousand degrees in order that thermal motion be sufficiently repressed so that reasonably large molecules may exist. At the very bottom of the temperature scale, molecules are stable but there is no dynamic chemistry since reaction rates become vanishingly slow.

The intermediate range of temperature—the region of chemistry—may be divided into two parts, the lower one of which I have called the region of "positional synthesis." In this lower region, there are a limited number of reaction paths or mechanisms, and chemical reactions are seldom carried to equilibrium. Essentially all of the reactions of organic chemistry are assumed to lie in this category. In the upper part of the temperature range of chemistry, many reaction paths are available and reactions generally proceed to equilibrium. This is the region of structural reorganization where, in the extreme case, everything and anything that is chemically possible happens. This means that an equilibrium is set up between all of the molecules which can result from exchange of parts between them. Since such is a thermodynamic equilibrium, reaction

* A Sigma Xi-RESA National Lecture, 1961–1962.

mechanisms have no significance with respect to the kinds and amounts of reaction products.

The region of structural reorganization might also be called the region of pyrolysis, using the common terminology of organic chemistry. For aliphatic hydrocarbons without catalysts, pyrolysis does not become appreciable until the temperature is raised to a range of 700–900° C., so that, for this system, the transition temperature between the region of positional synthesis and that of structural reorganization is high. On the other hand, the phosphorus analogs of the hydrocarbons—the phosphorus hydrides—undergo structural reorganization at temperatures as low as −75°C. This may also be considered as pyrolysis! Generally speaking, bonds to carbon are very slow to reorganize under normal circumstances; whereas, bonds to halogen atoms have the ability to make and break readily. Moreover, reorganization rates appear to be generally more rapid as one proceeds downward in a given column of the Periodic Table.

Reorganization of Simple Structures

A large number of names have been used to describe structural reorganization. In 1939 Calingaert used the term "redistribution reaction" to describe those processes whereby there was an essentially random interchange of organic ligands between organometallics, such as the tetraalkyl leads [2]. In 1942, Flory introduced the term "random reorganization" for the interchange of parts between macromolecules [3]. Likewise, rearrangement, disproportionation, exchange, instability in a neutral environment, interchange (especially of esters), displacement, scrambling, and thermal transformation have been used to describe structural reorganization. In addition, a number of reactions classified in the broad category of metathesis are of this type.

A very good way of studying ligand-interchange reactions is to combine two compounds—say, MX_n and MZ_n—under conditions where the X and Z atoms can exchange places so as to form all compounds of the type $MX_{n-i}Z_i$ where $i \leqslant n$. Such reactions must be carried out in a single-fluid phase. The single phase is necessary in order to avoid shifting the equilibria by phase separation according to the principle of Le Chatelier; and fluidity is required in order that the system remain homogeneous and that the reactions can proceed without hindrance by slow diffusion. When there are three sites per central atom M so that $n = 3$, four compounds result: MX_3, MX_2Z, MXZ_2, and MZ_3, with their relative amounts being dependent on the Z/M ratio or, equivalently, the proportions of X and Z. If there is no preference for particular combinations of X and Z, the relative amounts of these four compounds will be determined statistically in accord with random sorting of X and Z ligands.

Figure 1 shows some data obtained by Calingaert, *et al.*, nearly a quarter of a century ago [2]. In this study, a mixture of tetraethyl lead and tetramethyl lead was allowed to equilibrate at 80°C. for 5–7 hours in a solvent with a catalyst. Distillation of the reaction products showed

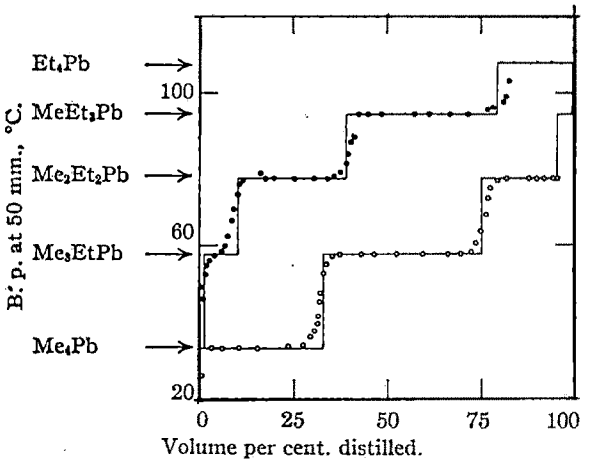


FIG. 1. Distillation of the reaction product from $Me_4Pb + Et_4Pb$. Solid line calculated for random equilibrium mixture having O $R \equiv$ Me/Pb = 0.776, ● R = 0.345.

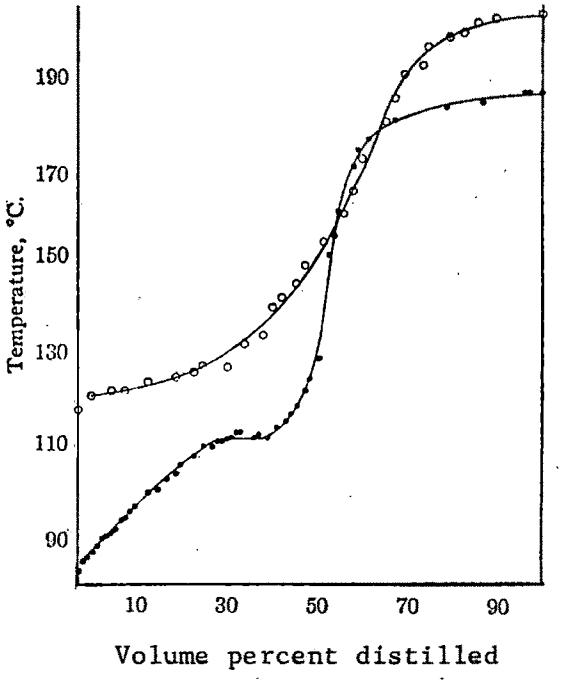


FIG. 2. Distillation curves of germanium (●) and stannic (O) chlorobromides.

that all three mixed methyl-ethyl leads were formed and appeared in the amounts expected for random interchange of the methyl and ethyl groups. In many cases, however, distillation is not suitable for studying structural reorganization, as shown in Figure 2, which gives distillation

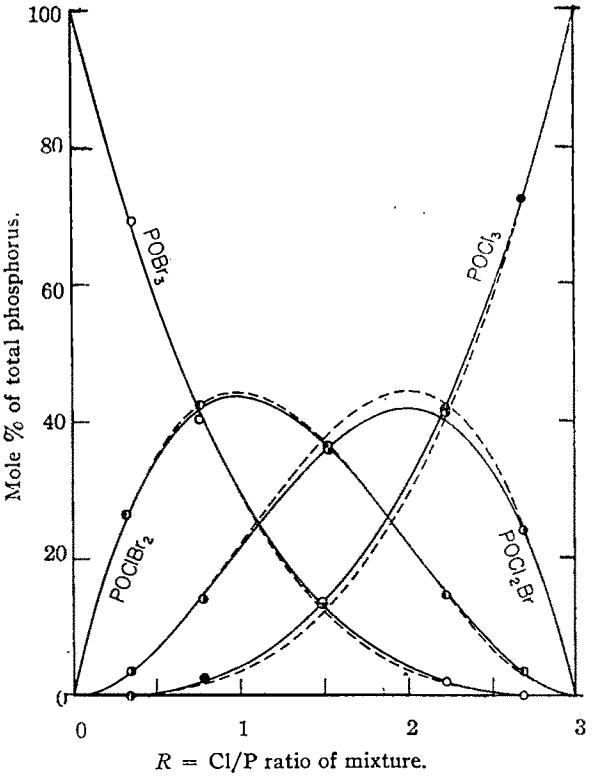


FIG. 3. Equilibria of $POCl_3$-$POBr_3$ randomly reorganized mixtures. The solid lines correspond to $K_1 = 0.406$ and $K_2 = 0.341$; the dotted lines correspond to the completely random case in which $K_1 = K_2 = 0.333$.

curves for mixtures of germanium or tin chloride with the respective bromide [4]. In the case of the germanium halides, there is evidence of one mixed species. However, the distillation curve for the tin halides gives no indication of the formation of mixed stannic chlorobromides. Spectral data have since shown that three mixed chlorobromides exist for both germanium and tin [5]. The high temperatures involved in distillation caused continuous reorganization so that the lowest boiling compound (the tetrachloride) was formed as quickly as it could be distilled off. This resulted in complete decomposition of the mixed halides so that, with good rectification, all of the chlorine went into the distillate as the tin or germanium tetrachloride.

The results given in Figure 2 indicate that relatively gentle experimental procedures must be employed in separating many reorganizing mixtures or that, better still, the amounts of the various species ought to be detected in unseparated mixtures. Nuclear magnetic resonance (n.m.r.) offers a good technique for doing the latter, as shown by the data of Figure 3 taken from P^{31} spectra [6]. In this figure, the relative amounts of the four species $POBr_3$, $POClBr_2$, $POCl_2Br$, and $POCl_3$

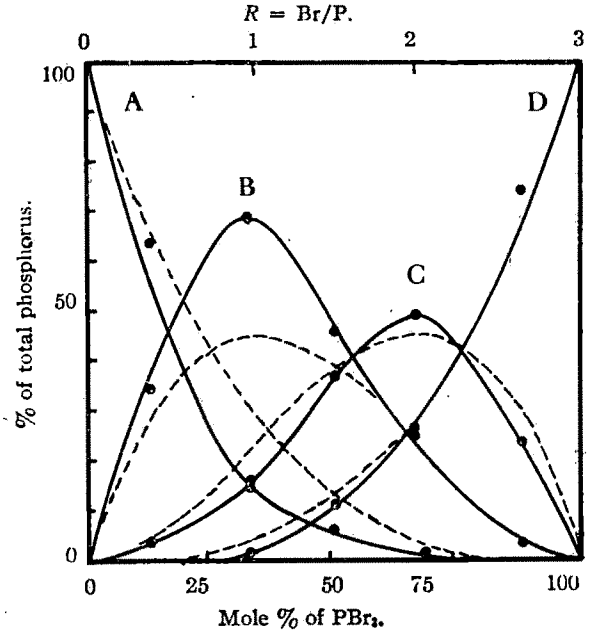


FIG. 4. Equilibrium in the system $P(OC_6H_5)_3$-PBr_3 at 180°. The dotted curves represent the equilibrium values to be expected if the reorganization were completely random: A = $P(OC_6H_5)_3$; B = $PBr(OC_6H_5)_2$; C = $PBr_2(OC_6H_5)$; and D = PBr_3.

obtained from various mixtures of the two end members are compared with the amounts to be expected if the chlorines and bromines redistributed in a completely random manner. The experimental results are very close to the ideal random case.

A nonrandom example is presented in Figure 4 [7]. Since Gibbs free energies are proportional to the logarithm of the respective equilibrium constants, the deviation from randomness can be discussed in terms of the free-energy difference between the experimental findings and ideal random sorting. For the equilibrium between compound B in Figure 4 and the two end-member species, A and D, the deviation from randomness corresponds to a free-energy difference of 0.6 kcal.

Figure 4 corresponds to the distribution curves to be found along the bottom axis of Figure 5, from which it can be seen that ten compounds result from mixing triphenyl phosphite, $P(O\Phi)_3$, with phosphorus tribromide, PBr_3, and phosphorus trichloride, PCl_3. The n.m.r. trace (see Fig. 6) obtained from an equimolar mixture of these three end-member compounds shows nine peaks corresponding to the ten species depicted in Figure 5 [7]. Although the resonances for PBr_3 and $PClBr_2$ were not completely resolved in Figure 6, their amounts could be determined from the composition of the mixture and the relative areas of all nine resonance peaks. In a properly taken P^{31} n.m.r. trace with no magnetic saturation, the areas under the resonance peaks correspond exactly to the amounts of phosphorus in the respective chemical species.

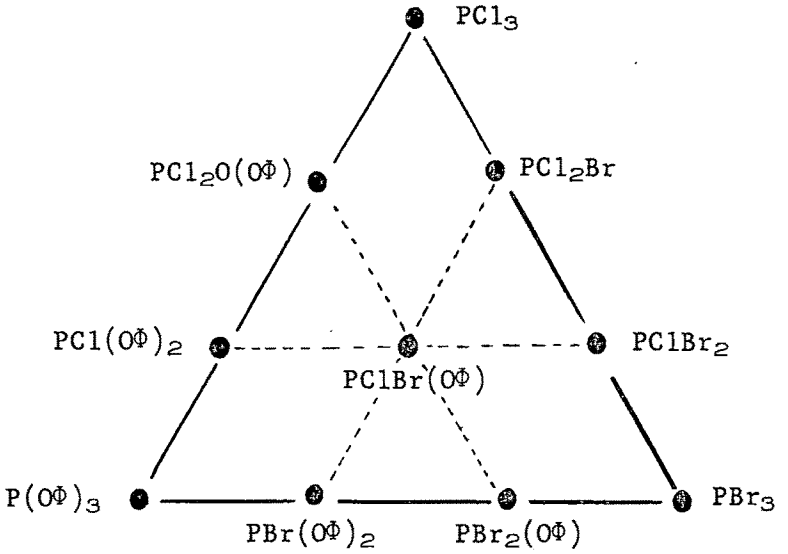


FIG 5. The compounds in the system $P(OC_6H_5)_3$-PCl_3-PBr_3.

Although it has previously been pointed out in this article that reorganization of hydrocarbons generally occurs at a very high temperature, there are a few special cases where such reorganization takes place at much lower temperatures. One such special case is the redistribution near room temperature of hydrogen atoms and alkyl groups on benzene rings, as catalyzed by boron trifluoride [8]. The equilibrium corresponding to this type of redistribution is shown in Figure 7 [9]. It should be noted in Figure 7 that the dimethyl and tetramethyl benzenes are present in larger amounts than would be expected for random reorganization of H and CH_3 groups.

The data of Figure 8 have two key features. The first is that $N(CH_3)_2$ and chlorine atoms undergo rapid exchange at room temperature where

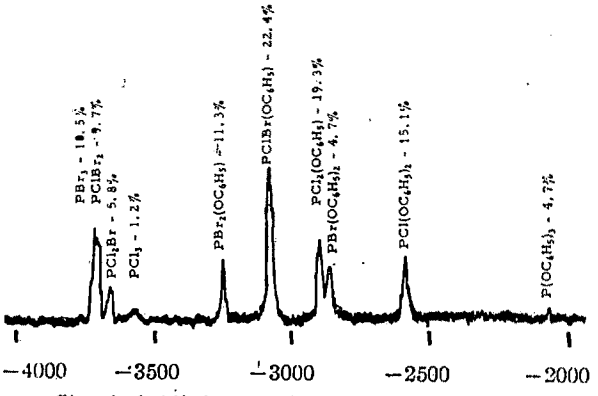


FIG. 6. P^{31} nuclear-magnetic-resonance trace for an equimolar mixture of PCl_3, PBr_3, and $P(OC_6H_5)_3$, equilibrated at 180°. Values following the chemical formulas are mole percentages.

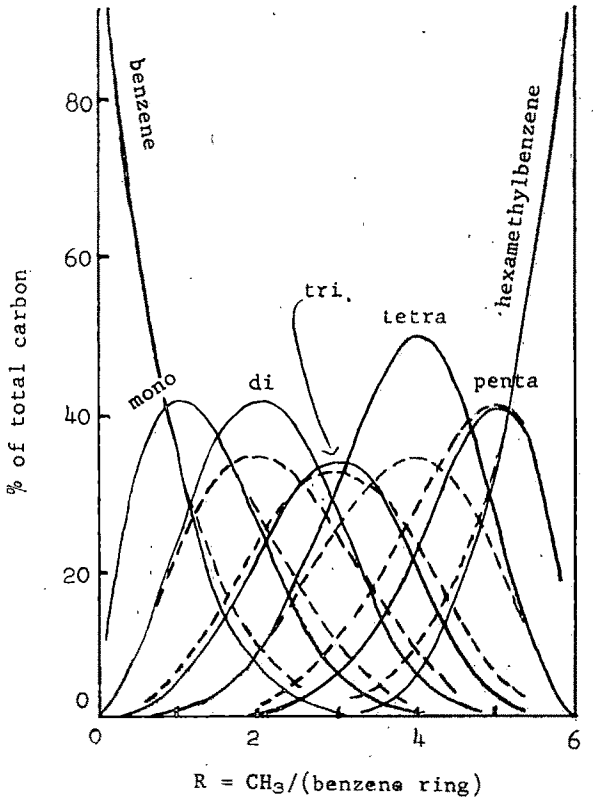


FIG. 7. Equilibrium concentration of methylbenzenes in ideal gas state at 1000°K. as calculated from thermodynamic data. The random case is in dotted lines.

the carbon-phosphorus bond is completely stable. However, by heating the resulting equilibrium mixtures up to several hundred degrees C, a new equilibrium involving making and breaking of the carbon-phosphorus bond is achieved. At these higher temperatures, not only the three species shown in Figure 8 are to be found, but all of the species (such as

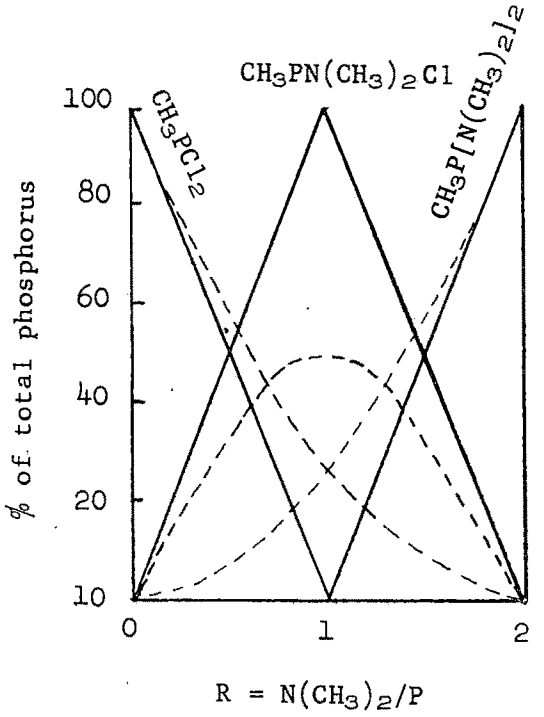


FIG. 8. Exchange between methyldichlorophosphine and methylbisdimethylaminophosphine.[10]

$(CH_3)_3P$ and $(CH_3)_2PCl$) obtained by redistribution of the methyl groups also appear. The second point of interest in Figure 8 is the fact that the intermediate species is strongly preferred. The deviation from ideality in this case is *ca.* 9 kcal./mole. From the thermodynamic equation

$$\Delta F = \Delta H - T\Delta S \tag{1}$$

and the fact that entropy changes are never extreme, it is apparent that large deviations from random behavior must be attributed to the ΔH term—the heat of the reaction, and this is found to be true for the example at hand. For random reorganization, $\Delta H = 0$ and the only contribution to the free energy is the entropy of mixing.

In most cases, it appears that deviations from randomness in reorganization afford a measure of the amount of rehybridization of the central atom M induced by substituting one ligand for another. This

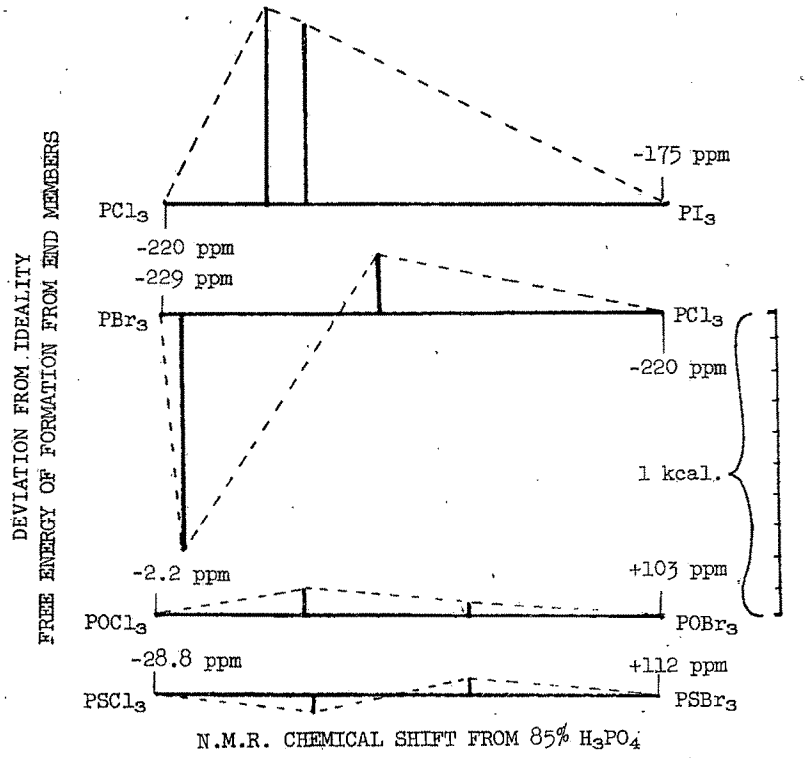


FIG. 9. Comparison of the nonideality of redistribution equilibria with the spacing of P^{31} n.m.r. resonances for several systems of reorganizing phosphorus compounds.

is in accord with our belief that physical properties such as index of refraction, density, and even the n.m.r. chemical shift generally change in a regular manner when going from one compound to another in a series of molecules resulting from random reorganization but that, when the reorganization is nonrandom, these properties often vary in a nonlinear manner. Examples of linear and nonlinear behavior are shown in Figure 9. In this figure, it should be noted that the n.m.r. chemical shifts are linear when the deviation from ideal randomness measured by the free energy of formation from the end members is small. However, in the case of nonrandom reorganization equilibria (large free-energy deviations), the resonances tend to bunch up at one end or the other; or, in the extreme case, the n.m.r. resonance of intermediate species may not even lie between those of the end members of the series of compounds!

Exchange of Parts Between More Complex Molecules

Thus far, we have discussed the less important aspects of reorganization chemistry. Now we shall get to the seat of the matter! Let us again consider the system where we have a central atom M exhibiting three

ligand sites. At reorganization equilibrium, there will be the four compounds: MX_3, MX_2Z, MXZ_2, and MZ_3. Now, if we let X be a monofunctional ligand such as the halogen atom and Z be a bond to another M atom, we find that MX_3 is a simple compound which we will call the *neso* molecule. Moreover, MX_2Z has the form X_2M- and is an end group in a molecule, while MXZ_2 and MZ_3 are middle, $-XM<$, and branch $M<$, groups respectively. Obviously, the ends, middles, and branches have no separate existence but must appear as parts of molecular structures. Now we are faced with a second type of randomness. Not only may there be random sorting of X and Z ligands, but consideration must also be given to random or nonrandom sorting of the resulting ends, middles, and branches into molecular structures [1,11]. Although the mathematics is not identical, the sorting of, say, middle groups between end groups is somewhat similar to sorting of a large number of books between many pairs of book ends. If the latter sorting process is carried out randomly, there will be an occasional pair of book ends with no books between them and some other pairs with very many books between them, so that there is a size distribution the average of which is determined by the relative proportion of books to book ends.

It is now necessary to introduce the concept of infinite network molecules. If one is sorting a mixture of the various kinds of structural building units and there are a large number of branch groups, the building of a given molecule will never be completed if there are not sufficient ends to cap off all of the branches in a ring-free structure. The general equation for the presence of infinite-network ring-free molecules is given below [1].

$$\sum_{n=0}^{\infty} (n-2)_n b_n > 0 \tag{2}$$

where b_n is the amount of the structure unit of functionality n.

For the *neso* molecule, $n = 0$; for ends, $n = 1$; for middles, $n = 2$; and for a threeway branch, $n = 3$; etc. It is apparent from this equation that the *neso* molecules and middle groups have no effect on the formation of an infinite network. In the case of *neso;* ends, *e;* middles, *m;* threeway branches, b_3; and fourway branches, b_4; equation 2 reduces to the form:

$$3\,b_3 + 8\,b_4 > e \tag{3}$$

Equation 3 states that it takes, on the average, three ends to cap off a threeway branch and eight ends to cap off a fourway branch so as to prevent the formation of an infinite network or, as it is sometimes otherwise called, a wall-to-wall molecular structure.

TABLE I

MATHEMATICS OF STRUCTURAL REORGANIZATION
ON THE IBM-704 COMPUTER

Input	(1)	$o/e/m/b_3/b_4$
	(2)	% of total units in rings of size 2 to 20
Output	(1)	No. av. mol. size (with and excluding neso)
General	(2)	Av. no connections/mol.
data	(3)	% of mols. as straight chains
	(4)	No. av. mol. size of straight chains
	(5)	% of total units in straight chains
	(6)	Extent of gelation (with and without rings)
Tabulations	(7)	Total molecules (sizes: 1 to 1000) when no rings
	(8)	Straight chains (sizes: 2 to 50)
	(9)	Simple rings (sizes: 2 to 20)
	(10)	Branched, small molecules (sizes: 4 to 8) when no rings

It is obvious from equation 2 that the higher the functionality of a given branch group, the greater the number of ends are needed to keep the molecules finite. Thus, for example, for a sixway branch, twenty-four or more ends are needed to avoid the formation of a wall-to-wall molecular structure, assuming no rings.

The effect of rings containing branches is obviously to reduce the need for end groups to avoid infinite molecules. For example, if two

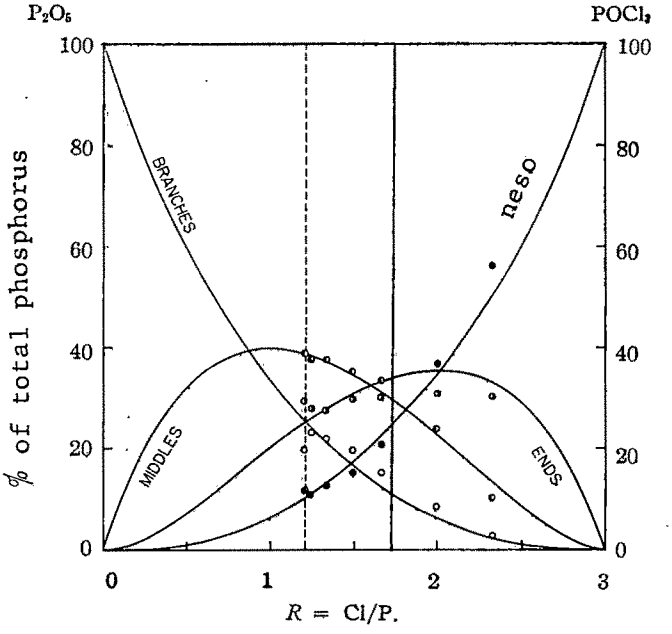


FIG. 10. Distribution of structure units as a function of composition in the reorganized system $POCl_3$-P_2O_5. The dotted line corresponds to the observed gel point and the solid line to the theoretical value based on no rings.

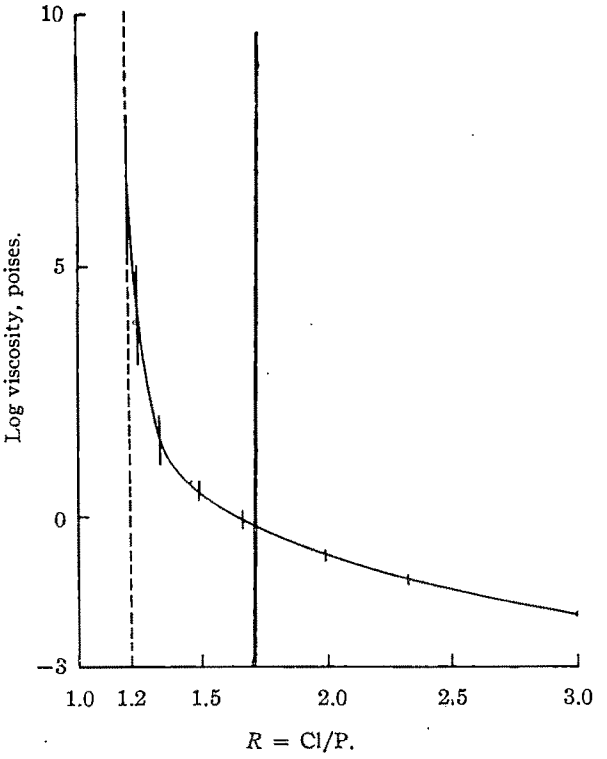


FIG. 11. Viscosity of reorganized phosphoryl chloride compositions at 25°.

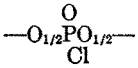
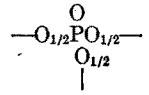
closely located branch groups in a molecule connect together so as to form a ring, there is no need to terminate chains originating at these branches by end groups. In the extreme case, one may have all branches and yet end up with relatively low-molecular-weight species by putting the branches together in the form of cagelike molecules. However, the random probability for such cages is quite small.

We have set up the mathematics of structural reorganization on an IBM-704 computer, as shown in Table I. This has proved especially valuable since the computation of the random case corresponding to various ratios of $o/e/m/b_3/b_4$ is very time consuming and we need to know the random distribution of molecules in order to see whether the experimentally found distribution is random or not.

Some Phosphorus-Based Reorganized Families of Molecules

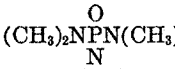
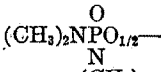
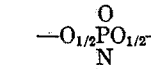
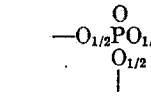
An interesting example of structural reorganization involving molecules is found in the polyphosphoryl chlorides [12]. In this case, we have the series of compounds: OPX_3, OPX_2Z, $OPXZ_2$, and OPZ_3, where X = −Cl and Z = −O− (a bridging oxygen). This means that our

building units consist of the well-known orthophosphoryl chloride, $OPCl_3$ (the *neso* molecule), and the following parts of molecules:

			
neso	end	middle	branch

Structural reorganization in this system leads to the equilibrium shown in Figure 10, which is obviously not far from the random case. If there were no rings in this system, infinite-network molecules would have been found for Cl/P ratios smaller than 1.7, as indicated by the solid line in Figure 10. Instead, an infinite network is not detected until the Cl/P ratio is reduced to around 1.1. This difference is attributed to the presence of rings. The value of 1.1 is obtained from the room-temperature viscosity of these reorganized phosphoryl chloride compositions as shown in Figure 11. The transition from the region of finite molecules to the region where a wall-to-wall structure is found has been called the gel point. As indicated by the sharp upturn in viscosity for Cl/P mole ratios near 1.2, this is a very apt name.

Another system of interest is the polyphosphoryldimethylamides, the structure units of which are shown below:

			
neso	end	middle	branch

Equilibrium between structure-building units for this system is shown in Figure 12 [13]. In this figure, we see that the ends and middles are present to much greater amounts than would be expected for random sorting of bridging oxygens and $-N(CH_3)_2$ groups. This means that the chemistry of the polyphosphoryl chlorides is quite different from that of the polyphosphoryldimethylamides.

As can be seen in Figure 10, there are an appreciable number of branches even for relatively large values of the Cl/P mole ratio. Thus, polyphosphoryl chloride compositions must exhibit a number of branched or *iso* structures, even among the smaller molecules. Furthermore, because of the fact that there are roughly only about twice as many middles as branches below the gel point, the larger molecules should be highly branched. On the other hand, as shown in Figure 12, the composition corresponding to $(CH_3)_2N/P$ mole ratios between 2.8 and 3.0 consists of simple mixtures of the *neso* compound, with the second member of the polyphosphoryldimethylamide family consisting of two ends bonded to each other. By the way, this second member is the well-known insecticide, octamethylpyrophosphorylamide or OMPA. In the composition region for $(CH_3)_2N/P$ between 1.4 and 1.8, there is

only a mixture of *neso* and straight-chain molecules. Then, in the region from $(CH_3)_2N/P = 1.15$ to 1.40, there is essentially only a distribution of straight chains, with these being of considerable length at the composition corresponding to $(CH_3)_2N/P = 1.15$.

For the polyphosphoryldimethylamides, the observed gel point is very near that predicted for no branch-containing rings. After all, with so few branches, such rings are not highly probable in this system. It should be noted in Figure 12, that the viscosity becomes appreciable and, finally, the material exhibits elastic properties in the region of the

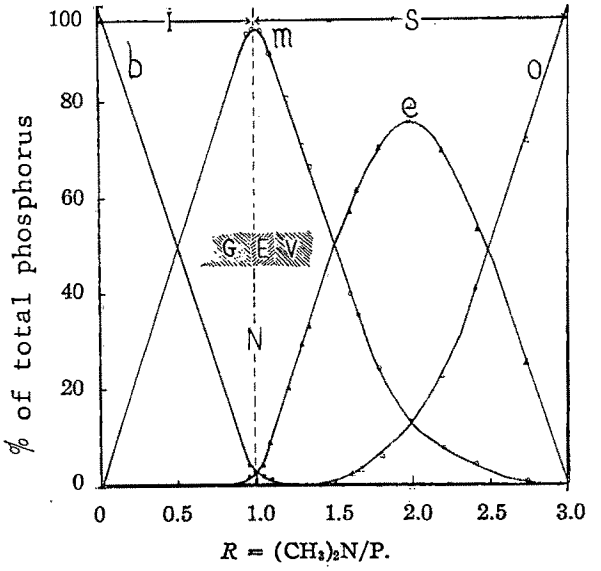


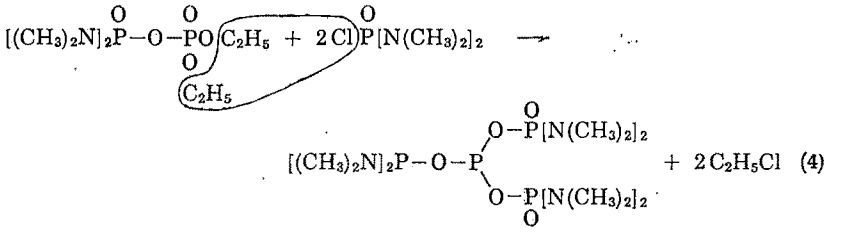
FIG. 12. Distribution of structure units as a function of composition in the reorganized system $PO[N(CH_3)_2]_3$-P_2O_5. The *neso* molecule is represented by *o*, the end group by *e*, the middle group by *m*, and the branch group by *b*. Solubility in hot CCl_4 is denoted by *S* and insolubility by *I*. The gel point is at *N* and *V* stands for the region of viscous liquids; *E*, elastomeric material, and *G*, glassy material.

gel point. Below the gel point, the compositions are found to be soluble in hot carbon tetrachloride; whereas, they are insoluble above the gel point. A little beyond the gel point, the systems become glasslike because of the crosslinking due to the branches.

It should be noted that the preparation of moderately long straight chains of polyphosphoryldimethylamides as well as the formation of elastomeric compositions was not the result of chemical syntheses based on *a priori* rationalizations. Rather, these results were obtained willy-nilly because of the reorganization equilibria.

An example of the misapplication to the temperature region of random

reorganization of thinking appropriate to the region of positional synthesis is shown by the following equation [14]:



$$[(CH_3)_2N]_2P{-}O{-}PO(C_2H_5)_2 + 2\,ClP[N(CH_3)_2]_2 \longrightarrow [(CH_3)_2N]_2P{-}O{-}P\left(O{-}P[N(CH_3)_2]_2\right)_2 + 2\,C_2H_5Cl \quad (4)$$

By obtaining essentially a 100% yield, the author of the paper describing equation 4 believed he had proved the existence of an isotetraphosphoryldimethylamide. For such a structure, 75% of the

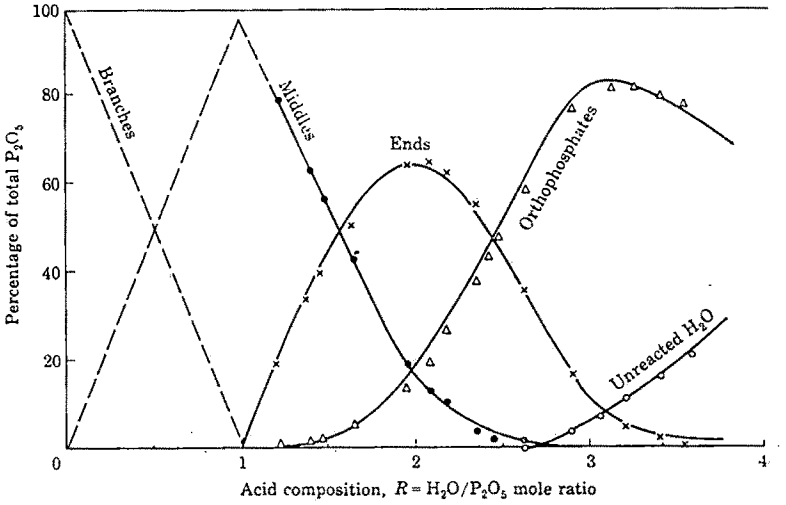


FIG. 13. Distribution of structural units as a function of composition for the strong phosphoric acids. Solid curves calculated for $K_1 = 10^{-4}$, $K_2 = 0.022$, and $K_3 = 0.020$.

phosphorus should be present as ends, with the remaining 25% as branch groups. Our P^{31} n.m.r. study of this reaction product showed it to consist of the equilibrium mixture of 1% *neso*, 48% ends, and 51% middles, with no detectable branches! It is quite clear that, during the normal laboratory operations involved in carrying out the reaction shown as equation 4, complete structural reorganization took place. This type of misunderstanding is often found in the chemistry of elements other than carbon. For such compounds, one must be cautious in trying to deduce structures from chemical reactions, using procedures which would be highly acceptable in classical organic chemistry.

Molecular Size Distributions

As shown in Figure 13, there is an equilibrium between *neso*, ends middles, and branches in the condensed phosphoric acids [15]. Molecular size-distribution curves corresponding to two cuts through the curve of Figure 13 are shown in Figure 14. It should be noted in the two curves of Figure 14 that the agreement between the experimental data and random sorting of the ends, middles, and branches is good.

A very different situation is found in the sodium phosphates where the ends and middles are present to the maximum possible amount, as shown in Figure 15, and here we are faced with a third type of random behavior [11,15]. In this case, the random distribution of charge leads to predominance of the middle and end groups. Let us, for example,

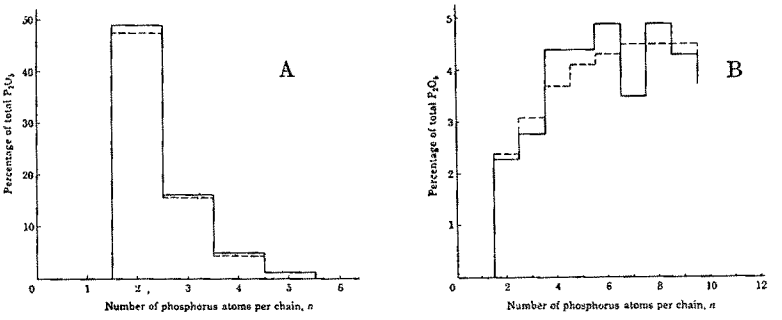


FIG. 14. A shows the size distribution of chain phosphates in a condensed phosphoric acid for which $R = 2.192$ corresponding to 78.3% P_2O_5, and B the size distribution of chain phosphates in a condensed phosphoric acid for which $R = 1.286$ corresponding to 86.0% P_2O_5.

take the case for an Na_2O/P_2O_5 mole ratio of 2. One could write the equation for ends, *e*, forming middles, *m*, and *neso* molecules, *o*, as follows:

$$2e^{--} = m^{-} + o^{---} \tag{5}$$

Likewise, branches can be included by means of an equation such as the following:

$$3e^{--} = b^{0} + 2o^{---} \tag{6}$$

Since detailed distribution of charge at an Na_2O/P_2O_5 mole ratio of 2 is best satisfied by having all PO_4 groups with the same charge, we conclude that equations 5 and 6 should be driven to the left. Likewise, we can consider the Na^+ counter ions and show that, unless equations 5 and 6 are driven to the left, the sodiums will not achieve a uniform microscopic distribution [16].

In Figure 16, an experimental size distribution corresponding to a cut through the curve of Figure 15 at $Na_2O/P_2O_5 = 1.667$ is shown as a

thin solid line [15]. This distribution which exhibits, for this composition, a maximum at the triphosphate ion did not agree at all with the continuously decreasing distribution curve calculated for ideally random sorting of ends and middles (the heavy black line dubbed the "flexible-chain model") in Figure 16. Therefore, we argued that a fourth type of randomness would be needed to fit the data. Since phosphate glasses are high-charge polyelectrolytes, it is not at all unreasonable to demand that the constituent chain anions assume a rodlike configuration because of the mutual repulsion of the distributed negative charges along their length. In this case, we derived a distribution function in which, simultaneous to the sorting of ends and middles, it was required that each group of anions of size n exhibit a random distribution with respect to

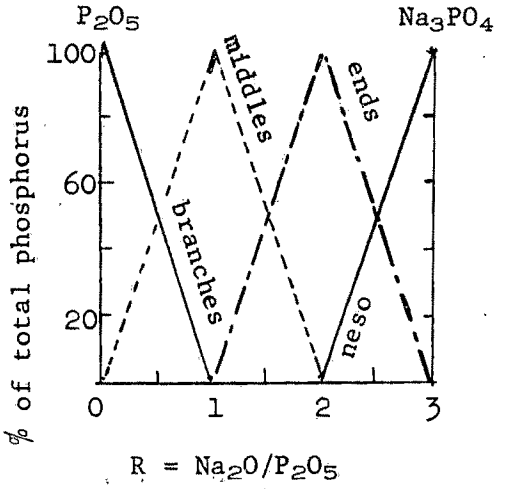


FIG. 15. Distribution of structure units as a function of composition in the reorganized system Na_3PO_4-P_2O_5.

direction in space, in other words, that there be as many rods of a given size pointing in one direction as in any other direction [11]. This led to the distribution called the "rigid-rod model" shown by the checked heavy line in Figure 16. By the way, it should be noted here that the random size distribution calculated with no chain orientations assumed is mathematically equivalent to having made the assumption of all kinds of chain orientations and arrangements; hence, the name "flexible-chain model" for the previously discussed case of random sorting.

Although the fit between the rigid-rod model and the experimental data for the sodium phosphate glasses was reasonably good, subsequent data on lithium, sodium, potassium, and calcium, and, most recently, rubidium phosphates show that neither the rigid-rod nor flexible-chain random-distribution curves give a suitable fit [17]. It must be concluded

that second-order effects, such as the packing of the ions, bond angles, etc., must be considered in order to obtain a proper distribution curve. To do this would be mathematically very difficult since mere incorporation of the concept of randomly directed rods led to such involved mathematics that a rather complicated computer program was needed for calculating numerical distributions. My colleagues and I have dropped the problem of calculating proper size distributions for the

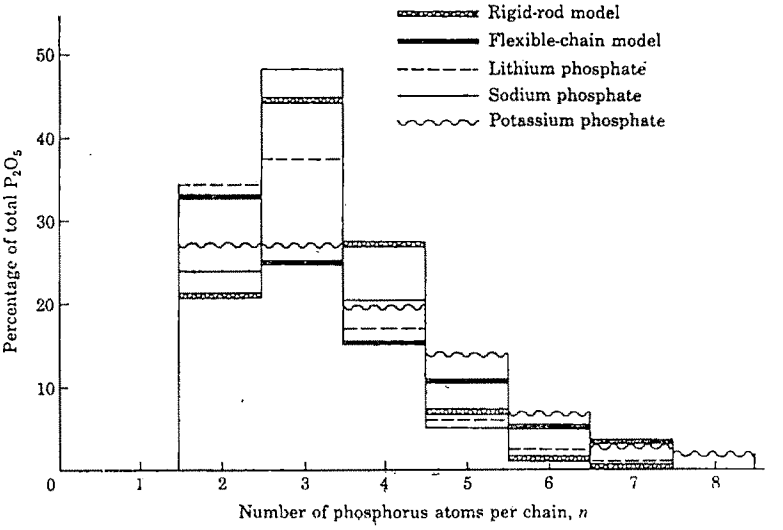


FIG. 16. Size distribution of chain phosphates in various phosphate glasses for which $\bar{n} = 3.0$.

anions in phosphate glasses on the basis that it is beyond the level of sophistication of present-day theoretical chemistry, at least as we know it.

Reorganization Leading to Network Molecules over Nearly the Entire Composition Range

From the thermodynamic data available on saturated hydrocarbons, it is possible to calculate free energies of formation of the various structure-building units in the aliphatic hydrocarbon family; that is, the *neso* molecule or methane; the end or methyl group; the middle or methylene group; the threeway branch or tertiary carbon; and the fourway branch or quaternary carbon. From these values, one can obtain the reorganization distribution curve shown in Figure 17 [18]. It should be noted in this figure that the mixed species—that is, the methyl, methylene, and tertiary-carbon groups—are present to considerably less than the amounts predicted from random interchange of hydrogen and carbon ligands on a carbon atom. Since, according to

equation 3, it takes three methylene groups per tertiary carbon and eight methyl groups per quaternary carbon to avoid formation of a ring-free wall-to-wall polymer, it can be seen from Figure 17 that the gel point in this system, assuming no rings, comes between the methane and ethane compositions.

Low-temperature pyrolysis of a hydrocarbon oil gives, at equilibrium, methane and the well-known char (the infinite polymer). This is a case where structural reorganization leads to uninteresting results, since

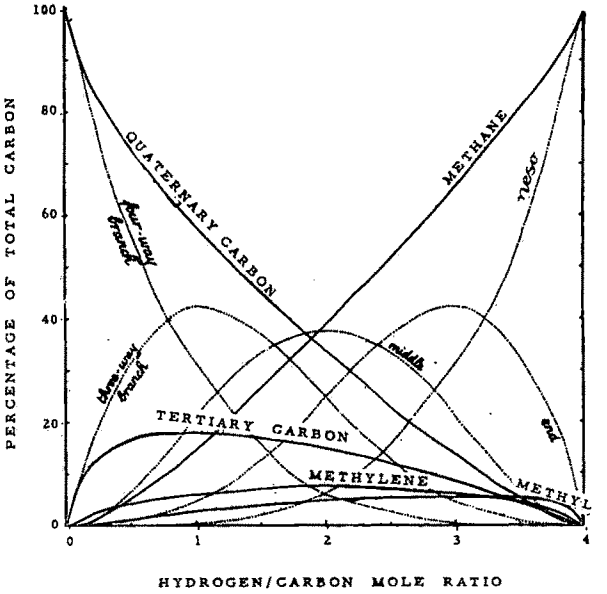


FIG. 17. Reorganization between structural building units calculated for gaseous aliphatic hydrocarbons at 25°C.

char is not amenable to detailed structural characterization (as is the case for amorphous network polymers in general). A similar situation is found for the polyphosphorus(III) chlorides. In this case, the equivalent of char is red phosphorus.

Future Investigations on Structural Reorganization at Monsanto

My colleagues and I are particularly interested in developing a sufficient theoretical basis so that *a priori* predictions as to deviations from random ligand interchange can be made. In order to afford a sound basis for the theory, we are studying ligand-interchange reactions throughout the Periodic Table. At the present time, we have work underway on structural reorganization in which boron, carbon, silicon, germanium, nitrogen, phosphorus, arsenic, antimony, and sulfur are the central atoms. An example of the type of results which we are looking

for is to be found in Figure 18, where it can be seen that merely by substituting oxygen for sulfur, the equilibrium concentration of ends drops greatly in the family of polyphosphate esters [19,20]. How could this have been predicted *a priori*?*

In addition to developing a theory which accounts for nonrandom ligand interchanges, it is also desirable to find and explain nonrandom sorting of structural units into molecules. We are also looking into this subject. A third topic of study is the kinetics of reorganization. Obviously, if one can predict equilibria as well as the rate of approach to these

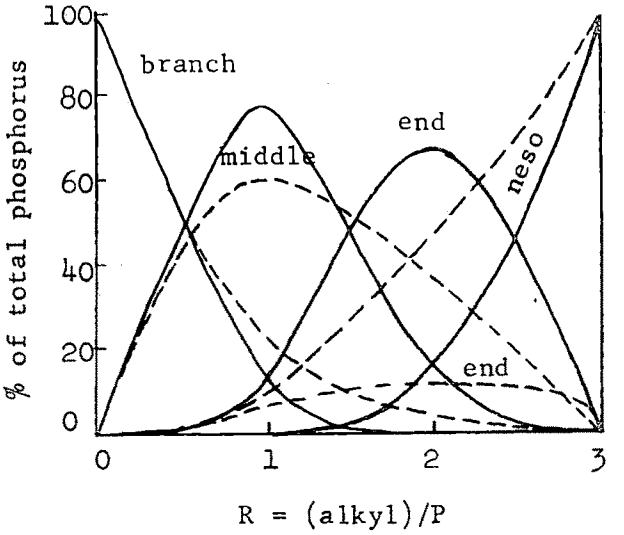


FIG. 18. Variations of *neso*, ends, middles, and branches with composition ratio for the alkyl polyphosphates and the completely sulfur-substituted alkyl thiopolyphosphates. —— = alkyl phosphates and ..... = alkyl thiopolyphosphates.

equilibria, he has developed a reasonably thorough understanding of the entire upper temperature range of chemistry—the chemistry of structural reorganization.

An interesting example of reorganization kinetics is shown in Figure 19 in which it can be seen that one of the intermediate species [diphenyl chlorophosphite, $P(OC_6H_5)_2Cl$] goes through a maximum [22]. Using a large analog computer, we have been able to trace out similar curves going through a maximum, assuming that ligand interchange occurs through bimolecular reactions between the various pairs of the four compounds involved in this system. In treating the kinetics of reorganization, a fifth type of randomness is involved. This kinetic random-

* We are working on the answer to this rhetorical question but are not yet ready to publish the results. For a preliminary paper on this subject, see Ref. 21.

ness is based on the idea that the exchange of an X ligand should be equally likely as the exchange of a Z ligand. It is interesting to note that random kinetics lead to random ligand-interchange equilibria, but that nonrandom kinetics can also lead to exactly the same equilibria, since there is an extra degree of freedom in the kinetics [22].

The Present Challenges of Chemistry

It seems to me that there are two outstanding challenges in chemistry today. One of them is to develop a thorough understanding of the most complicated of all chemistries of which we are aware—that is, the chem-

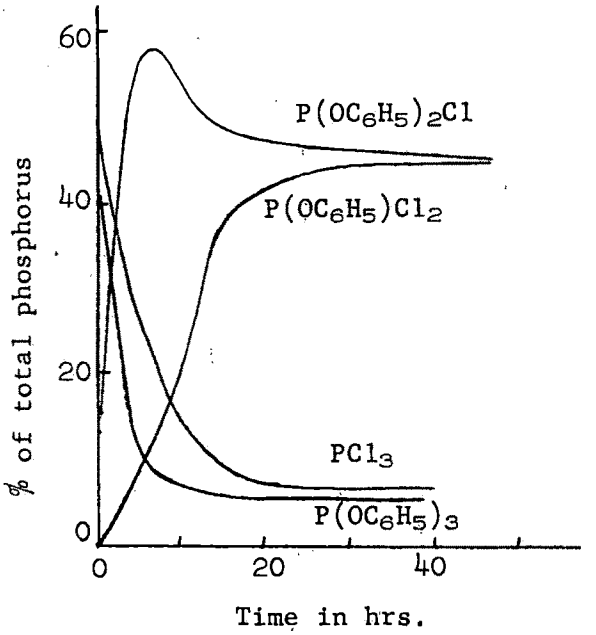


FIG. 19. Rate of equilibration of a mixture of phosphorus trichloride and triphenyl phosphite at 150°C.

istry of life. Biochemists have been doing a marvelous job in elucidating this chemistry and it would not be surprising anytime in the future to hear that some very simple form of life (say, a protovirus) had been synthesized in a test tube. The other important challenge in chemistry today is the development of replete chemistries of elements other than carbon on which complicated molecular structures may be based. Much of what is known about organic chemistry today involves molecular structures which are unknown in nature and have been prepared only through chemical reactions instigated by man. A whole host of such "unnatural" compounds are undoubtedly possible for elements other than carbon. Their

synthesis and characterization form the basis of the renaissance in inorganic chemistry wh ch is now just beginning.

An understanding of structural reorganization is vital both to modern biochemistry and to the inorganic renaissance. The role of phosphorus and the enzymes in biochemistry is to allow achievement of partial thermodynamic equilibrium. The Lipmann concept of "high-energy phosphate bonds" is very similar to the case presented in Figure 15 where the intermediate species in ligand interchange are present to greater amounts than correspond to random sorting of ligands without ionization. It surely seems that the principles of reorganization chemistry underlie many of the important reactions of life. Moreover this paper should have made it abundantly evident that one of the more difficult problems facing the inorganic chemist is the lability of molecular structures present in a fluid phase. Here, the approach to reorganization chemistry employed in this article is directly applicable.

From the ideas presented herein, it is apparent that the problem of developing systematic chemistries of the elements in the upper central part of the Periodic Table (e.g., B, Si, As, etc.) is primarily one of obtaining and exploiting the analytical tools suitable for labile substances, since many of the needed reactions can be carried out via reorganization chemistry. However, the case of the transition-metal elements, which often exhibit a sixfold coordination number in their compounds, may also be treated from the viewpoint of the fundamental reorganization chemistry discussed herein [1]. Since, to avoid wall-to-wall structures in the random case, it takes twenty-four end groups for each sixway branch group, one would conclude that much of the chemistry of the transition metals should correspond to amorphous network structures. In other words, for such systems, *neso* compounds, some di and even tri compounds (for example, the dichromates) ought to exist along with three-dimensional networks; but intermediate-sized molecules, such as long straight chains, will not be formed if the structural reorganization is anywhere near random. The other type of compounds to be expected for highly nonrandom equilibrium systems based on transition-metal elements consists of cage structures in which the branch groups are fitted together into relatively small molecules having few or no end groups. Examples of these structures are to be found in the polymolybdates and the heteropolyacids. Thus, it seems that much more structural chemistry of the usual type involving chains and rings can be expected for the lighter covalent-bonding elements, such as silicon and phosphorus, than for the transition metals.

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THE BEGINNINGS AND FUTURE OF SPACE ASTRONOMY*

By LYMAN SPITZER, JR.

THE SUGGESTION that scientific observations could profitably be made with instruments in space is a relatively recent development in the literature on space travel. The possibility of traveling to the moon and the other planets seems to have been discussed about as soon as men realized that these objects were probably bodies in space, somewhat similar to our earth. The first book dealing with a trip to the moon was written by Lucian of Samosata in the second century A.D. In more modern times the astronomer Kepler wrote on the same theme shortly before his death in 1630, and in the ensuing three centuries many discussions of space travel have been published. A summary of this extensive literature has been given by Willy Ley [1].

Many of these writers have pointed out that exploration of the moon and the planets would be a fascinating objective. Clearly, if man is to travel somewhere new, he will want to look around when he gets there! However, a clear realization of the astronomical advantages of space research dates from only a few decades ago.

Limitations of Ground-Based Astronomy

Only recently have the limitations of ground-based telescopes begun to place a serious restriction on astronomical research. There are three major such restrictions, as follows:

(*a*) Extinction of radiation other than visible light and short radio waves. Gamma rays, X rays, ultraviolet, infrared and long radio waves cannot penetrate the earth's atmosphere.

(*b*) Poor "seeing" for visible light. Light is bent by variable motions and thermal microstructure in the air, and images are blurred.

(*c*) Gravitational flexure of large telescopes. The bending of a large mirror and its supports, as the telescope is pointed in different directions, imposes a limit on the precision of the optical surface. In the absence of poor seeing this effect would limit the resolution obtainable.

The importance of restriction (*a*) has become very clear during recent years as a result of our increased understanding of astrophysics. We know enough about stellar spectra in the visible range to have some idea of the additional information that should be available at other wave lengths. Extension of the wave-length range is clearly a primary objective of space research.

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Restrictions (*b*) and (*c*) together limit the resolution obtainable in photographs. Restriction (*b*) is usually so serious that restriction (*c*) can be made unimportant with a suitably careful engineering design. Even with the most powerful instruments under the best conditions, however, the resolution obtainable on astronomical photographs is no better than 0.3 arcsecond, the theoretical resolution of a 12-inch telescope. This limitation on resolution, which is obviously of very critical importance in studies of the sun, moon, and planets, has become of increasing importance in stellar and galactic studies, where, for example, it limits the faintness of stars both in this galaxy and in other galaxies that can be detected against the ever-present background illumination.

These two objectives—an extended wave-length range and improved resolution—provide the overwhelming arguments for space astronomy. The importance of these objectives came gradually to be realized during the period between the two World Wars. Undoubtedly many scientists during these years discussed the potentialities of space astronomy. Here a few references may be made to typical analyses. H. Oberth, in his pioneering 1923 work [2] on the use of rockets for space travel, stresses the advantages of space astronomy; he points out that a large telescope in space, where stars do not twinkle, would be vastly more powerful than on earth, and also suggests that satellite observations of ultraviolet light from the stars might well lead to important discoveries. In 1929 the astronomical advantages of observing the full ultraviolet and infrared spectrum from a space ship were pointed out by my Princeton colleague, J. Q. Stewart [3]. The first publication I have been able to find dealing in specific detail with programs of space astronomy is an article by M. N. Saha [4] dated 1937; this paper analyzes the possibility of observing the ultraviolet spectrum of the sun from a balloon in the stratosphere. The analysis seems sound, although previous attempts [5] along this line had not proved entirely successful.

Start of Observational Space Astronomy

The real beginning of space astronomy was immediately following World War II. Two dates may be singled out as milestones in this subject. The first is October 10, 1946, when the first ultraviolet spectrum of the sun was successfully obtained by a group under R. Tousey [6] at the Naval Research Laboratory. A small spectrograph was mounted on a German V-2 rocket, one of a group being fired at White Sands, New Mexico, for purposes of testing and scientific research; a number of exposures were made automatically during flight, and the film developed after the rocket returned. While the resolution was not so great as has been obtained in later flights, features in the spectrum were shown down to wave lengths as short as 2100 Å. All previous solar spectrograms had shown essentially nothing at wave lengths shorter than about 2900 Å,

and this result was clearly an important scientific and historic achievement. Since that time, extensive solar research has been carried out not only with spectrographs and photographic film but also with filters and photon counters.

A second historic date was September 25, 1957, when the first high-resolution photograph of the sun was obtained from above most of the earth's atmosphere by a group under M. Schwarzschild [7], at the Princeton University Observatory. A 12-inch telescope was flown to about 82,000 feet from a balloon, and a number of photographs taken of the solar surface with nearly the theoretical definition to be expected with this aperture. Since at this altitude the telescope is above 97 per cent of the atmosphere, it is practically out in space as far as high resolution is concerned. Additional flights of this instrument have yielded important photographs of sunspots and of the solar limb.

Evidently, since the end of World War II space astronomy has been safely launched. What of the future? It is clear that telescopes in space can be vastly more powerful than those on the ground. The qualitative change made possible by placing astronomical telescopes in space is potentially a revolution in technique comparable with the initial invention of the telescope itself. Since equipment along these lines is very costly, one cannot predict how rapidly space astronomy will develop. The present paper will discuss what technical possibilities exist in this area, without consideration of economic problems. Some of the more ambitious undertakings are entirely speculative.

The future development of space astronomy will be subdivided here into three categories—the near future, the long-range future, and the ultimate. The near future is defined to include the next five to ten years, and is restricted to programs already under way, at least in part. The long-range future includes possible subsequent developments over the next few decades; large orbiting telescopes, several hundred inches in diameter, are a natural objective of this long-range program. Discussion of the "ultimate" in space astronomy involves a discussion of what limits are imposed by nature on the power and resolution of telescopes in space.

The Near Future

The program for the near future is well known to most astronomers. It includes three elements. First there is a vigorous program of small-scale experiments. For example, extensive measurements of ultraviolet stellar spectra from sounding rockets are under way at a number of centers. In addition, some important gamma-ray spectroscopy from high-altitude balloons is in progress. A second element is the program of Orbiting Astronomical Observatories organized and sponsored by the National Aeronautics and Space Administration. This major program,

accommodating a series of telescopes of about 30 inches aperture, should yield important results during the next few years on stellar ultraviolet spectra observed with various degrees of resolution. A third element in this short-range program is the Stratoscope II project, which is designed to obtain the theoretical resolution of a 36-inch telescope flown by balloon into the stratosphere. If this instrument can in fact obtain the desired resolution of 0.1 arcsecond, and maintain its pointing with the same accuracy for hours at a time, it should outperform the 200-inch Hale Telescope in some of the most critical problems of modern astronomy.

The scientific problems that will presumably be investigated in this near-future program have been discussed on many occasions. Here I shall list a few particular problems that seem of outstanding interest for astronomy.

1. *Ultraviolet Flux from Hot Stars:* The rare stars of surface temperature above 20,000°K. radiate most of their energy in the previously inaccessible ultraviolet. A precise measurement of the ultraviolet luminosity would give information on the amount of time these stars require to burn their nuclear fuel, and would thus fix the time scale for stellar evolution. Also, since this radiation provides the chief source of energy for the interstellar gas, this information is important for interstellar gas dynamics.

2. *Chemical Composition of the Stars and Interstellar Gas:* Measures of stellar and nebular spectra in the ultraviolet should much improve our information on the abundance of chemical elements. Measures of interstellar absorption lines should give information not now obtainable at all and may, for example, reveal that molecular hydrogen, hitherto completely unobserved, is a dominant constituent of the Galaxy.

3. *Photometry of Dense Clusters:* With a resolving power of 0.1 arcsecond, giant stars at the center of a globular cluster can be successfully resolved, and their spatial distribution measured. Since kinetic equilibrium exists at the center of a cluster, the relative density gradients for stars of different types should give accurately the ratios of the masses. At the present time there is no direct determination of the mass of these luminous stars, and this program will therefore be of interest for theories of stellar structure as well as for stellar dynamics.

The Long-Range Future

Let us consider now the long-range program. Obviously, there are many possible areas in which space astronomy might develop, including research on such problems as low frequency radio waves, X rays, γ rays, infrared and ultraviolet light. Large special-purpose instruments may be visualized for detecting photons in each of these wave-length ranges. Here I shall restrict myself to a single type of instrumental development, and consider the scientific progress that might be carried out with an optical telescope 400 inches in diameter, or some 10 meters across.

Such an instrument would be an order of magnitude larger than any of the instruments planned for the near future. An instrument of this size for ground-based research has been proposed from time to time, but it now seems clear that the proper location for such a major astronomical tool should be out in space. Obviously, substantial scientific and engineering work would be required before such an instrument could be placed in orbit. If a 400-inch telescope could be figured to yield the theoretical resolution, its resolving power would be 10^{-2} arcsecond.

First let us consider some of the engineering problems associated with such a large venture. Problems of launching and of operation in space environment will not be treated here, since these areas are being very actively investigated by many engineers. We discuss here the difficulties which are peculiar to a large astronomical space telescope. If the mirror is to be figured on the ground, the deflection under gravity becomes important. As with the 200-inch mirror in the Hale telescope on Mt. Palomar, a system of multiple supports can be devised which will support the mirror uniformly. However, the uniformity can certainly not be exact. Let us assume that a small fraction of the mirror weight, say 10^{-3}, is carried by the central supports alone, with some resultant sag at the periphery. If we replace the mirror by a glass beam 10 meters in length, supported entirely at its center, but with gravity reduced by 10^3, the sag becomes $0.4/d^2$ cm where d is the thickness in centimeters. If the sag is to be less than 5×10^{-6} cm, or one tenth the wave length of visible light, d must be 300 cm, or 3 meters. So great a thickness is probably not practical. For a 100-inch mirror the necessary thickness computed on these same assumptions is only about 20 cm; figuring and testing of a 100-inch mirror on the ground is apparently possible with existing techniques. However, for a 400-inch mirror we may conclude that either some means must be devised to support a mirror much more uniformly during ground tests, or the desired precise contour for the mirror surface must perhaps be produced in space. Precise testing of a mirror in orbit would not be difficult, but extensive polishing in orbit would seem a more complicated task.

One auxiliary result of this analysis is that unless much more uniform supports can be devised, the moon is no place for a large diffraction-limited telescope. Even with the reduced surface gravity on the moon, the necessary thickness for a 400-inch mirror, on the assumptions stated above, is 1.2 meters. Problems of temperature control, discussed below, and possibly of vibration might also be troublesome for a large lunar telescope. If problems of radiation shielding for personnel can be solved, the ideal location for a large telescope is in orbit, where the advantages of weightlessness and of thermal uniformity as well as of vacuum can be realized.

The problem of temperature control is a very serious one. If the

mirror is fused silica 25 cm thick and 10 meters across, then, at room temperature, a detectable change of focus will be produced if the temperature difference between front and back exceeds 3×10^{-3}°K. The focal change by itself is not so serious, if it is uniform across the mirror. However, if the temperature difference front to back varies by more than 10^{-2}°K. across the mirror surface, a serious loss of resolution will result. Evidently great thermal uniformity is required for a large mirror.

It follows that a vast telescope of this sort cannot be placed in a close orbit, a few hundred kilometers above the earth's surface, if it is to obtain the theoretical resolution. The continual occultation of the sun by the earth, together with the variable radiation from the earth, would make the maintenance of such uniform thermal conditions very difficult. In a more remote orbit, however, substantial thermal uniformity should be possible.

The problem is greatly eased if the operating temperature is reduced to about −140°C. At this temperature fused silica of the type provided by Corning for large mirrors has a minimum length. Depending on how close to the optimum temperature the mirror can be maintained, the requirement on thermal uniformity can be substantially relaxed. One can visualize a telescope shielded from direct sunlight by several concentric spheres of insulation, to give a low leak of heat into the telescope at a fixed rate, independent of the direction of pointing. Leakage of heat out the viewing opening might then suffice to bring the temperature down to its desired value. A Cassegrain secondary would presumably be necessary; its position could also be fixed by fused silica rods, or could be adjusted for optimum focus from time to time.

Evidently an instrument of this sort would be an impressive installation. The mirror alone would weigh about 50 metric tons, if it were solid fused silica 25 cm thick. Revolving about the earth in a remote orbit, or possibly in its own orbit about the sun, a substantial crew in space would be required to assemble the device, get it into operation, and possibly even to produce the final mirror figure. Remote operation might be advantageous for most of the time, but maintenance visits at least yearly would be required, since it would be expected that such a powerful and costly tool would be kept in operation for many decades.

What scientific problems could be attacked with so penetrating an eye in space? With a resolution of 0.01 arcsecond such a telescope could resolve an object 50 feet in size 200,000 miles away. It could, for the first time, resolve surface features in a few stars; the diameter of supergiant stars such as α Orionis, α Scorpii, and o Ceti is between 0.04 and 0.05 arcsecond. It could measure precisely the parallaxes of stars as far away as 1000 parsecs. Since the image size for a point source would be so very small, the background illumination, substantially

reduced below its level at the earth's surfaces, would not be an important limiting factor in masking faint stars. A star of apparent magnitude 30, 7 magnitudes fainter than can be detected with the 200-inch Hale Telescope, could be detected by a photoelectric image tube with a 2-hour exposure. Stars several magnitudes fainter might be detected with longer exposures. A star of solar type could be detected in M 31 (the famous galaxy in Andromeda), while supergiant stars could be resolved even in galaxies a hundred megaparsecs away.

Detection of Extra-Solar Planets

One problem of particular interest that will be emphasized here, as an important part of the long-range program, is the detection of planets around other stars. This is a matter of very great philosophical and cultural as well as of scientific interest. Our view of man and his place in the universe naturally depends very much on whether planetary systems like our own are exceptional or whether they occur very frequently throughout the Galaxy. In fact, in many ways, the question of how frequently stars are accompanied by planets capable of supporting life is fully as important as the over-all structure of the universe, i.e., whether space is flat or curved.

The detection of other planets, however, is an extraordinary difficult problem. Not only are planets intrinsically very faint, they are necessarily very close to a star if they can be detected at all, and light from the central star, scattered or diffracted in the telescope system, inevitably tends to mask the very faint planetary light.

To consider a specific example, let us look at the problem of detecting the planet Jupiter if it revolved about a star some 5 parsecs away. According to Van de Kamp [8], within five parsecs (sixteen light years) from the sun are eleven stars somewhat similar to the sun—one F star, two G stars, and eight K stars. If the planetary orbit is taken to be a circle five astronomical units in radius, like Jupiter's orbit around the sun, the maximum apparent separation between the planet and the central star is one arcsecond, or many times the width of the theoretical diffraction pattern for a large telescope. The problem is entirely one of distinguishing the planet from the scattered and diffracted starlight.

The average brightness of Jupiter relative to its central star is simply the albedo (the over-all reflectivity) times the solid angle intercepted by the planet, divided by 4π. The actual brightness will depend on the phase, of course, but at maximum separation, when the phase angle is 90°, the apparent brightness should not differ greatly from the average. At 5 astronomical units, this relative brightness is 10^{-9} for an albedo of 0.5. Thus Jupiter would be fainter than its central star by 22.5 magnitudes, on the average, an impressive difference.

If we neglect scattered light, and consider only the theoretical diffrac-

tion pattern for a uniform circular aperture of radius a, the intensity $I(\theta)$ varies with angle, θ, according to the simple formula

$$I(z) = 4I(0)\frac{J_1^2(z)}{z^2}, \tag{1}$$

where $J_1(z)$ is the usual Bessel function of order 1, and

$$z = \frac{2\pi a \sin\theta}{\lambda}; \tag{2}$$

λ is the wave length of the light under consideration. For large z, $J_1^2(z)$ has an average value of about $1/\pi z$, and

$$\frac{I(z)}{I(0)} = \frac{4}{\pi z^3}. \tag{3}$$

The angular resolution of the telescope is usually taken to be the value of θ corresponding to the first zero of $J_1(z)$, or z equal to 3.83. We denote this value of θ by θ_r.

From these results we can readily see that if the peak intensity for Jupiter is to exceed the intensity of diffracted light from the central star, θ must exceed θ_r by about 300, requiring a telescope of 1200-inch aperture. In theory, the diffracted light can be much reduced by reducing the reflectance of the mirror gradually to zero at the edge in a predetermined way. In addition, detection of a signal is possible even in the presence of substantially greater background illumination. On the other hand, for visible light the scattered radiation from achievable surfaces is likely to exceed the diffracted light, and considerably more information would be needed to indicate how large a telescope might be required.

Use of Occulting Disc

Fortunately there is a better way to detect planets around other stars, a method pointed out by R. Danielson at Princeton. This method involves the use of a large occulting disc far in front of the telescope to reduce the light from a star. To evaluate the possibilities of this technique we consider a semi-infinite plane sheet at a distance l from the telescope. The reduction obtainable may be computed from the standard equations of Fresnel diffraction [9]. If x represents the distance from the geometrical shadow edge to the center of the telescope mirror, then the amplitude Φ of the light wave is a function of the parameter u, where

$$u^2 = \frac{2x^2}{\lambda l} \tag{4}$$

The quantity u^2 is four times the phase shift from the geometrical edge of the shadow in to the telescope. For optimum results, the apparent edge of the occulting plane, as seen from the telescope, should be about

half way between the star and the planet. This will place the greatly weakened diffracted light still an appreciable distance away from the planet, where the resolution of the telescope can discriminate against it. Thus x/l corresponds to half a second of arc, or 2.5×10^{-6}. If Φ^2 is normalized to unity well outside the shadow then Φ within the shadow is given by Fresnel's integrals. As shown by Cauchy, for large u we have

$$\Phi^2(u) = \frac{1}{2\pi^2 u^2} \left\{1 - \frac{5}{\pi^2 u^4} \cdots \right\} \tag{5}$$

For the occultation to be effective, the distance x into the shadow must considerably exceed the telescope radius. We take x to be 1000 inches or 25 meters, equal to five times the mirror radius; the distance l becomes 10,000 km, u^2 is 300, and Φ^2 is about 1.7×10^{-4}. The remaining reduction of 6×10^{-6} can be achieved with the telescope. If the diffracted light emanated from a point, the theoretical resolution of a 110-inch telescope would suffice to make the light from the planet equal to the background. However, the source will resemble a line, the diffracted light will fall off less rapidly with angular distance than from a point source, and a somewhat larger telescope might be needed.

In the same way that the diffracted light from a telescope mirror can be reduced by a smooth reduction of the reflectivity towards zero at the edge, the shadow behind an occulting disc can be made much blacker if the transparency of the disc varies smoothly at the edge of the disc rather than abruptly. This effect has been used by R. Tousey and his group in the design of a solar coronagraph; a reduction of intensity within the shadow by an order of magnitude was achieved with the use of an occulting disc edged with sharp spikes. By such techniques the requirement on the scattered and diffracted light in the main telescope can certainly be eased; further study is required to indicate in detail how much might be gained by this technique.

To detect a planet such as the earth is more difficult, partly because the earth is intrinsically fainter by about a factor 10, but mostly because it is closer to the star. If u is kept constant, l must be increased by a factor 25 and the distance x inside the shadow increased by 5 to reduce x/l to 0.10 arcsecond, half the angular distance subtended by one astronomical unit at 5 parsecs distance. If a conventional occulting disc with a sharp circular edge were used, the telescope would have to detect the planet in the presence of diffracted light 1.7×10^6 times as intense and 0.10 arcsecond away; a 1200-inch aperture would be required according to equation (3). If the use of a disc with a more complicated edge can darken the shadow by two additional orders of magnitude, a 300-inch should be adequate on these assumptions.

Controlled and precise positioning of an occulting disc would pose a variety of problems. If a circular occulting disc with a sharp circular

edge were used, a diameter of $3x$ would probably be sufficient to make the formulae for a semi-infinite plane a reasonable approximation. A disc or balloon of 75 meters diameter would thus be adequate for detecting a planet at 1 sec of arc distance; an obstacle five times this diameter would be required for a planet at 0.2 arcsecond. The large distances involved, 10,000 km in the first case and 250,000 km in the second, would complicate the control, since the relative motion across the line of sight must be kept very small. Observation of the obstacle from the telescope—perhaps a flashing light—would provide an error signal which could be transmitted back to control motive power in the obstacle. At a relative distance of 10,000 km, precise control might be possible with moderate power in an orbit around the earth. At a separation of 250,000 km, on the other hand, precise relative positioning during the several hours exposure required might be achieved more simply, and with lesser power, in an orbit around the sun.

There are at least two other techniques that might be used for detecting other planetary systems. The first is the use of infrared, where the luminosity of planets relative to the sun is much greater. For example, in the far infrared, longwards of 25μ, where the Rayleigh-Jeans law is applicable both for the sun and for Jupiter, the luminosity of Jupiter relative to the sun is 2.3×10^{-4}, a gain of more than 10^5. This ratio of infrared luminosities corresponds to 9 magnitudes. Most of this gain is offset by the loss in resolution at the longer wave lengths, and the telescope needed to resolve Jupiter at 5 astronomical units from a star 5 parsecs away from the sun is now 1000 inches, instead of the 1200 inches found for visual light. On the other hand, the theoretical diffraction pattern is more easily achieved at 30μ wave length than with visible light and one can even be fairly confident of reducing the far wings of the diffraction profile by one or two orders of magnitude by means of radial variation of reflectivity. The occultation technique could still be used in the far infrared, although use of a complex edge, as described above, would be required to give a really dark shadow at the low values of u^2 involved. The relatively low sensitivity of infrared detectors is perhaps a major obstacle to this proposed technique.

A second method, suggested by Fessenkov [10], might be to look not for planets but for the zodiacal light which probably accompanies a planetary system. The total luminosity of the zodiacal light in the planetary system much exceeds the combined luminosity of the planets, and the same might well be true of other systems. On the other hand, the surface brightness of the zodiacal light would be no greater than in our own system, and at five astronomical units away from a star might be very low. The discrimination against scattered and diffracted light from the central star required for detecting zodiacal light may well be much the same as for detecting the larger planets.

We conclude that, with a 400-inch space telescope, several planets could be detected if the stars within 5 parsecs possess planetary systems similar to our own. The optimum technique would probably be to use a precisely positioned occulting disc many thousands of kilometers away to facilitate detection of the faint planets by the great telescope. In any case, the discovery of other planets around neighboring stars would be a tremendously important event.

Ultimate Possibilities

We pass on now to the ultimate possibilities in space astronomy. Again we concentrate on the one problem of high resolution, leaving aside such equally fascinating potentialities as X-ray and γ-ray telescopes, low frequency radio arrays, etc. The problem we consider here is how sharp an optical resolution the laws of nature will permit for a space telescope.

The first basic restriction that might appear is poor interstellar seeing, with the wave front becoming slightly corrugated as a result of the inhomogeneous medium through which the light passes. This effect is not likely to be appreciable, even at angular resolutions many orders of magnitude sharper than the 10^{-2} arcsecond discussed above. Since the particle density of the interstellar gas is less than in the earth's atmosphere by a factor in the neighborhood of 10^{-16}, the bending of rays produced by density gradients should be correspondingly reduced. Moreover, the mean free path of all particles in interstellar space is many millions of kilometers, and any refraction effects should be uniform across the aperture of any conceivable telescope or interferometer. We conclude that any imperfection in interstellar seeing is probably much too small to detect.

Another basic restriction might be the flexure of a large telescope resulting from the tidal force of the sun's gravitation. In principle, this flexure can always be reduced arbitrarily low by going sufficiently far from the sun. Similarly, thermal inhomogeneities resulting if a telescope is pointed at different angles to the sun can also be decreased arbitrarily low if the instrument is sufficiently far away from the sun.

Let us consider, for example, how large a telescope would be needed to obtain on a planet at one parsec distance the average resolution—about 300 km—obtainable on Mars from ground-based instruments, when the planet is about one astronomical unit away. Since the distance of the planet would be increased by a factor 2×10^5 if it were located at one parsec, the telescope size must also be increased by the same factor over the 12-inch diameter that achieves the seeing-limited resolution of 0.3 arcsecond. The resultant diameter would be in the neighborhood of 60 km. An asteroid of moderate size might provide the best blank for such a mirror! The tidal force of the sun at one astronomical unit would

distort the figure beyond tolerance if the mirror thickness were less than 30 km. However, well beyond the orbit of Pluto the necessary thickness would be reduced to some 100 meters. Obviously, we have no reason to believe that it will ever be possible to fabricate a surface of this diameter to optical quality.

The above computation is an interesting exercise which shows how many, many orders of magnitude away we are from observing other planetary systems with enough resolution to distinguish clouds, oceans, snow-covered regions, and forests. It would probably be easier to send a probe on a manned expedition to several stars nearby than to build the equipment necessary to view these planets with appreciable resolution from close to the sun.

This brief survey of space astronomy is necessarily very superficial. It may serve to suggest at least a few of the many fascinating potentialities that lie ahead, the many challenges offered to astronomers both now and in the centuries to come.

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PHYSICOCHEMICAL ASPECTS OF HYDROGEN BONDS AND THEIR APPLICATION TO BIOLOGY*

By MAURICE L. HUGGINS

IT IS NOW universally recognized that hydrogen bonds are of very common occurrence in biological systems and that they are of great importance in determining the most stable arrangements of the atoms in such systems and also their chemical, physical and biological behavior[1,2,3].

A hydrogen bond—also called a "hydrogen bridge"—consists of a hydrogen atom bridging usually two electronegative atoms. In biological systems the electronegative atoms are almost always nitrogen or oxygen, but under special circumstances one can be carbon, chlorine, or another element.

One reason for the frequent occurrence of hydrogen bonds in and between biological compounds is that these compounds contain many groups of atoms of types which readily form such bonds: for example, —OH, NH, C=O, etc. It might reasonably be contended, however, that these groups are so prevalent because of their hydrogen bond forming ability and the special properties which they impart to the systems containing them.

Energy Considerations

Nearly all of the hydrogen bonds in organic compounds of known structure are unsymmetrical: The equilibrium position for the hydrogen is closer to one of the two bridgehead atoms than to the other and the hydrogen is more tightly bonded to one than to the other. The shape of the curve representing the variation of the energy of the hydrogen bond system as a function of the location of the hydrogen between the bridgehead atoms is one of the types *a*, *b*, *c* of Figure 1, not of type *d*.

Symmetrical hydrogen bonds, with energy curves having only a single minimum (Fig. 1, type *d*), exist in the FHF^- ion. They may also exist in biological materials of special kinds. It would be reasonable, for example, to postulate hydrogen bridges in nerve fibers which would individually be either symmetrical, of type *d*, or unsymmetrical (type *a* or type *b*) with a very small energy hump[5]. To account for the conduction of nerve impulses would require the additional assumption that

* This article will also be published, in Japanese, as a chapter in a book by S. Yamabe, *Physical Chemistry of Drugs*, Asakura Publ., Inc., Tokyo, 1962.

the bridges are so coupled together electronically, either directly or through conjugated systems, that a shift of a hydrogen in one produces a shift in the same direction in the next, and so on. It is also tempting to speculate about similarly coupled hydrogen bonds in the brain. Other systems of coupled polarizable units can of course be envisaged, but the known existence of extended chains of hydrogen bonds, coupled through conjugated systems, in fibrous proteins makes this hypothesis especially attractive.

The energy required to break a hydrogen bond is usually in the range between five and eight kilocalories per mole[6,7]. This is much less than the range for ordinary covalent bonds, which is roughly 50–100 kilo-

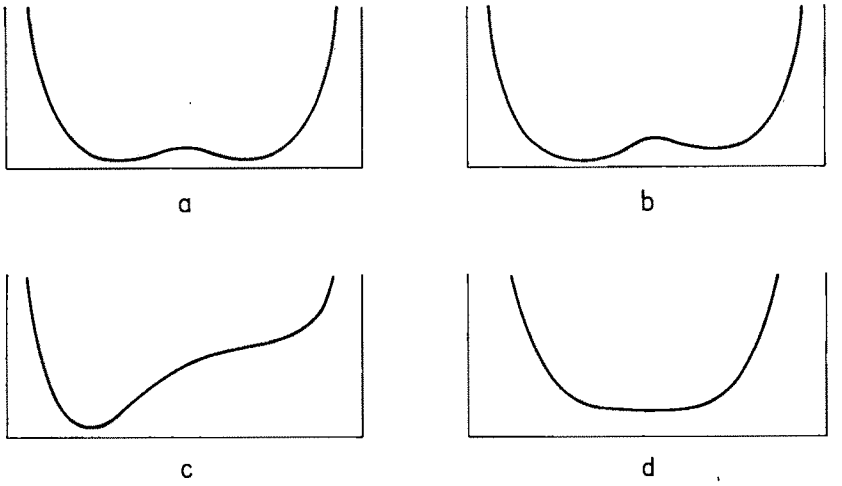


FIG. 1. Types of energy curves for hydrogen bonds [4]. Each curve shows the variation of the energy of the system (ordinate) with the distance (abscissa) of the hydrogen atom from the center of one of the bridgehead atoms.

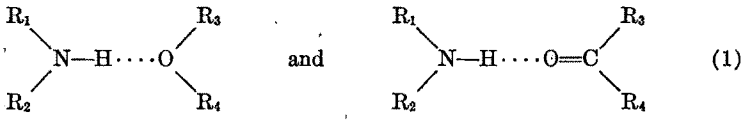
calories per mole. Hydrogen bonds are thus much more readily broken. It may be noted that the temperature motions in a solid or liquid will cause interatomic "bonds" to break frequently, if the bond strengths are less than one or two times RT—i.e., of the order of one kilocalorie per mole, at room or body temperatures.

In biological or other systems containing hydrogen bonds, at temperatures other than very high ones, these bonds are not usually broken without other compensating changes taking place. In most instances new hydrogen bonds are simultaneously formed, so that the reaction is one of an exchange type. The energy needed is much less than that required to break the bond without forming another; it is undoubtedly often less than the average energy of the temperature motions, or even less than zero. (For quantitative treatments one must of course consider

free energies when dealing with equilibria and the magnitudes of the *free* energy barriers which must be surmounted in the reaction process, when dealing with reaction rates.)

Factors Affecting Hydrogen Bond Properties[3,6]

Typical N—H...O hydrogen bonds, connecting an amine or imine with an ether or a ketone, can be represented in simplified fashion by the formulae

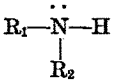


(1)

The polarity of each of these hydrogen bonds and the ease with which it can be dissociated (at the dotted line between the H and the O) are both greatly dependent on the effective electronegativity of the N and O atoms, and so on the nature of the atoms or groups R_1, R_2, R_3, and R_4. (The R's here denote hydrogen or other atoms or groups.) Other things being equal, the more electronegative R_1 and R_2 are, the greater is the attraction of the N for the bridging hydrogen, the greater the dissymmetry (polarity) of the bridge, and the greater the ease with which it can be broken. On the other hand, the greater the electronegativity of R_3 and R_4, the stronger is the attraction between the oxygen and the bridging H atom, hence the stronger and less polar the bridge. Any changes in the R groups affecting the effective electronegativities of the N and the O affect their attractions for the positive hydrogen nucleus between them and so the strength and polarity of the hydrogen bridge.

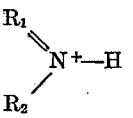
A ketonic oxygen is in general more electronegative than an ether oxygen; hence the former makes stronger and less polar hydrogen bonds, with an NH group for instance, than the latter.

If the bonds connecting R_1 and R_2 to N are both single bonds, the nitrogen atom has a lone pair of electrons in its valence shell:



(2)

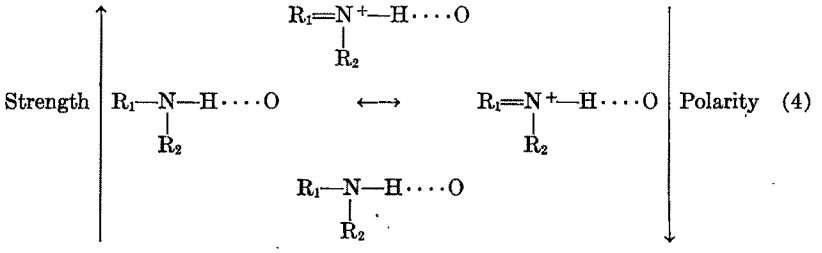
If, however, R_1 or R_2 is connected by a double bond to the nitrogen, that atom has no lone pair and its formal charge is not zero, but +1:



(3)

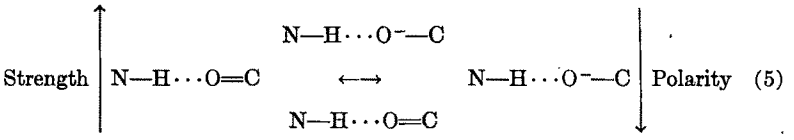
This nitrogen is less electronegative than in the preceding case (formula 2); it has less attraction for the hydrogen attached to it; and, if that hydrogen forms a hydrogen bond to an oxygen atom, as in formula 1, the hydrogen bond is stronger and less polar.

If R_1 (or R_2) is of such a nature as to give resonance between two extreme states in which the bonding at the nitrogen corresponds to formulas 2 and 3, the hydrogen bonds formed by the NH of this group with other electronegative atoms have strengths and polarities intermediate between those of corresponding hydrogen bonds formed by moieties which are strictly of the type of formula 2 and those which are strictly of the type of formula 3:



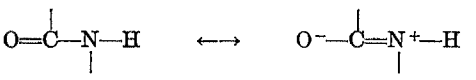
(4)

Similarly, the oxygen of a C—O⁻ moiety is more negative than that of a C=O moiety. The former therefore has a stronger attraction for a bridging hydrogen and (if this oxygen is at the weak end of a hydrogen bond) it gives the stronger hydrogen bond. That bond is also less polar. Resonance, with these two CO structures as extreme states, produces bonds of intermediate strength and polarity:



(5)

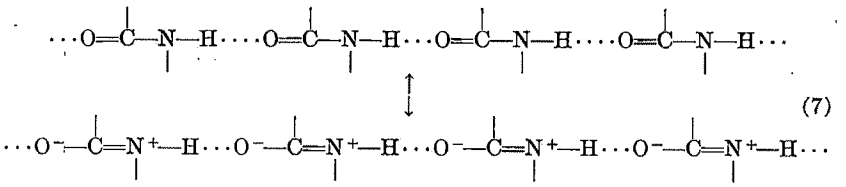
In an amide or imide there is undoubtedly strong resonance of the sort indicated by the following equation.



(6)

As a result of this, the NH groups of these compounds form stronger hydrogen bonds (to oxygens, for example) than do the NH groups of simple amines or imines, and the carbonyl oxygens of these compounds form stronger hydrogen bonds (with NH or OH groups, for example) than do simple, nonresonating carbonyl oxygens. Many proteins contain

chains or rings of hydrogen bonds connecting resonating imide systems:

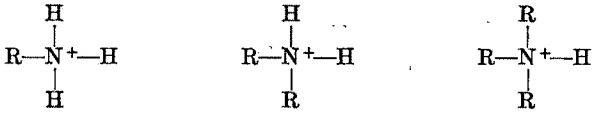


(7)

The hydrogen bonds are doubtless much stronger than they would be without the resonance.

The hydrogen atoms in the hydrogen bonds in chains of this type would be expected to oscillate in synchronism, producing polarity changes within the OCNH groups with each oscillation, in such a way as further to strengthen the hydrogen bond system [8].

The lone pair of electrons on the nitrogen atom of a primary, secondary or tertiary amine is capable of adding a hydrogen ion to form a substituted ammonium derivative:

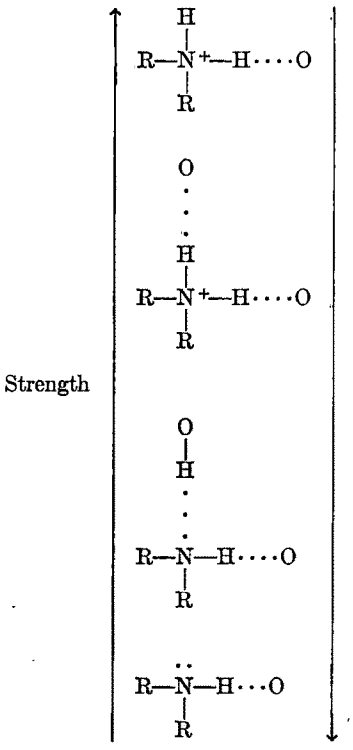


(8)

Applying the principles outlined above, one can conclude that hydrogen bonds formed with the NH groups of these quaternized nitrogen systems are stronger than those formed with the NH groups of corresponding nonquaternized nitrogen systems. The addition or removal of hydrogen ion thus markedly affects the equilibria and reaction rates concerned with the formation, breaking, and exchange of hydrogen bonds.

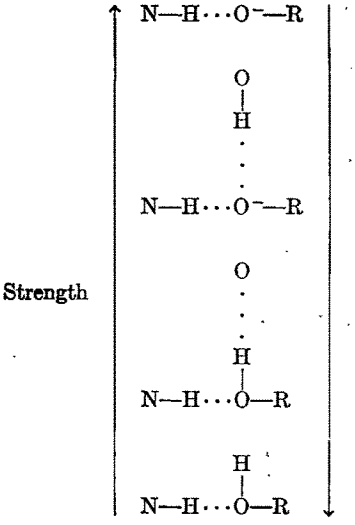
Ions of certain metals, especially those of transition elements, can add, like hydrogen ion, to amines and other nitrogen compounds containing available lone pairs[9,10]. Such additions affect the strengths and other properties of hydrogen bonds having the nitrogen in question as a bridgehead atom in (qualitatively) the same way as do additions of hydrogen ions.

If an atom of nitrogen, for example, is simultaneously connected by hydrogen bonds to two other electronegative atoms, the strength and polarity of each of these hydrogen bonds are affected by the existence of the other. Consideration of the forces involved leads to the conclusion that the strengths of the hydrogen bonds represented horizontally in the following formulas decrease and the polarities increase in the following order:



(9)

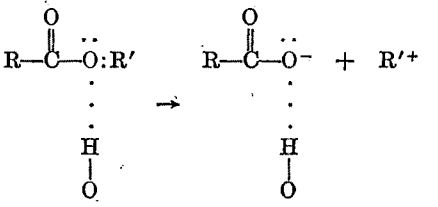
Similar considerations indicate that the strength and polarity of a hydrogen bond are also greatly affected by the existence of *other* hydrogen bonds to or from the bridgehead atom at its weak end. For example, these properties would be expected to change, for the hydrogen bonds represented horizontally, in the following order.



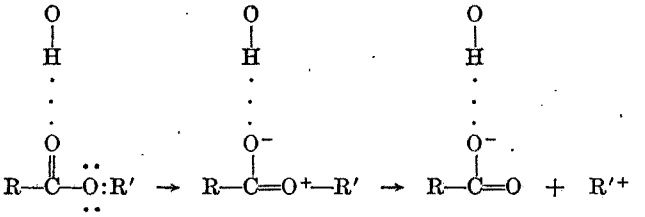
(10)

From the foregoing, it should be obvious that reactions involving the formation or breaking of a hydrogen bond are greatly affected by changes in its environment, especially with regard to the presence and concentrations of hydrogen bond forming atoms or groups and of hydrogen ions or metallic ions capable of interacting with the lone pairs on the bridgehead atoms.

Reaction rates and equilibria concerned with the making and breaking of other types of bonds—ordinary covalent bonds—are also often greatly affected by the presence of groups which can form hydrogen bonds with the atoms adjacent to the bond in question or otherwise electronically connected to it (e.g., through a conjugated system). For example, the hydrolysis of an ester can be facilitated by hydrogen bond-forming reagents, acting on either of the two carboxyl oxygens.



(11)



(12)

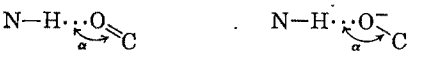
(These representations are schematic. It is not intended to imply that R'^{+} ions are produced in the reaction, but merely that any structural changes favoring their formation would also favor any reaction in which the O-R bonds are split in polar fashion.)

Structural Considerations

The distance between the centers of the bridgehead atoms of an NHN, NHO, or OHO hydrogen bond is usually[6,7] between 2.5 and 3.2 A. The hydrogen is about 0.95 to 1.1A from one bridgehead atom and 1.4 to 2.2 from the other. Since most of the force holding the bond together (i.e., the force between the hydrogen and the more distant bridgehead atom) is probably coulombic, rather than covalent, in nature, this force varies with the distance between the bridgeheads less rapidly than does the force between two covalently bonded atoms. As a result, the lengths of hydrogen bonds can vary considerably with but small changes in the bond energy.

Covalent interactions between the hydrogen of a hydrogen bond and its two neighbors tend to make these three atoms collinear, i.e., to produce a straight hydrogen bond. The coulombic repulsion between the two bridgehead atoms acts in the same way, but only very weakly. With only weak covalent contributions to the interaction between the hydrogen and its more distant neighbor, it is not surprising that other factors produce some departures from collinearity, in compounds for which sufficiently precise structural determinations have been made.

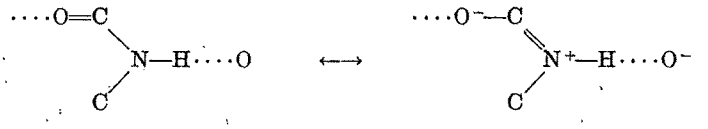
Coulombic forces between a bridging hydrogen and the electronegative atom at the weak end of the bridge are essentially independent of the orientation of the hydrogen bond relative to the bond (or bonds) connecting that bridgehead atom to another atom (or atoms). Covalent bonding forces would be expected to favor an orientation in which the hydrogen bond is in the direction of maximum extension of an orbital occupied by a lone pair of the bridgehead atom. These covalent bonding forces, however, are usually relatively weak and their effect seems to be usually less important than other factors affecting the hydrogen bond orientation. To be more specific, the angle α at the O atom, for a hydrogen bond joining an NH group to a carbonyl oxygen,



(13)

would be expected to vary over quite a wide range with but little change in energy, even though any covalent bonding through oxygen electron pairs would favor an angle of about 125° for a ketonic C=O structure, 110° for a single bonded C—O⁻ structure, or an intermediate value for a resonating structure[11]. Truly satisfactory data for testing this point are, however, not yet available.

A somewhat similar question, also unsettled, is that of the preferred orientation of the N-H bond relative to the plane containing the nitrogen and the two carbon atoms to which it is also attached, in a resonating peptide system such as occurs in proteins and synthetic polypeptides.



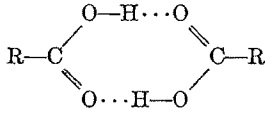
(14)

(See also formula 7.) In the first of the two extreme structures represented, the nitrogen atom has a lone pair and the three bonds extending from it would be expected to prefer angles between them which are approximately tetrahedral—hence not coplanar. In the second of these extreme structures the three bonds should be coplanar. Pauling has estimated that the bond from the carbonyl carbon atom to the nitrogen has approximately 40% double bond character. He assumes coplanarity

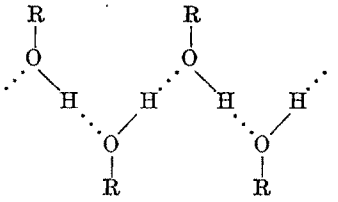
of the bonds in question, but in the writer's opinion a noncoplanar structure might be slightly favored[11]. Definitive experiments are lacking.

Hydrogen Bonding, Aggregation, and Complex Formation

Many organic compounds with suitable polar groups form dimers or larger aggregates in the pure liquid state or in solution, the individual molecules being held together by the hydrogen bonds formed between them. Thus, carboxylic acids form dimers having structures corresponding to the formula



while alcohols form chains (or rings of various sizes)[6]:



Polymeric molecules containing hydrogen bonding groups interact through these groups both with other similar molecules to form molecular aggregates, gels, etc., and with low-molecular molecules and ions of appropriate types, to give either transitory or relatively permanent addition compounds. Proteins, for example, behave in this way. Many of their properties can be related directly to the hydrogen-bond-forming abilities of their component groups. An excellent start at a semiquantitative theory of the effect of hydrogen bonding in proteins on the reactivities of their polar groups (and also on the reactivities of their peptide and disulfide primary valence bonds) has been made by Laskowski and Scheraga[12].

Hydrogen Bonding and Solubility

Most substances containing both groups (NH, OH) capable of forming the hydrogen-donating ends of hydrogen bonds and electronegative atoms (N, O) which are good hydrogen bond acceptors, are relatively insoluble in solvents which cannot form even stronger hydrogen bonds with these substances than the latter form with themselves. Exceptions to this generalization are substances which satisfy the hydrogen bonding tendencies of their component groups as well (or better) by *intra*molecular hydrogen bonding as the *inter*molecular hydrogen bonding. For quantitative treatments one must of course consider the energies of the solvent-solvent, solvent-solute, and solute-solute hydrogen bonds, as well as various other energy and entropy factors.

Hydrogen Bonds in Protein Structures[1–3]

It has been conclusively established that hydrogen bonding plays an extremely important role in protein structure. Without hydrogen bonding, the relatively easy rotation around many of the bonds in the polypeptide chains would result in a high degree of conformational randomness—that is, in a wide variety of molecular shapes, hence a highly amorphous material. Such random structures do of course exist in denatured proteins, but not in proteins in their native state. In a fibrous protein such as keratin, for example, the normal structure of the polypeptide chain is a helical one, with internal NHO hydrogen bonds connecting consecutive turns of the helix[1–3,11,13–15]. Stretching (while hot, in the presence of water vapor, which facilitates the exchange of hydrogen bonds) extends the chains, causing the internal hydrogen bonds to break and new external hydrogen bonds (connecting adjacent chains) to form.

Collagen is also composed of helical chains, but the helix is of a different type from that in keratin. This can be related to the fact that collagen contains fairly high percentages of proline and hydroxyproline imino acid residues. It is impossible to fit the rings of these residues into a keratin-type helix. Also, these residues, unlike the others, contain no NH groups capable of forming NHO hydrogen bonds to carbonyl oxygens.

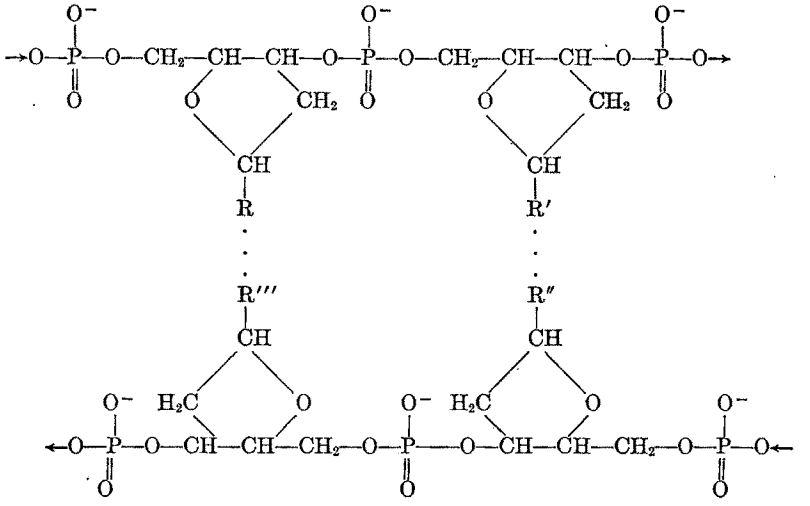
The structure of the collagen helices is yet to be decided. Ramachandran[16–18], Rich and Crick[19,20] have proposed structures in which three polypeptide helices twist around a common axis. NHO hydrogen bonds connect the helices of each triplet together, but this uses only half (or less) of the NH groups. From our knowledge of many other structures containing groups which tend to form strong hydrogen bonds, we should expect *all* of these NH groups to be involved in hydrogen bonding, but no suggestion has been forthcoming as to how this might occur, with the twisted triple helix structure.

The writer[1–3,21,22] has proposed a single helix structure, with all the NH groups hydrogen bonded and contributing to the stability of the structure. Extensive comparisons of the X-ray diffraction data with predictions based on these two types of structure should decide which of them, if either, is correct.

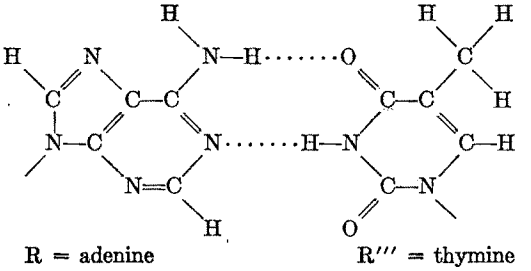
X-ray diffraction studies by Kendrew and co-workers[23] of the corpuscular protein, myoglobin, have yielded much detailed knowledge of the molecular structure. It contains a polypeptide chain, portions of which apparently have an α helix conformation, similar to that in keratin. There are, however, some quite sharp bends in the molecular axis and some of these have been attributed to hydrogen bond interactions between specific side chains.

Hydrogen Bonds in Nucleic Acids

Another example of the structural importance of hydrogen bonding in biological systems is found in deoxyribonucleic acid. There is a considerable amount of evidence[24,25] in favor of the general correctness of the structural model for this substance, proposed by Watson and Crick[26]. In this, two polyphosphate chains of composition



are helically coiled around a common axis. In this formula R, R′, R″ and R‴ represent purine and pyrimidine groups, connected together in pairs by hydrogen bonds. For example, adenine and thymine groups might be held together in this way:



R = adenine R‴ = thymine

Although many of the structural details are still uncertain, there seems to be little doubt that the arrangement is determined in large part by the steric requirements of hydrogen bond formation between the purine and pyrimidine groups. The unusual and important properties of this and related substances are probably also closely related to the hydrogen bonding.

It may be noted that the oxygen atoms and OH groups attached to the phosphorus atoms would both be expected to form strong hydrogen

bonds—with like groups, to form OHO hydrogen bonds, for example. Their role in the structure and in the chemical and biological properties of the molecule needs to be clarified.

Conclusion

It is evident from what has been written that hydrogen bonds play a very important role in biology, both with regard to the structures of substances and systems of biological interest and with regard to the types, mechanisms, and rate of the reactions which they undergo. Much of the present knowledge concerning the properties of hydrogen bonds in biological systems is, however, only qualitative. If (or when) this knowledge is put on a quantitative basis, a much better understanding of the behavior of biological materials and of possible ways of changing that behavior will become possible.

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MOLECULAR PHYSIOLOGY—ANALYSIS AND SYNTHESIS

By WILFRIED F. H. M. MOMMAERTS

> "How unfortunate," he said, "that we have not a long series of lives at our disposal, for we find universe within universe the further we explore, like those Japanese lacquer boxes that you buy in Port Said, the smallest of which is so tiny that you can't open it at all." It was precisely in the last and most infinitesimal cell that the truth of all things lay concealed.
>
> GUY DE POURTALES—"La pêche miraculeuse."

WHATEVER the extent to which the author of the quotation may have been cognizant of its wide scientific implications, there is no doubt that it represents one of the most concise summaries of the character which analytical biology has gradually assumed. The living organism presents itself to us as an object of study, which we can approach descriptively or experimentally. But when inquiring about the mechanism of its actions, we come up to the fact that these mechanisms are concealed within the appearance of the object, which is like the outer box in the analogy. So we open this box, with greater or lesser difficulty, only to find another one inside; and so on, until we are left with the smallest box we can handle. This, we think, must be the last one to keep the secret from us. Biology has followed this course, ever since it began to rely upon ever more powerful microscopes with which to observe the machinery of life. But each time a higher resolution was achieved, the secret appeared to be hiding within the next smaller entity. This is precisely the attitude of a child facing an unknown machine such as a cigarette automat, coming to the conclusion that "there is a man inside." Throughout the development of microscopy, polarization- and ultramicroscopy, and nowadays electron microscopy, we have been looking for that little man, in the cells, the nuclei, the mitochondria, the fibrils and now within their submicroscopic constituents. Each time he seemed to be waiting in the next smaller box, but, having arrived there now, we may as well realize that, meanwhile, he has quietly slipped away. Because, while the analytical investigation of biological mechanisms unavoidably leads us to smaller and smaller entities, when we proceed in this fashion we gradually lose the complexity and integration which, fully as much as the mechanism itself, are also an essential part of our original problem.

Be this as it may, this analytical method seems to be inherent in our approach to the biological fundamentals. This is not wrong, as long as we are aware of the paradox, and do our best to supplement our progress along the line of "dissection" with some synthesizing considerations. We should always remember Dean Rusk's dictum: "A respect for complexity is the beginning of wisdom."

Much has been made, at times, of the analogies between organisms and man-made machines, not only at the level of gross mechanical systems such as a moving limb or the pumping action of the heart, but also with respect to the more fundamental machinery. But here we meet with a fundamental difference: man-made machines, large or small, are "macroscopic" in the sense of fundamental physics. They consist of large assemblies of molecules which, by virtue of energy acquired in a chemical reaction or otherwise, exert forces upon macroscopic parts of the engine. By contrast, whatever the diversity of their mechanisms, we are probably justified in stating that all biological mechanisms are "microscopic" or molecular, in that a single "engine molecule" interacts with a single "fuel molecule," in such a manner that the fundamental event takes place as a topochemical reaction, with the proviso that the specific biological organization is such that these elementary events are cooperative and lead to the appearance of a macroscopically manifest phenomenon. These were the considerations that were fundamental in 1950 to my choice "Muscular Contraction, a Topic in Molecular Physiology" as the title of a book.

This was not the first time the term molecular physiology, or molecular biology, had been used. It was first introduced, to my knowledge, in the writings of Carl Nägeli about a century ago, as the subject of much prophetic thought as well as some polemics. In later times, the over-all concept was less strongly emphasized, although major directions of research developed in the ways Nägeli must have foreseen. The polarization microscope did much to reveal the ordered structure of the submicroscopic cellular parts, and the concept of "micelles" introduced a common viewpoint in colloid chemistry and biology. Not all phases of the development of colloid chemistry were fruitful to biology. It went through a period in which it distracted biology with sometimes inexact concepts of aggregation, coagulation, hydration, and surface properties, of which remnants are still encountered. But all that changed when colloid chemistry too became "molecular," and, nowadays, macromolecular chemistry is one of the strongest of all the foundations upon which biology has ever built. While the development of polymer chemistry is still leading to increasingly exact theoretical understanding of the behavior of giant molecules, the first great advance came when Svedberg began to demonstrate that proteins, rather than being ill-defined aggregates, are specific molecular entities, even though their aggregative behavior may be more complicated in some cases than the initially selected examples appeared to suggest.

Having considered that microscopy has revealed a graded series of structural units of decreasing dimensions, we should not forget that Nature has also chosen such a series in the inverse direction, with respect to the sizes at which organisms have stabilized. Dimension alone, without

reference to standards, is not relevant as such. An elephant and a bat, and these are not even extremes, differ a millionfold or so in mass, yet each of them has a comparably complex organization as the substrate for highly developed vegetative, perceptive and behavior functions. But, below a certain volume, there just is not enough room any more, and so we find among "microbes" a wide variety of organisms that have refrained from all social ambitions and aims at personal aggrandizement, and concentrate on perhaps only a few of the basic attributes: metabolism, growth, multiplication, heredity, irritability, and motion. They display a wide variety in these functions, which together with their relative simplicity have made them excellent objects for the study of comparative biochemistry and fundamental genetics.

At one time it must have seemed as if most microbes, some exceptional giants aside, were of a not too diverse class of dimensions. This was, of course, illusory, being due to the fact that below a certain dimension they were no longer visible, a limitation set by the wave length of visible light, not by a cytologically essential limit. Yet the coincidence presents itself that not far below this limit, irritability and motion already having been de-emphasized, organisms stop being organisms, and begin to lose some of their essential capabilities, specifically those of independent growth, metabolism and multiplication, while maintaining only a certain morphology and specific macromolecular constitution as the matrix for hereditary coding and biosynthesis. The limits are indistinct, and overlap widely. We may, perhaps, call a "microbe" any small organism that is capable, in principle, of independent growth and metabolism (some exceptions to be regarded as a secondary reversal to obligatory parasitism, as we even find such parasites among unquestionably higher organisms); and call a "virus" an entity dependent for metabolism and growth upon its integration into the metabolism of a host cell. But the microbes of the Pleuropneumonia group, recognized as filterable saprophytic organisms by Elford and Laidlaw, are smaller than many typical viruses. One transition, then, from macroscopic to molecular biology can be followed by studying the descending complexities offered by microbes and submicrobes ranked by their diminishing dimensions, a region discussed by Kluyver in his famous lecture "Life's Fringes."

Along this line we will observe entities entirely specialized toward the simplest manifestations of a single mechanism such as the use of a nucleic acid structure for an informational code; we will meet with exceptions to the rule of the total loss of metabolic activity, like the retention of adenosinetriphosphatase activity by fowl leucosis virus, instructive for the understanding of the integration of the virus into the host's metabolism. And we will have the opportunity to observe that even when the molecular dimensions are approached, there still

is variety in the morphology of even the smallest units. It is the study of these hereditary and biosynthetic activities in viruses, and of comparable phenomena in higher organisms, which at this moment, rightly or wrongly, seem to pre-empt the term "molecular biology." This is, perhaps, an undue restriction upon an attractive word, but we shall let this take its own course.

The unprecedented success of this part of molecular biology during the last few years has already begun to give rise to a few "second thoughts." Two groups of these are really quite insignificant and can be ignored. The one group originates from critics who are remote from this present day activity, and wish it would just go away. They may refer to it as just a fad, forgetting perhaps that, for example, comparative anatomy once was such a fad too, and a very intolerant one at that. The other group has its roots in the uninformed zeal of some participants in some limited area of molecular biology or biophysics who, not appreciating the overwhelming wealth of biological materials and manifestations, simply act as if these do not exist. G. G. Simpson, in a recent article on the relation between reductionist and organismic biology, has quoted a few expressions that fall in this and the previous category, one of which: "Biology starts with biochemistry and goes on to neurophysiology and genetics; all else is stamp collecting" is enough to make an informed biologist suspicious of the maturity of molecular biology and its adherents. The truth is, of course, that the value of molecular biology is not diminished by the expressions of extremists of either category; its results speak for themselves.

Still, there is place for critical reflection. Molecular biology, as now practiced in what is currently its prime field, is predominantly analytical, looking as it is for that infinitesimally small box that holds the ultimate truth. This is temporary, there is so much to discover just now! It certainly is no reason to predict its forthcoming doom. Still, if only to counterbalance the opinion of non-biological outsiders, it may be wise from time to time to voice a reminder that biological systems are characterized by their multiple levels of organization, and that the comprehension of this complexity is fully as much a part of biology as is the recognition of its composite mechanisms. This has recently been set forth by C. Grobstein for the problem of cellular development and tissue differentiation, and we shall now proceed to examine the same standpoint for the field of physiology.

Higher animals share with microbes the fundamental features of metabolism, growth, and heredity, albeit often in more complex forms, following the order of sequence commensurate with increasing size and diversification. But other functions, notably those of specialized motion, excitability, and perception, although found in unicellular organisms and reaching remarkable specialization in complex Protozoan cells, are, in

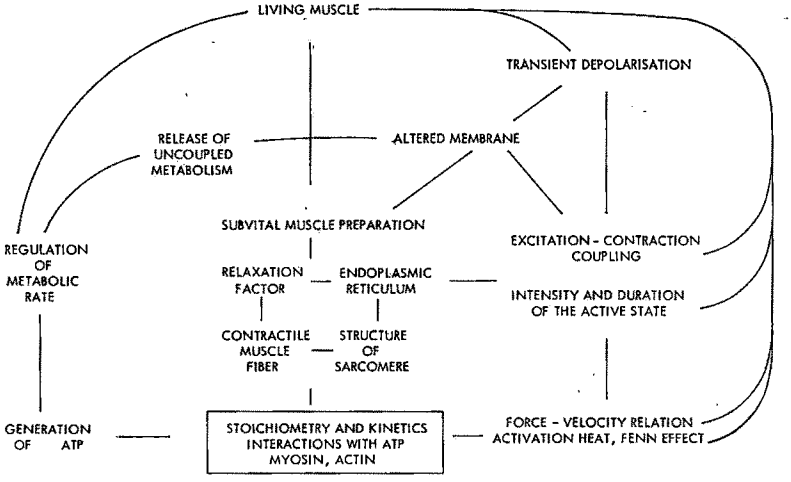
the main, typical properties of macrobes, and so the other organizational sequence will be found within the cells of multicellular organisms. While the choice may be personal, I cannot resist selecting three favored problems. The one is the nature of transport mechanisms and their relation, as far as ion distributions are concerned, to the fundamentals of excitability and its link to cellular responses; these are vast problems of wide occurrence and profound implications, now sometimes hidden behind facetious terms as sodium-pumpate and calcium-linkate. Even without its relation to excitation, active transport is a fundamental feature of living cells, one which lends itself promisingly to biochemical analysis, and begins to display fascinating relations to other areas of molecular biology. Secondly, there is the problem of light perception in the retina, because the receptor structures are well described, and the primary event is of a quantal nature, accessible to stoichiometric-photochemical description, and apt to lead to an understanding of the amplifier properties of the whole cell. And, lastly, there is muscular contraction, as the most highly specialized example of the chemo-dynamic transformations so fundamental to all forms of life.

The latter example lends itself particularly well to a discussion of molecular physiology and its relation to synthetic and integrative approaches. It is not the purpose of this article to supply detailed factual information, but it is common knowledge how much has been gained, since the fundamental discoveries by Szent-Györgyi in 1942, in the identification of actin and myosin as the components of the contractile structure, and of their interaction with ATP as the basis of mechano-chemical coupling. This is not meant to say that the primary mechanism of contraction has been explained, on the contrary; but progress during the last 20 years has been about as good as can reasonably be expected for a problem of such magnitude and fundamentality. What is less generally realized is how much we have advanced already, on the level of experimental investigations actual or contemplated, in the integration of these molecular mechanisms into higher organizational systems. The scheme (Table I) may be helpful in focusing upon these interrelations; some parts of it, being obvious, are not further commented upon in the text.

The subjects at the bottom-center of the scheme represent molecular physiology at its extreme, and stand in the closest analogy to present-day work in molecular genetics. From here, upwards to the next organizational niveau, we meet with the extracted muscle fiber preparations that contract with adenosinetriphosphate, and with the actual structures of the contractile matter as investigated electron-microscopically. But, all around these reduced systems, we meet with problems of specific physiological complexity.

It is known that the contracting muscle involves energy for its activa-

tion which it liberates as heat, and that it involves additional energy equal to the work done (Fenn effect). These phenomena, for their interpretation, will have to be linked stoichiometrically with molecular events in actin and myosin, a connection which has not yet been made experimentally accessible; but there is now significant progress in the difficult task of quantitatively accounting for the magnitude and time course of energy release in terms of metabolic reactions, which in turn are conjugate with these molecular events. Secondly, the muscle contracts at a velocity determined by the force (or vice-versa), which must reflect a specific kinetic feature of the actomyosin transformations, likewise not yet formulated in experimentally approachable terms, but at least recognized as a future problem. This force-velocity relation, as we have discovered in heart muscle (bearing out a prediction by A. V. Hill)



is not a constant function, but depends on excitation parameters suggesting that properties of the excitatory event determine the molecular kinetics of the contractile components.

So much about events when the muscle is active. What turns the activity on and off? On or near the molecular level, we begin to understand the role of the relaxation factor, and the significance of the endoplasmic reticulum which may be the immediate site of activation, perhaps by releasing and reabsorbing calcium ions. On the biophysical and physiological level, there is the concept of the active state (which must eventually be formulated in terms of a molecular change in actin or myosin), of which the intensity and duration are determined by the act of stimulation, raising the question as to the nature of the coupling of excitation and contraction, apart from its morphological allocation in the endoplasmic reticulum and the presumable role of a calcium link.

Excitation itself is accompanied by depolarization of the cell membrane, but permanent depolarization with potassium ions leads to a lasting increase in metabolism without maintained contraction. Metabolism can thus be elicited independently of contraction, and a muscle with such an altered membrane displays only part of its full set of vital activities. Still "less alive" is our subvital muscle preparation, which can contract with the ATP made in its own intact mitochondria from added metabolites. Thus, we have at our disposal a graded series of systems, from pure contractile proteins to the whole living cell, each displaying specific features and allowing the investigation of limited questions at various levels of organization. This is synthesis from molecular physiology upward, not in the philosopher's chair, but in the actual conduct of experimental work. Stamp collecting?

This is not a list of finished accomplishments, but of problems being investigated in our laboratory, with the definite aim to practice the synthesis of molecular physiology into cell and organ physiology in the practice of laboratory research. This aim, the reader will notice, is very similar to what Grobstein has urged for the field of cell differentiation and organic development. Solved or not, the occupation with such questions absolves us from the objection that molecular biology removes itself from the essence of biology proper. Conversely, it shows that in molecular biology there are other fields than those which during the last year or so have begun to monopolize the name. To use another of the gems collected by Simpson: "Genetics, the central discipline of biology, has as its ultimate subject a group of chemical compounds. Genetics requires knowledge of the physical sciences but no other knowledge in the field of biology itself," I hope that this essay, speaking for one section of physiology, will show that there are other biological disciplines that are worth studying, even by that special timely criterion of being relatable to molecular mechanisms.

I have already referred to Grobstein's recent article on "Levels and Ontogeny," and wish to acknowledge how much it has stimulated the final writing of these considerations. His opening sentence was: "I have three objectives in writing this essay—to reiterate the importance of multi-leveled analysis in biology, to illustrate the particularly crucial connection between the concept of levels and the process of development, and to join those who advocate the thesis that recent spectacular biological progress through the application of molecular approaches will be self-defeating if it leads to atrophy of interest in, and understanding of, supramolecular problems and approaches." To make this quotation applicable to the present article, one needs only to replace the "process of development" by the "processes of cellular and organic activity"; and to make it conform to my convictions, I would place special emphasis on the "if," near the end of the quoted sentence.

The given exposition of our program in molecular physiology will, I hope, have illustrated that this condition can be amply fulfilled.

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An extensible model of the electron is the title of a recent communication by P. A. M. Dirac, F.R.S. and Nobel Laureate, in the *Proceedings of the Royal Society*, A, *268*, 57 (1962). In this mathematical article it is proposed that the electron "should be considered classically as a charged conducting surface, with a surface tension to prevent it flying apart under the influence of the charge." Dirac points out that "such an electron has a state of stable equilibrium with spherical symmetry, and, if disturbed, its shape and size oscillate." He deduces the equations of motion from an action principle and obtains a Hamiltonian formalism. When the energy of the first excited state with spherical symmetry of the electron is worked out according to the Bohr-Sommerfeld method of quantization, Dirac finds the energy to be about 53 times the rest-energy of the electron. He suggests that this first excited state may be considered as one of the elementary particles, a muon (see AMERICAN SCIENTIST, *50*, 252 (1962). Dirac observes, finally, that his present theory of the electron can only be a rough one and cannot accurately agree with experiment since his model gives no spin to the electron. It serves, he says, "to indicate how one can look upon the muon as an electron excited by radial oscillations. It gives to the muon the same spherical symmetry as the electron, so that the muon cannot disintegrate into an electron and a photon, and would only have a very small probability of disintegrating into an electron and several photons."

HERE AND THERE

By the Board of Editors and the Membership of The Sigma Xi-RESA Societies

In a press release on February 14, 1961, The Metropolitan Museum of Art in New York announced that "as a result of recently completed studies, its three 'Etruscan' terracotta statues must be considered of doubtful authenticity." The three terracottas, a large warrior, a smaller warrior, and a large helmeted head, were acquired between 1915 and 1921. For some years there had been conflicting claims about these statues based mainly on stylistic grounds. A series of technical and scientific studies undertaken by the Museum developed "convincing proof that these famous statues were not made in ancient times."

The record of these studies has now been published with a Foreword by the Museum Director, James J. Rorimer, as Occasional Papers, No. 11, with the title *An Inquiry into the Forgery of the Etruscan Terracotta Warriors in the Metropolitan Museum of Art*, by Dietrich von Bothmer and Joseph V. Noble, copyrighted by the Museum in 1961. Through the courtesy of the Director, AMERICAN SCIENTIST is permitted to present here the scientific evidence accumulated by Mr. Noble, supplementing the History of the Terracotta warriors by Mr. von Bothmer, which also sets forth the stylistic arguments and finally the story of the forgery by one of the participants, Alfredo Adolfo Fioravanti, in a deposition signed before the American Consul in Rome on January 5, 1961. Fioravanti had possession of the left thumb of the big warrior "but also two test pieces of terracotta with the black glaze and the accessory colors, as well as bits of unfired clay similar to that used in the body of the big warrior and the colossal head." The quotation following, from the record of Joseph V. Noble, reveals the nature of the scientific evidence, incontrovertibly disproving the authenticity of the figures:

> During 1958 I became interested in the practical applications of the late Theodor Schumann's explanation of the ancient method of producing the Greek black glaze. Dr. Schumann had discovered that the ancient black glaze was made from the same red clay as the body of Greek vases, with no coloring agents added to achieve the black color. He reproduced it by using a peptized clay slip as the glaze matter and subjecting his pottery to oxidizing, reducing, and reoxidizing firing conditions.
>
> The chemistry of the process is as follows. Red ferric oxide (Fe_2O_3) is present both in the clay body of the vase and in the unfired black glaze matter, which is made from the clay by a

deflocculating separation process that extracts only the finest particles of the clay. The particles, mixed with water, are used as a slip for the decoration of the vase. If the entire firing were done under oxidizing conditions (in which air is allowed to enter the kiln), both the vase and the glaze would turn, and remain, red.

In the middle of the firing cycle, however, the oxidizing atmosphere is changed to a reducing atmosphere by the introduction of green wood or damp sawdust. The smoke produced by this action is not important chemically as it is largely composed of carbon, which does not affect the coloring of the black glaze. The reducing atmosphere, with the kiln closed to the outside supply of oxygen, causes incomplete combustion of the green wood or wet sawdust and produces carbon monoxide gas (CO), instead of the carbon dioxide gas (CO_2) which would be developed during normal, complete combustion. Carbon monoxide unites with any oxygen that it can obtain. In this case it combines with part of the oxygen in the ferric oxide (Fe_3O_3) of the clay. This changes part of the carbon monoxide into the stable form, carbon dioxide, and the red *ferric* oxide (Fe_2O_3) into *ferrous* oxide (FeO), which is black. The reaction in this process is $Fe_2O_3 + CO = 2FeO + CO_2$. Water vapor in the kiln, from the moisture of the pottery, from the green wood or wet sawdust, or perhaps even from a vessel filled with water in the kiln itself, also produces a black glaze, a magnetic oxide of iron (Fe_3O_4), which is even blacker than mere ferrous oxide (FeO). The reaction in this process is $3Fe_2O_3 + CO = 2Fe_3O_4 + CO_2$. If the firing cycle were stopped at this point, the clay body of the vase and the glaze would be completely and permanently black.

The process is concluded by a reoxidizing phase. Through a small hole now opened, oxygen is again permitted to enter the kiln; it unites with some of the black ferrous oxide (FeO) or magnetic oxide of iron (Fe_3O_4), turning it back into the red ferric oxide (Fe_2O_3). The porous clay body of the vase readily allows this to happen, and consequently unglazed portions of the surface and most of the body again turn red. On the other hand, the glazed areas have sintered and do not reoxidize at the temperature of this last phase, which does not exceed 1000°C. These areas remain black, and the result of the three-phase firing is the red and black coloring characteristic of ancient, especially Attic, vases.

That this was the ancient Greek and Etruscan technique of making black glaze can be scientifically confirmed. Spectro-

graphic analyses of the body and the glaze of genuine classical pottery are identical, except for a slightly greater concentration of the heavier minerals in the glaze. They do not show any added coloring agent in the black glaze. The spectrographic method is, therefore, an excellent way to differentiate between genuine ancient vases and imitations.

This test would prove at once whether the fired black glaze of the warriors was indeed an ancient one . . . . If it were proved that the black glaze was not identical to the ancient glaze, but included an added coloring agent, then it was certain that the figures were forgeries.

In January 1960, spectrographic analyses of the clay body and the glaze of the three warriors were made for the Museum by Lucius Pitkin, Inc., a technical laboratory in New York City. For comparative purposes, analyses were also made of an Etruscan architectural tile that was known to be genuine. The spectrographic analyses revealed that in the tile there was only a trace of manganese present in the glaze, whereas in the black glaze of the three warriors there were very substantial amounts of manganese. The disproportionate amount of manganese indicated that it had been added to the glaze of the three warriors as a coloring agent. The use of manganese in a ceramic glaze was, of course, known in ancient Egypt and the ancient Near East, but it was not employed in Greece in classical times. Manganese was not used in Italy until the introduction of the manufacture of maiolica, of which it is one of the chief coloring agents. In modern times it was used for the reproduction of *bucchero* vases in the workshop of Alessandro and Antonio del Vita, and it was even assumed in Italy that ancient *bucchero* was made with manganese. We already knew, through spectrographic analysis, that one of the two Tarquinia imitators of Attic and Etruscan pottery, Scapini, employed cobalt, lead, and manganese as coloring agents for his excellent reproductions of the Greek black glaze.

The old warrior does not have any more manganese in the clay body than the amount normally present in most central Italian clays, Etruscan or modern; the amount is identical, in fact, to that in the Etruscan architectural tile. Therefore, in the old warrior the concentration of manganese in the black glaze differs from the amount present in the clay body. The colossal head and the big warrior, however, have manganese also added to the clay. This was probably done to darken the color of the terracotta to imitate the gray color of the interior of fractures of ancient terracotta pieces that had been subjected to a three-

stage firing, or to copy *bucchero* ware. The series of spectrographic analyses, therefore, proved that the warriors were not made by a procedure known in classical antiquity.

To check this conclusion, an additional test was devised. Genuine ancient black glaze does not survive a temperature much above 1025° C., for at that temperature the black ferrous oxide begins to reoxidize to red ferric oxide and the black color is lost. On the other hand, a glaze made with a ceramic coloring agent, such as manganese, cobalt, or lead, will remain black at this temperature. A specimen of the glaze of the Etruscan architectural tile was heated to 1050° C.: the black reoxidized and became permanently red. This proved that this Etruscan glaze was similar in structure to Attic black glaze. Samples of the black glaze of the three warriors were subjected to the same 1050° C. temperature, and they remained black and unchanged. This second test gave corroborative proof that the glaze of the three warriors was not made by the ancient technique.

The next step, in order to draw the net tighter, was to ascertain the composition of the glaze of large-scale Etruscan or so-called Etruscan terracottas abroad. Spectrographic analyses of a specimen of the Castellani sarcophagus in the British Museum, an acknowledged forgery, revealed that the coloring agent was lead, whereas analyses of samples from genuine Caeretan slabs in the British Museum and in the Louvre agreed with those of the architectural terracotta in New York. Owing to a reorganization of the Villa Giulia in Rome, requests for samples from the Veii terracottas were not answered immediately, but when the specimens were obtained, they, too, confirmed the assumption that the glaze of large-scale Etruscan terracottas was achieved in exactly the same manner as the famous Greek glaze.

An examination of the edges of breaks in the warriors revealed that the clay body is uniform in color all the way through, indicating that the statues were fired only under oxidizing conditions. Pottery fired under the three-stage firing conditions usually is grayish in the core of the terracotta and reddish near the outside.

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Despite the enthusiasm with which the union is consummated, it is not notably stable. If the bottle cap stays on tight, the complex will probably keep all right as the pyridine vapor drifts in and out of the chromium oxide. Since the function of the pyridine is to tie down the oxidizing potency of the CrO_3, the contents of this particular bottle may not weaken with age.

The material has recently been found to be a good room-temperature oxidizing agent even outside the steroid fraternity. It converts benzylic and allylic alcohols to their corresponding aldehydes and simple aliphatic and aromatic secondary alcohols to ketones. Source: (*J. Org. Chem. 26,* 4814).

Source for the reagent at the above price is Distillation Products Industries, Rochester 3, N. Y. (Division of Eastman Kodak Company).

How to fabricate photographically

"Photography provides a quick, easy, and reproducible system of weight control," says a new data book of ours, illustrating the perils in out-of-context quotations. The weight control it talks about has nothing to do with embonpoint, and the photography isn't even based on silver halide. It is based on the photosensitivity of certain monomers and on the chemical resistance that the resulting polymers have toward reagents as mighty as hydrofluoric acid and aqua regia.

The growth of this branch of photography proves it was just what the space age was waiting for; the reduction in weight of the age's playthings without sacrifice of strength is but one aspect of photography's importance as the newer kind of fabrication. The changes it has wrought in the whole art of electronics are now recognized in financial circles as profound. If it has gone that far, readers here addressed will have doubtless passed some time ago the stage of wonderment at it all. Nevertheless, guidance on technique may still be badly needed.

The new book doesn't so much guide as inform. (Let your conscience be your guide, assisted by a lawyer who closely follows the patent situation proliferating as a result of industry's heavy investment of brainpower in the field. Most important firms are reasonable about licenses.) The book brings together a lot of hot tips on preparing the many kinds of substrate, applying our various resists, preparing the coatings for exposure, developing them, choosing the proper etchants or electroforming solutions for use with stainless steel, glass, gold, germanium, etc.

If you happen to know a Kodak Graphic Arts Dealer, call up and ask for the $1 data book, "Kodak Photosensitive Resists for Industry." If you don't even know what a Kodak Graphic Arts Dealer is, send the dollar to Eastman Kodak Company, Graphic Arts Division, Rochester 4, N. Y.

Ruddy-faced fellow, isn't he?



He is showing a color photograph of terrain. A skillful photographer can do an excellent job in black-and-white of *suggesting* the color of a man's complexion. Terrain photography is another matter. There suggestion is worth an awful lot less than objective information. For geology, for forestry, for hydrography, for stream-pollution studies, and for just about every other field of photo-interpretation, few will dispute that color—and particularly color *change*—add several dimensions to information capacity.

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THE SCIENTISTS' BOOKSHELF

By Hugh Taylor, the Associate Editors, and Guest Reviewers

SEE INDEX AT END OF THIS SECTION

The Photosynthesis of Carbon Compounds by MELVIN CALVIN and J. A. BASSHAM; 127 pages; $5.50; W. A. Benjamin, Inc., 1962.

Fifty years ago a query as to photosynthesis could be adequately answered by a basic over-all equation,

$$CO_2 + H_2O \xrightarrow{h\nu} O_2 + (CH_2O)_x,$$

which indicated that sugars $(CH_2O)_x$ and oxygen resulted from the interaction of carbon dioxide and water under the influence of light, which could be measured in quanta, $h\nu$, several of these being required for the production of one sugar molecule $(CH_2O)_x$. Fifteen years ago van Niel proposed that the photochemical part of the photosynthesis was separable from and independent of the reduction of carbon dioxide. How research in the intervening years has progressed to the point that the senior author of this volume could be awarded the Nobel Prize in Chemistry for 1961 is the theme of this book.

This is a typical story in the development of science. What was impossible twenty-five years ago became possible by reason of progress in areas remote from the subjects of photosynthesis. As one reads the story it is evident that the new researchers depended heavily on new tools, chromatographic analysis, the use of radioactive C^{14} as a tracer tool, and autoradiography to determine the extent to which this radioactive carbon could enter into the photosynthetic cycle. Figure 3 on page 15 of this book is eloquent testimony of the value of these new techniques. It reproduces the results of a radioautograph of a two-dimensional paper chromatogram on an alcoholic extract of the alga, *Chlorella pyrenoidosa*, after 10 sec photosynthesis with $C^{14}O_2$. Identified in the chromatogram are alanine malic acid, aspartic acid, PEPA (phosphoenolpyruvic acid), triase phosphates, PGA (3-phosphoglyceric acid), sugar phosphates, and sugar diphosphates. Two co-factors, ATP and TPNH, phosphorus-containing compounds are "at present the only ones known to be generated by the light reactions of photosynthesis" and at the same time seem to be required for steps in the carbon reduction cycles. The carbon dioxide is incorporated by a carboxylation of a diphosphate resulting from the interaction of light with water, oxygen, and the phosphorus-containing compounds noted already. Enzyme systems in the plant are essential to these processes.

The over-all equation previously cited covers therefore a multitude of processes in which not only sugars, but amino acids, carboxylic acids such as malic acid, saccharides, fats, pigments such as carotenoids, chlorophyll and heme, aromatic nuclei and other biosynthetic products are synthesized from water, carbon dioxide, and ammonium salts. In seventy-five pages the authors take you on a conducted tour through the labyrinth of their exploratory efforts and then provide, in a further fifty pages, three reprints of basic published researches in this field. Altogether this is a most satisfying and exhilarating record of the resources of modern science applied by skillful scientists to the solution of age-old problems.

McGraw-Hill Yearbook of Science and Technology, 1961 Review, edited by WILLIAM H. CROUSE, *et al.*; 558 pages; $17.50; McGraw-Hill Book Co., 1962.

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sampling, the fifteen-volume *Encyclopedia of Science and Technology*, giving it a very warm reception for the breadth and quality of the work. Now, the publishers have decided to supplement their effort with a review of scientific developments in 1961 and a preview of 1962. The first sixty-four pages contain five major articles on Basic Research, Man in Space, Mind of Man, Man and the Sea, Man and Intelligent Machines. The quality of these articles can be judged by the names of the authors, respectively, Alan T. Waterman, Werner von Braun, Harry F. and Margaret K. Harlow, Athelstan Spilhaus, and Simon Ramo. The article on Man in Space is a dispassionate and thorough evaluation of the progress attained. What the Russians did with their orbiting Vostoks is set over in detail against the beginnings of Project Mercury and the sub-orbital flights of Shepard and Grissom. The three-orbit flights of Glenn and Carpenter will be part of the 1962 record. In Man and the Sea, Dean Spilhaus does for the oceans what von Braun has done for the air. The five main articles are followed by sixteen pages of photographic highlights. Photography of scientific products, processes and operations has now reached a superb level of aesthetic delight as well as scientific utility.

Next comes the Review of 1961 from A to Z in 440 pages followed by thirty pages of contributors and index. The reviews are of 300 or more items and the range is from acoustic masers through aerodynamics to zeolite molecular sieves and Mrs. Leakey's find in Tanganyika Territory of the Zinjanthropus Skull. An AMERICAN SCIENTIST article by Katz on Deuterium in Biology is abstracted in one such item. New penicillins, Woodward's total synthesis of chlorophyll *a*, the chromosome fine structure, amino acid sequences in tobacco mosaic virus, the structure of hemoglobin, nuclear fusion and gas chromatography, were among the items which particularly interested the reviewer. The illustrations and diagrams are superb.

One final observation: these volumes will have an appeal to an increasingly large segment of the scientific community, those whose active days in the pursuit of science are behind them, who cannot keep up with the ever-growing volume of scientific literature but who will find, in these annual reports, the rich record of progress and at a price, even though they do not possess the fifteen-volume *Encyclopedia*, not greatly in excess of a theatre party for two in town.

Planets and Satellites, edited by GERARD P. KUIPER and BARBARA M. MIDDLEHURST; 601 pages; $12.50; University of Chicago Press, 1961.

This book is the long-awaited Volume III of *The Solar System*. Volume I (*The Sun*) was published in 1953 and is now the standard reference book on solar physics. Volume II (*The Earth as a Planet*) was published a year later and is a well-known reference on geophysics. Now, after seven years, Volume III (*Planets and Satellites*) has appeared. It was worth waiting for, however.

As in the previous volumes, the individual chapters were written by experts. Many of the chapters are devoted to summaries of basic observational data on the planets. For example, a chapter by Dollfus summarizes polarization measurements and their interpretation. These measurements are the basis of a significant portion of our quantitative information on the surface and atmospheric conditions on the planets (Mars, in particular). Two other chapters by Pettit and Sinton review the infrared measurements which are the source of most of our knowledge of planetary temperatures, while a chapter by Harris reviews photometric and colorimetric measurements and their results. Finally, a chapter by Brouwer and Clemence is devoted to a summary of our present knowledge of masses and orbits. These are nice summaries and are one of the features which give this book permanent value.

Three chapters are devoted to radio measurements of the planets. A chapter by Mayer reviews these very important observations (obtained only during the

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ROBERT HOOKE, by Margaret 'Espinasse: A biography of the first Curator of Experiments of the Royal Society of Britain: Cal 65, $1.95 DIALOGUE CONCERNING THE TWO CHIEF WORLD SYSTEMS, by Galileo Galilei, translated by Stillman Drake. Cal 66, $2:95

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Berkeley 4

past few years) and briefly discusses their interpretations. As an example, radio measurements have revealed a temperature of 600°K. on Venus. This observation has stimulated a great deal of theoretical effort to produce a model atmosphere for Venus which contains a region having a temperature of 600°K. These efforts have greatly increased our knowledge of Venus because it now appears that only a model atmosphere with a 600°K. surface temperature produced by a large greenhouse effect can satisfy all the observations. Two other chapters by Burke and Gallet discuss the equally interesting non-thermal radio noise emitted by Jupiter.

Four chapters are concerned with visual observations of the planets. The most extensive of these is another chapter by Dollfus. This article contains a wealth of photographs and drawings of the planets obtained at Pic du Midi. The other chapters contain photographs of the earth taken by the Tiros satellite, spectacular photographs of the planets taken with the 200-inch telescope, and some impressive color photographs of Mars. Unfortunately, the printing process used to make plates in books cannot reproduce as small contrast differences as the photographic process. As a result, much of the information on the original negatives can be lost. Hence the reader cannot always expect to see on the plates all the low contrast details that the author can see on the original negatives and which he describes in the text. This seems to be the case in the photographs of Mercury reproduced in plate 3 (following page 554), for example.

One of the remaining chapters, by Wildt, is devoted to the complicated problem of planetary interiors. An important feature of this chapter is a critical review of the theoretical and experimental properties of matter under high pressures. Other chapters are devoted to the stability of the solar system, the Moon, the discovery of Pluto, and the completeness of our knowledge of the membership of the solar system.

It would be impossible to escape the conclusion that this, like the preceding volumes, is an excellent book. Much of it is of permanent value. The many experts who have contributed insure that it is authoritative and, moreover, the book has an attractive format. Furthermore, for its quality and size the price is low. Volumes IV and V of this series are anxiously awaited.—*Robert E. Danielson*

Introduction to Plant Ecology by M. Ashby; 249 pages; $5.75; St Martin's Press (Macmillan & Co., London), 1961.

This is an interestingly written book on plant ecology by a British ecologist. Maurice Ashby is obviously a superior teacher and a good student of ecology. One appreciates especially the stimulating way he has illustrated various ecological concepts in the text. The diagrams and figures he has selected to go with this are good and well chosen. The student will find intriguing the constant recurrence of enticing new ideas everywhere tied to what to him are new and unrealized facts of relationships. For example, Ashby cites the situation of the butterfly orchid which was found to grow well and abundantly in a sheltered meadow, and although it also grew well in an exposed meadow it was rare there. Its rarity in the latter habitat was shown to be related to the ecology of the pollinator, a moth, which was practically excluded from the windy site. He describes the ingenious method by which this relationship was worked out. And Ashby goes on to point out that in exposed, windy habitats in England insect pollinated plants are comparatively few compared with places protected from winds. This plant-animal-microclimate relationship is the sort of thing that, in many forms, he has brought together in his book from the literature and from his own studies to make it lively, and to make his point that ecology "... is a way of looking at life, always attempting an integration that may help in understanding the whole community."

This example illustrates to a certain extent Ashby's objective throughout the book. Although he devotes only a small section to the plant community, the first two thirds of the book on the ecology of the species is in the frame-

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work of the species as an element of the community. He emphasizes that an understanding of the "behavior" of species is basic to our understanding of the plant community and thus devotes a major amount of space to autecology.

It is interesting to note that the ecology of the species is reducible to (1) tolerance and (2) aggression. Tolerance is introduced by: "Of the various species that arrive as colonists in any area some will thrive, but for others the conditions of the habitat will be unfavourable and they will at best make only poor growth." Tolerance is considered under several headings: environment of the roots; variations in the soil environment and reactions of roots; environment of the shoot (light, temperature, water); biotic factors. Aggression on the other hand is introduced by: "A plant community emerges as a result of successive competition by the species which can tolerate and exploit the microhabitats it offers." Aggression is considered under: shade cast (vegetative reproduction and spread); sexual reproduction and dispersal (pollination and variation, fruits and seeds). Sufficient is covered by these topics to give the student a really good idea of what ecology is, the kind of studies it involves, and what its aims and objectives are.

The chapter on pollination and variation is especially good. Here we find discussed not only the influence of pollination on plant distributions but ecological influences on speciation, such as development of ecotypes and pollination barriers. The opportunity is also taken here to fit in a lesson in statistics at a very logical point and show how useful it can be in ecological studies.

One comment on source material—a very small per cent of the references are from North American ecological literature. This reminds one of criticism levelled at some of the recent ecology texts originating in the United States, which contain only a small number of references to European literature. Furthermore, a large proportion of the American sources are textbooks such as Oosting (Plant Communities), Snedecor (Statistical Methods), Weaver and Clements (Plant Ecology), Woodbury (Principles of General Ecology). Daubenmire (Plants and Environment), which emphasizes autecology most among American texts, is missing. Also there are no references to publications by Maurice Ashby.

The text is "written with sixth form scholarship and training college students in mind." In a few places, but only in a few, it is clear that he is writing for young people at precollege level. For the most part this book makes one wonder if the precollege level in Great Britain is not more advanced than in the United States.—*Murray F. Buell*

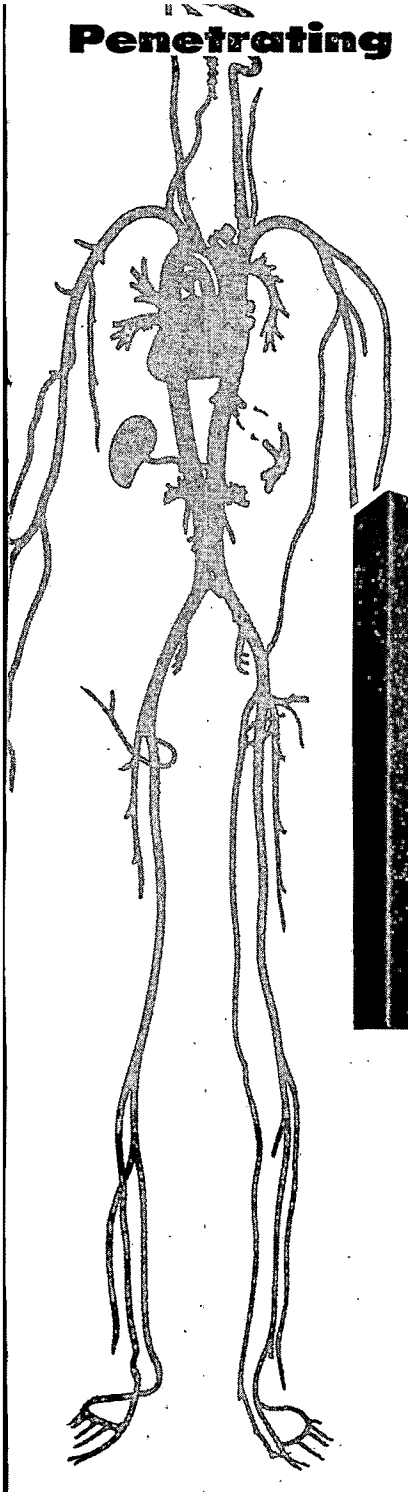
Fundamentals of Scientific Mathematics by GEORGE E. OWEN; 274 pages; $5.00; The Johns Hopkins Press, 1961.

The mathematics required in modern science and engineering is a major factor behind the curriculum developments that have made basic mathematics courses so different from those of a few years ago. To instill students with an appreciation of rigor as well as a familiarity with many subjects, imaginative teachers are trying interesting new approaches, of which this text is a good example. It was written by a professor of physics working with a professor of mathematics under a grant to investigate the teaching of applied mathematics to secondary-school teachers.

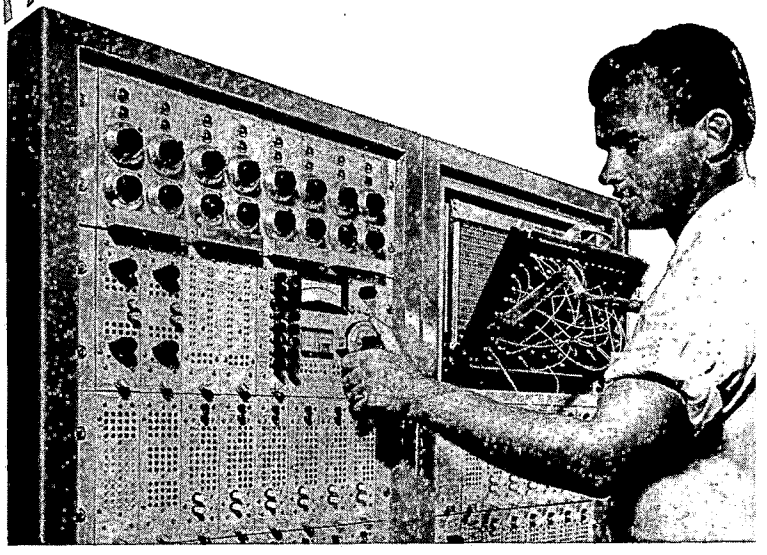
For high school seniors or college freshmen with a background in algebra and trigonometry, the book should provide a good introduction to matrices, vector algebra, analytical geometry, and calculus. Although computers are not mentioned, they have obviously influenced the presentation, particularly in the unusual emphasis on matrices for a book of this level. Vectors are appropriately considered as matrices, and the transformation concept is very well explained; the idea is carried as far as indicating the existence of tensors, and eigenvalue problems are discussed. The calculus is interestingly developed from the matrix point of view, with many illustrative curves.

The book was entirely illustrated by the author, with artistic sketches that are amply clear though not the precise

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diagrams of most texts. Some of the drawings may be distracting (such as a flowerpot being dropped from a window towards a man below), but they give a personal uniqueness, as do the author's informal comments (like "Why all of this worry?").

The book is also quite unique in lacking any formal problems. Although this would inhibit its value for many instructors, the work's solid modern mathematical content and its personality recommend consideration as a text or as stimulating supplementary reading.—*Peter L. Balise*

Descriptive Palaeoclimatology, edited by A. E. M. NAIRN; 380 pages; $11.00; Interscience Publishers, 1961.

This is a trim book with an adroit title succeeding admirably in describing the essence and present state of paleoclimatology including the perplexing ambiguities inherent in the climatic interpretation of most records. It also exemplifies, when synthesis in time and space are attempted, the wide gap between the groping, tentative suggestions of some and the triumphant faith of others in one hypothesis.

The book comprises 14 chapters, each by a different author. A bold and sketchy introductory chapter by Nairn is followed by papers whose topics may be grouped under such general headings and subheadings as I *The Meteorological framework;* Fundamentals of climate; II *Paleoclimatic criteria;* (A) Geologic, such as Desert sandstones, Evaporites, Red beds and Evidence of cold climate; (B) Geophysical, such as Paleotemperatures and Paleomagnetism; (C) Paleontological, such as Vertebrates, Invertebrates and Paleobotanical evidence; III *Regional climatic histories,* such as that of Europe and North America, the Far East, Gondwanaland during Paleozoic and Mesozoic eras and Gondwanaland in the Kainozoic times.

It is to be hoped that the next edition will fill two conspicuous gaps. In what the reviewer calls Part I, a second chapter is needed, one on the physical conditions that produce the local climates, i.e., the environments from which the records of past climates arose. The "meteorological frame-work" is statistical and abstract. The geological and paleontological evidence is concrete and specific. The needed chapter should show that today, in 33°N latitude, vast areas with severe winters and cool summers lie within a few hundreds of miles of others with mild winters and hot summers; that today in latitude 7°S, the desert of the Pacific Coast lies within a hundred miles of the tropical rain forest; that the present site of the Cambrian salt beds of Siberia—supposed to indicate "warm or even hot climate" (page 293) lie 10 degrees farther *north* than the only large modern saline (the Kara Bugaz) which has a colder winter climate than the Gulf of Finland 20° farther north! Before one speaks of interpreting past climates, one must comprehend modern climates. The omission of this crucial chapter reflects no doubt the palaeomagneticist's overvaluation of latitude as a climatic factor, which is also evident from the wording of the introductory chapter.

In Part II, the discussion of "geological evidence of cold climate" does not even specify in its title the pressing problem of recognizing "glacial" conditions in the geological record and does not face realistically the hard stratigraphic facts which must be presented impartially and analysed critically before a claim of glacial origin can be considered valid. This applies especially to many of the far-flung "glacial" deposits in the southern hemisphere which lie intercalated in marine formations, often with rich faunas. In the last quarter century, the recognition of the existence, magnitude and ubiquity of turbidity currents has thrown severe and justified doubt on most such occurrences. There exists a rich literature on the results of the modern exploration of the sea floor and the systematic study of pertinent fossil occurrences. Yet the author index does not contain the names of those who have led in this development. The index does not contain the word "turbidity current" nor does the concept appear in the text or the table of criteria of "till and till-like sediments." Nor is there

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any indication of the presence and local importance of the products of mud-volcanoes which look so startlingly like tillites (including straited pebbles, even of quartzite, according to Kugler). Since "glacial deposits" in unlikely places have furnished the chief leverage with which to force continents to move in men's minds, this omission is serious. In view of the well-established tendency of turbidity currents to travel long distances parallel to a submarine trench or trough, the presence of minerals and rock fragments foreign to a given locality does not prove that land must have been where there is now the ocean floor. A critical separate chapter on the pre-Cenozoic "glacial" sediments is needed, for the same reason that in Part III the pre-Cenozoic history of climates is treated by separate authors in the cases of the Far East and the southern continents ("Gondwanaland").

The value of the chapters in Part III is directly proportionate to the extent to which the nature of the criteria on which the interpretations are based is indicated or at least made accessible by adequate references. The areas covered in these painfully concise chapters are so vast and the complexity of the lines of evidence so great, that the best that was humanly possible to achieve is to suggest the nature of the problems in such a way as to illuminate the vital significance of Parts I and II. It is a pleasure to acknowledge that so much has been achieved under such duress!

This book is indispensable for everyone concerned with problems of paleoclimatology. This includes those who teach geology at the graduate level. But the conscientious teacher must respect the obvious fact that this book is written by specialists for specialists. A routine assignment of any one chapter to students not sufficiently advanced in the particular subject covered by it can be straight intellectual poison. To expect a student to report on a chapter full of uncomprehended words and thoughts is nothing less then instilling the habit of intellectual hypocrisy in young people who should be taught above all else not to mouth empty words or concepts.—*Walter Bucher*

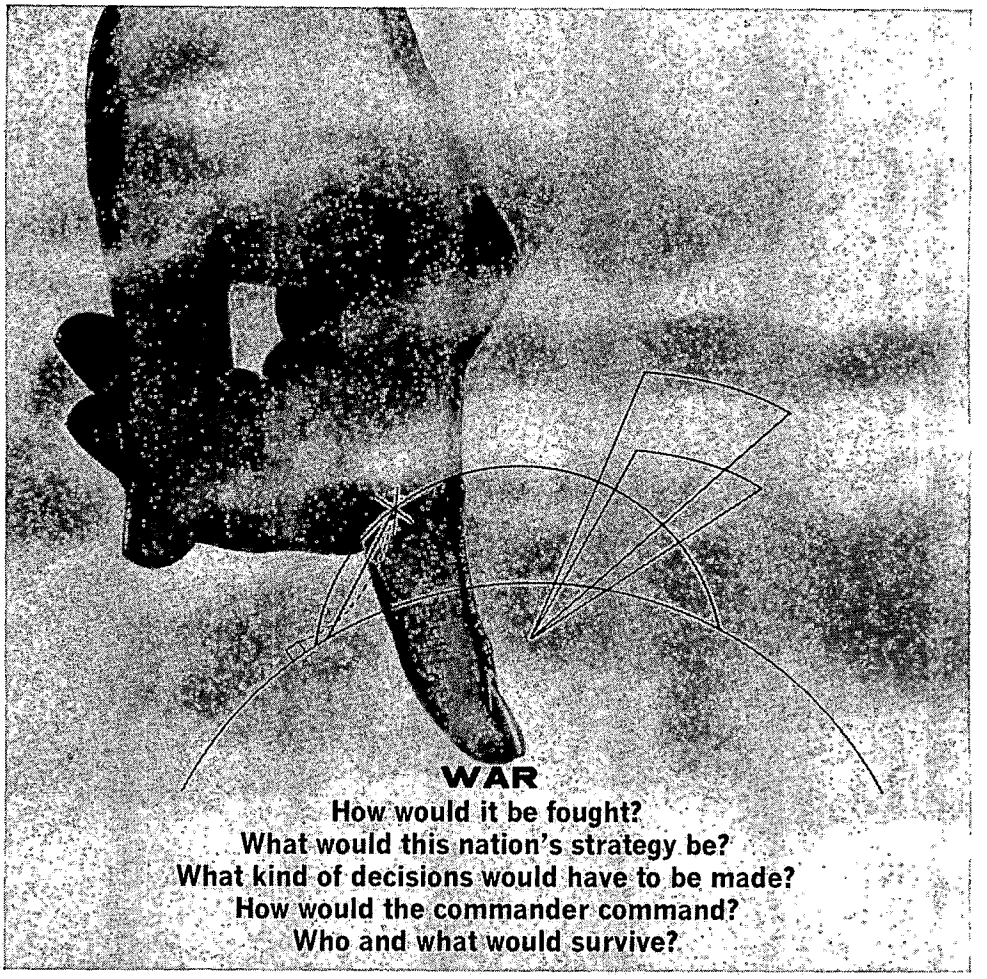
Astronomical Photography (from the Daguerreotype to the Electron Camera) by GERARD DE VAUCOULEURS; (translated by R. WRIGHT); 94 pages; 21 plates; $6.00; The Macmillan Co., 1961.

This little book aims "to trace the development of photography before it finally established itself as the astronomer's indispensable tool, and to serve as a reminder of the early achievements of the pioneers in celestial photography." There is also a summary of more recent and presently developing techniques.

The major emphasis of the book is on the pictorial aspects of direct photography. There are over twenty full page illustrations including many photographs that were spectacular in their time and several that are still so today. In a book of this size there were bound to be omissions. The prospective reader is warned that he will find little about photometry, less about spectroscopy, and nothing about blink microscopes, Photographic Zenith Tubes, or parallax measurements. More surprising, there is no mention of photographs taken with the spectroheliograph, birefringent filter or coronagraph. Within the stringent limitation of its size the book achieves its aims.—*N. J. Woolf*

Vertebrate Speciation, edited by W. FRANK BLAIR; 642 pages; $8.50; University of Texas Press, 1961.

This large volume contains papers presented before a symposium at the University of Texas in 1958. Under "Isolating mechanisms," Carl L. Hubbs discusses speciation in fishes and cites a number of cases of hybridization, some of which are not safely distinguished from character displacement or other phenomena. Hubbs also argues for the probability of sympatric speciation, mentioning Fryer's work on the Lake Nyasa cichlids and the cyprinids of Lake Lanao studied by Herre, Myers and others, but he fails to consider the small-scale geographical isolation inherent in these situations, in which, as Fryer points out for Lake Nyasa,



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most fish species are strongly tied to habitats that are discontinuously distributed within the lake. J. S. Mecham gives a very good review of isolating mechanisms in frogs and toads, and suggests that the only mechanisms reinforced by natural selection are the pre-mating ones. John Moore briefly reviews his interesting work on the interaction between nucleus and cytoplasm in interspecific nuclear transplants in frogs.

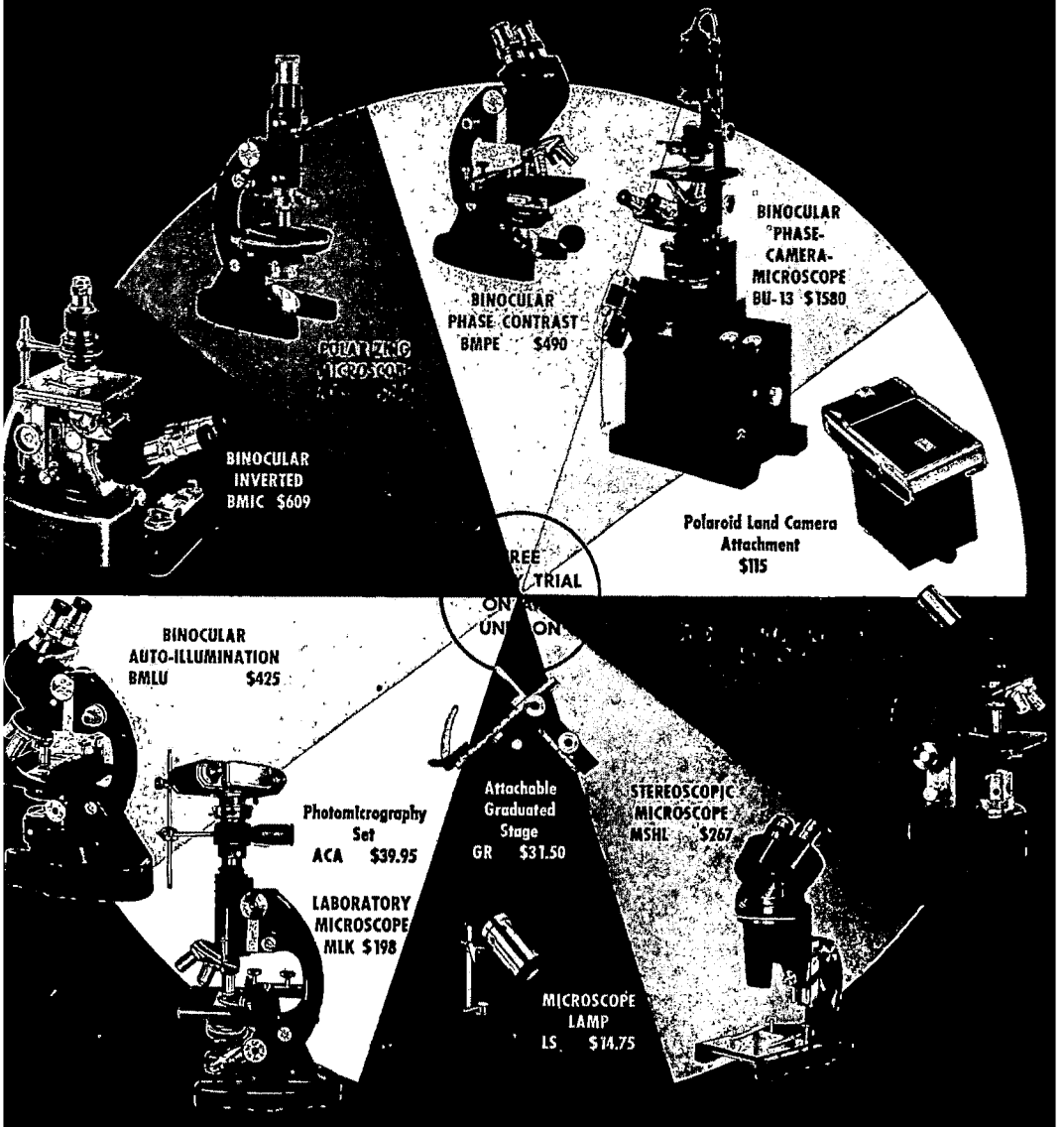
C. G. Sibley's review of natural hybridization, chiefly in birds, must be read with caution because of the author's unusually broad application of the term "hybrid" and because at least some of the cases cited are open to other interpretations not mentioned by the author. In the next two sections, Peter Marler (evolution of visual communication), J. A. King (ontogeny and behavior in *Peromyscus*), L. T. Evans (structure and behavior of reptile populations), K. L. Dixon (habitats and distribution of nearctic chickadees and titmice, genus *Parus*) and E. P. Volpe (polymorphism in anuran populations) present clear and informative balanced reviews of their respective fields. W. F. Pyburn, H. S. Fitch and V. C. Twitty report on their own specialized herpetological investigations. W. W. Milstead's study of competitive relations in lizards brings together valuable information, but gets into trouble with the all-too-common confusion of competition with the interferent behavior by means of which animals tend to avoid it. C. P. Haskins and his co-workers present an elaborate progress report on their complex long-range studies of polymorphism and population structure in the common guppy.

Under "Polytypic species," R. F. Inger deals with problems in the application of the subspecies concept—a concept now under heavy fire. He claims, contrary to other recent authors, that the known cases of discordant variation of racial characters "do not constitute the common pattern. Instead we usually find papers describing correlated geographic variation of two or three characters." Inger's argument is not helped by his failure to cite references documenting this contention, especially since the evidence against subspecies is piling up in the recent literature. The next contribution, by J. K. Doutt, is part of this evidence, though the author may not have intended it to be. Doutt extracts from very intensive studies of the Pennsylvania populations of the short-tailed shrew and the meadow mouse the fact that local populations within this one state often differ more sharply among themselves than do wide-ranging, formally named "subspecies" of these same species.

In the final section, M. J. Littlejohn summarizes recent work on the frogs of the close-knit *Crinia signifera* group against the conjectural background of Pleistocene and recent climatic changes in southern Australia. Of the three accounts of speciation and historical geography of particular fish faunas, those by G. Svärdson (northwestern Europe) and C. Kosswig (Asia Minor) are somewhat rambling and full of complicated geographical details unfamiliar to most American readers; much of their point is dulled for want of strict enough editing. Miller's paper on Speciation Rates in Western North American Fishes provides much information and some maps of distributions and past and present watershed systems, but actual judgments on rates of evolution are disappointingly sparse and apparently offered with some hesitation. E. S. Deevey concludes with a chapter on recent advances in our knowledge of the Pleistocene that is clear, compact and full of interesting information, but which barely touches on vertebrate speciation. At the end of his article, Deevey casts doubt upon Zeuner's minimal time per species-step of a half-million years. His caution in suggesting that species can arise and become extinct in less time seems unnecessarily exaggerated in view of the information accumulating on speciation rates from various sources. To this reviewer it seems likely that we shall soon be talking of minimal speciation rates of the order of 100, or even 10, years or generations.

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Speciation" have become increasingly common in the past twenty years. The most valuable papers in such works are the reviews of broad subjects, which help to make up for the lack of an "Annual Review" in the evolutionary field. The present volume contains a tremendous amount of compressed information of the review type, and it is a bargain for the price charged.—*William L. Brown, Jr.*

The Cloud Forest (A Chronicle of the South American Wilderness) by PETER MATTHIESSEN; 280 pages; $6.50; Viking Press, 1961.

This chronicle of the South American wilderness is recommended general reading for any scientist planning a sojourn in the land of missions, mountains, monkeys, and mosquitoes. It is not, however, an ecological treatise, nor even a description of vegetation and wildlife in The Cloud Forest *per se* but, rather, well-worded reports of the people, their habits, and their habitats. Here and there, forests, birds, rodents, hills, rocks, and mosses are vividly described by the well-qualified writer, who also wrote *Wildlife in America.*

The story begins in the Gulf Stream, moves swiftly to Trinidad, British Guiana, and to the mouth of the Amazon. Up the Amazonas, the adventures were not made in typical 20th century luxury, but neither were they endured as pioneering hardships. Planes and river boats expedite Matthiessen's journey and provide opportunities for discourse with the people. From these talks—frequently with Caucasian missionaries as well as the natives of every degree of color pigmentation—unwritten history unfolds and is here recorded. The historical account is not separate from the geography of the continent, as in the academy, but rather the two disciplines are finely interwoven into a smooth fabric, yet with sufficient warp and woof exposed to reveal the dependance of each upon the other.

From the headwaters of the Amazonas, over the Peruvian Andes, descending to the western foothills of the great Atacama desert, and then to the Pacific Coastal town of Lima the journey continues. Thence the author travels through Chile to "Land's End," and again northward.

A thousand miles beyond the southerly limits of ordinary travel in South America is Tierra del Fuego, an island of the greatest of the world's archipelagos. Others, too, have written of their experiences here where the weak sun casts purple shadows. Captain Slocum described it in "Sailing Alone Around the World," Richard Dana in "Two Years Before the Mast," and Darwin in his "Voyage of the Beagle." Had Matthiessen been the contemporary of any of these, *The Cloud Forest* would take its place among the finest in the "Five Foot Shelf of Books." This man has a way with adjectives: he paints pictures without pigment.—*Laurence C. Walker*

The Chemistry of Heterocyclic Compounds, Vol. XIV, in 4 parts; A. WEISSBERGER, Consulting Editor: Part I, *Pyridine and Its Derivatives,* edited by E. KLINGSBERG; 613 pages; $49.00; $42.00 by subscription; Interscience Publishers; 1960.

Fortunately, editors can still convince competent chemists to review fields almost hopelessly diverse and complex. The more formidable the task of organization, often the more desperately such reviews are needed. This generalization is illustrated well by the subject of the book under discussion, the compound pyridine. A measure of the magnitude of current research on derivatives of pyridine, substances known over a century, is provided by recent subject indexes to *Chemical Abstracts;* more than twenty pages are listed each year. Pyridine, the most common heterocycle, requires a coverage of sixteen chapters in four volumes as part of the present series of monographs.

Part I of *Pyridine and Its Derivatives* consists of only two chapters. The first, "Properties and Reactions of Pyridine and Its Hydrogenated Derivatives," by R. A. Barnes of Rutgers University is an introductory essay.

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A general survey of theoretical and practical aspects of pyridine chemistry is discussed concisely, but somewhat biasedly since not all viewpoints are always presented. Students will find this introduction particularly worthwhile; research workers will, presumably, find greater detail in later sections of the series.

The very long second chapter (490 pp.) by F. Brody and P. R. Ruby (American Cyanamid Company) entitled "Synthetic and Natural Sources of the Pyridine Ring" documents methods of obtaining pyridine compounds. Over 1800 references are cited! I found the presentation and organization of this encyclopaedic chapter to be commendable.

By searching, minor errors can be found. These do not detract from a job generally well done. Two major faults, although regrettably common, irritate highly. The cost of the book is excessive; eight cents for a page of modest size is surely too much for even a library to have to pay! Why must almost three years elapse between the last cited literature references and the date of publication? Other publishers have recently reduced this lag to as little as six months for an advanced text.—*Paul Schleyer*

Treatise on Analytical Chemistry, edited by I. M. Kolthoff and P. J. Elving: Part I, Theory and Practice; Vol. 2; pp. 811-1308; $16.00; Part II, Analytical Chemistry of the Elements; Vol. 1; 471 pages; $16.00; Vol. 3; 380 pages; $13.25: Vol. 5; 409 pages; $15.50; Interscience Publishers, 1961.

Submicrogram Experimentation, edited by Nicholas D. Cheronis; 351 pages; $12.75 cloth, $10.75 paper; Interscience Publishers, 1961.

Over the past twenty years every field of science has advanced in much the manner of the fabled knight, he who "jumped upon his horse and rode off in all directions." The variety of instruments and techniques available to the experimentalist in any field has increased exponentially. This added complexity has necessarily resulted in an increase in specialization, to the point where, in fields such as analytical chemistry, it is no longer easy to find a general practitioner.

One result of this specialization has been evident in the authorship of technical books. Almost anyone working in a field thinks he could write a good elementary or intermediate text; this can be seen from the yearly booklists. Almost no one has the temerity to tackle a whole field single-handed at the graduate level. This is work for a committee, at the very least.

The quality of such a committee-produced book is dependent to a tremendous extent on the skill of the editors, both in choosing their authors and in reducing the many-styled result to a smooth, continuous whole. On the evidence of the four volumes reviewed here, Kolthoff and Elving have done a remarkable job of editing.

This series, which may eventually consist of a very large number of volumes, is divided into the three parts: theory and practice, a discussion of general techniques; analytical chemistry of the elements, in which each element is discussed separately; and the analysis of industrial products. The section of theory and practice discussed in Volume 2 of part I concerns reactive groups as reagents and the techniques of separations. The elements covered in the other three volumes are: Volume 1, hydrogen, water, the inert gases, and the alkali metals; Volume 3, cadmium, copper, magnesium, mercury, tin, and zinc; Volume 5, nitrogen, phosphorus, thorium, titanium, zirconium, and hafnium.

A glance at the very complete table of contents of any of these volumes will cause any analyst to wonder how so much ground is covered in so small a space. The answer is, of course, that it isn't. This treatise is aimed not at the man who needs a specific, detailed account of a technique for a particular element, but rather at the experienced analyst entering an unfamiliar corner of his field. It is tantalizing, rather than informative, to the man without access to a very good chemical library, with all of the latest journals. Some few

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techniques are described in detail, but even in these descriptions there is no discussion of probable difficulties or interferences. This series tells the analyst what can be done, but not how to do it.

The symposium volume "Submicrogram Experimentation" has all of the qualities, good and bad, typical of the breed. The book is concerned almost entirely with the analytical chemistry of submicrogram quantities, rather than with general experimentation. The techniques discussed include gas, column, and paper chromatography, x-ray fluorescence, microbiological assay, electron microscopy, infrared and ultraviolet spectrophotometry, and olfactory sensing methods. The best papers are very good indeed, and among these I would include Eyring's concluding statement, Mitchell's remarks on gas chromatography, Natelson on x-ray spectroscopy, and Cunningham on methods used in the study of the synthetic elements.

Like most symposium volumes, this book is not likely to become a working text in anyone's personal library. It is a good statement of the current methods and limitations in this area of analysis, but in a field so active it will certainly soon be largely of historical value.—*Peter J. Wangersky*

Advances in Inorganic Chemistry and Radiochemistry, Vol. III, edited by H. J. EMELEUS and A. G. SHARPE; 463 pages; $12.50. Academic Press, Inc., 1961.

This is the third volume in a series of collections of review articles dealing with various topics in inorganic and nuclear chemistry. As in previous volumes, the emphasis is on the inorganic rather than the radiochemical side.

Although this volume covers diverse subjects, the reviews are for the most part timely and well written. They include extensive bibliographies which in all but one case contain references dated 1960. The volume contains an author index to all included references as well as a fairly extensive subject index.—*W. D. Horrocks, Jr.*

Surface Activity and Detergency, edited by K. DURHAM; 250 pages; $8.00; St. Martin's Press, 1961.

This book, based on a series of lectures given by the five contributors at Liverpool College of Technology in 1957–58, is aimed at presenting a brief account of the basic physical chemistry of detergent systems, and examining detergent action from a fundamental point of view. Although the nine chapters vary considerably in quality and style, as is frequently the case with symposia, the over-all result is a book that everyone interested in this field will want to have continuously handy. The opening chapter, on properties of detergent solutions, and the sections dealing directly with detergent action, dirt removal and redeposition are well organized and presented. The authors have achieved a good synthesis and have made a good selection from their own experiences as well as from the overwhelming amount of published material available. The application of electrokinetic theory to the removal and redeposition of particulate soils is especially well presented. The discussions of wetting and adsorption kinetics, on the other hand, are disappointing. Although they cover the older work adequately, they fail even to mention some of the more notable recent contributions in these areas—for example, those of Zisman and Guastalla and their respective co-workers. In sharp contrast is the chapter by A. S. C. Lawrence summarizing his own work on the phase behavior of soap-oil-water systems and its relationship to detergency. This is particularly interesting and stimulating and is certainly one of the highlights of the book. Of similar novelty and interest is the discussion of ancillary effects in detergency by D. G. Stevenson, which emphasizes the role of complex formation and osmosis, and is excellently illustrated with photomicrographs. A brief but very helpful chapter on detergency evaluation, written for the experienced scientist rather than the technician, rounds out this valuable contribution to the literature of surface and colloid

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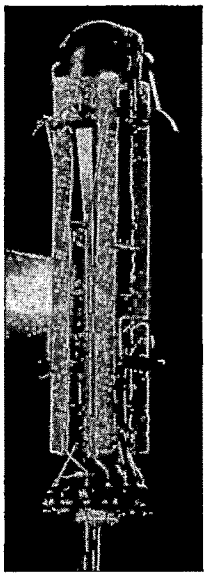
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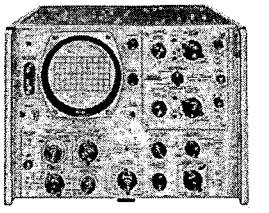
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chemical technology.—*Anthony M. Schwartz*

India (A Modern History) by P. SPEAR; 510 pages; $10.00; The University of Michigan Press, 1961.

This is not an easy book to review. It has certain virtues which deserve genuine commendation. At the same time, it has some limitations which produce a mixed reaction, at least for the specialist in the field. Perhaps mention should be first made of its patent virtues.

Mr. Spear has produced a coherent, well-organized and lucid volume. It covers a great deal of Indian history without falling into tiresome detail, and it avoids that emphasis on dynasties battles and rulers which has been so prominent a feature of the writing of Indian history. It is not confined to political history, which has probably been overdone, and does present social, economic and intellectual history. In all of these respects the book is to be commended.

Nonetheless, it has limitations—some of them rather subtle. To begin with, I think it largely fails to grasp the spirit and the sources of the Indian viewpoint during the modern period of foreign domination. It is clearly written in the context of an urbane, western view. Partly as a result of this, it tends to present the story of British rule with the usual emphasis on the British achievement and without due regard for the Indian response. Also, I think, as a result of this, it falls into the vice of casting up—at least by implication—a kind of moral balance sheet for the advantages and disadvantages of British rule. I rather doubt we need any more of that kind of interpretation.

Perhaps most important, because of its implicit western orientation, it tends to be superficial in its discussion of the vital changes in Indian society which should be so prominent a part of any challenging historical interpretation of India's modern period. The history of modern India is, *par excellence*, a history of the dynamics of change—much of which was inspired by foreign domination—and Mr. Spear's book seems to pay inadequate attention to the causes, the characteristics and the central tendencies of that change.—*Robert I. Crane.*

Races and Cultures of India by D. N. MAJUMDAR; 483 pages; $9.50; Distributed exclusively in the U.S.A. by Taplinger Publishing Co., for Asia Publishing House, 1961.

D. N. Majumdar, late professor of anthropology at Lucknow University, was one of India's more eminent anthropologists. His career coincided with the development of all branches of anthropology in India and, through his vigorous proselytizing, anthropology became a part of the curriculum of many of India's universities. He has done important work in physical anthropology, particularly in seriological surveys in Uttar Pradesh and Gujarat, on tribal ethnography in the Himalayas and in Southeastern Uttar Pradesh. In recents years he had directed community studies and urban surveys which have been useful in economic and social planning. Unfortunately, the book being reviewed does not reflect as it should Professor Majumdar's extensive research experience and undoubted capacities.

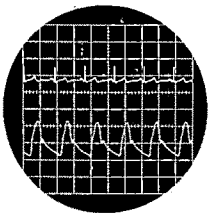
"Races and Cultures of India," is a series of loosely connected essays, the origin of which appear to be in some cases classroom lectures, given to students who learn from memorizing names and dates and brief characterizations, rather than from study of the original writings of authors.

The work is divided into three unequal parts. In the first, Prehistorical Archaeology and Physical Anthropology, pp. 3–120, Majumdar discusses prehistory. This consists largely of summary descriptions of lithic tool traditions. Even in the discussion of the neolithic in India he doesn't deviate from outdated classifications to explore the origins of agriculture in India. The Indus Valley civilization is not discussed with general developments of the neolithic. His discussion of Mohenjodaro (pp. 27–28) is replete with unfounded generalizations on its "Dravidian" origin, non-

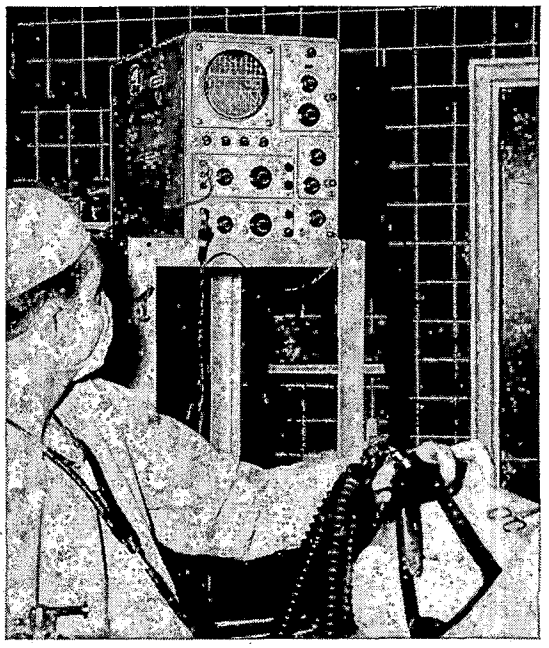
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sense about matriarchal institutions and the role of the Indo Aryans. The bulk of the first section is devoted to an attempt to make sense out of the racial history of India. Majumdar vigorously attacks many European scholars for introducing concepts of racial superiority which are an outgrowth of "exploitative capitalism" into their study of Indian history and sociology. Majumdar thinks that racial traits are important to the study of culture in India, particularly the caste system. Unfortunately, the reader is not given the data to support Majumdar's arguments on this important question.

The second two-thirds of the book is devoted to discussions of various tribal groups and to theoretical questions regarding the structure and function of social institutions in India. Like some Indian anthropologists he over-romanticizes the life of the tribal peoples in India.

> "Primitive man is subject to the powers of nature . . . . His tools are those which he can shape well from the materials at hand . . . . He uses tools because he wants to satisfy his needs . . . . Income is priced not for aggrandizement, not for the sake of profit, but for effecting an equitable adjustment between resources and social group needs . . . . Leaders are not those who possess property or wealth . . . . They are expected to lead their people out of harm's way, to warn them of impending trouble or calamity" (p. 121).

This is a viewpoint totally unsupported by the last 100 years of ethnographic and social anthropological research.

There is some interest in Professor Majumdar's reports on polyandry among the sub-Himalayan peoples but this summarizes the discussion in his other published work.

In terms of the data presented or the theoretical discussion this book adds little to previously published work of Dr. Majumdar's and, unfortunately, for the non-specialist in anthropology or Indian studies, it is too filled with mistakes, debatable interpretations and outdated theory to be of much use as an introduction to the races and culture of India.—*Bernard S. Cohn*

New Approaches to the Study of Catalysis by PAUL H. EMMETT; 157 pp; $3.25; Phi Lambda Upsilon, The Pennsylvania State University, 1961.

To the zealous researcher who asked: "What should I read to get acquainted with catalysis?" the answer was: The seven volumes of Emmett's Treatise. Now, fortunately, there exists an alternative: Emmett's Thirty-Sixth Annual Priestley Lecture. There are many interesting features about this lecture and the best ones do not really correspond to its title. First of all, what is *new*? For many graduate students, anything more than two years old is stone-age. Most of the approaches surveyed in this Lecture, such as the BET method of measuring surface areas of solids, field emission microscopy, the electronic viewpoint in catalysis, the effects of diffusion on catalyst activity and selectivity, the use of isotopes in the study of catalytic reaction mechanisms are all about twenty-five years old. What makes these approaches valuable is not their novelty but the fact that they have been tested time and again in fundamental investigations. What makes Emmett's Lecture so fascinating is not the novelty of these approaches but their timelessness insofar as a correct fundamental approach has no age. What makes this short monograph so useful is not its survey of new approaches but rather its critical and skeptical appraisal of a difficult field by a man who for the last twenty-five years has contributed so much to its development.

With the enormous facility in laboratory equipment from gas chromatographs to digital computers now available to the catalytic chemist in university and industry, the temptation to solve the problem of catalysis in one bold stroke becomes sometimes irresistible. A careful reading of this masterly lesson by Emmett will cool down over-enthusiastic beginners. But, at the same time, the wisdom of these pages will stimulate fresh approaches to old problems that are as challenging today as they were twenty-five years ago.

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By M. M. EL-WAKIL, University of Wisconsin. Available in November.

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• **MICROWAVE CIRCUIT THEORY AND ANALYSIS**

By RABINDRA N. GHOSE, Director, Advanced Development Laboratories, Space-General Corporation. Available in January, 1963.

Book presents a comprehensive and up-to-date treatment on microwave circuit elements and circuits employing such elements. Subject of microwave circuit theory and analyses is treated exclusively in this text instead of considering the subject as a part of electromagnetic theory as is done in most texts on microwave theories.

• **AIR, SPACE, AND INSTRUMENTS: Draper Anniversary Volume**

Edited by SIDNEY LEES. Available in January, 1963.

An anniversary volume of original contributions in honor of the sixtieth birthday of Charles Stark Draper, Director and Founder of the Instrumentation Laboratory of the Massachusetts Institute of Technology. The contributions in this volume were written by Draper's distinguished former students, colleagues, and friends in each of the areas where he made important advances. Many of the authors are world-authorities in their respective fields, and much of the material is unobtainable elsewhere.

• **THE THEORY OF PLASMA WAVES**

By THOMAS H. STIX, Princeton University. Advanced Physics Monograph Series. Available in October.

Treatise is concerned with the small-amplitude oscillations of a collisionfree plasma immersed in a strong static magnetic field. After a survey of waves in a cold plasma, attention is turned to the general description of energy flow through anisotropic inhomogeneous media, and then to the detailed normal-mode solutions of some illustrative boundary-value problems. The second and major portion of the work deals with the finite-temperature plasma, starting from the Boltzmann-Vlasov equations.

• **INTRODUCTION TO THE UTILIZATION OF SOLAR ENERGY**

By A. M. ZAREM and DUANE D. ERWAY, both of Electro-Optical Systems, Inc. Available in October.

Provides a thorough treatment of the fundamental aspects of solar utilization, and timely information on the nature and problem areas which arise in attempts to utilize solar energy by a very wide variety of means—from the basic one of obtaining heat to the more sophisticated applications in space power systems. Analytical work is presented to determine the performance capabilities of various devices.

• **MICROWAVE FERRITES AND FERRIMAGNETICS**

By BENJAMIN LAX and KENNETH J. BUTTON, both at Massachusetts Institute of Technology, Lincoln Laboratory. Lincoln Laboratory Publications: *Just published.*

This book discusses the solid state of physics of magnetic ferrites, garnets, and related ferrimagnetic materials and leads into a description of the theory and experimental behavior of microwave devices. Incorporated are the latest experimental and theoretical results necessary for a comprehension of the phenomena and applications. The mathematical development is exceptionally lucid and mathematical results are correlated with experimental data wherever possible.

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nitrogen on solid catalysts. What is the rate-determining step of the reaction between nitrogen and hydrogen to form ammonia? Why are able investigators in Japan and the Netherlands coming to diametrically opposite conclusions, using the same techniques and catalysts that look very similar? What is the nature of the active iron surface? What is the species at the surface that is critical to the reaction? Emmett discusses all these unsolved questions with clarity and completeness. Yet his unbiased discussion does not hide his deep personal commitment to a puzzling scientific problem which he helped to formulate in the correct way.

The Lecture contains a complete and up-to-date list of references. It is a very necessary guide to anyone who wants to be informed about the status of catalysis at solid surfaces. This little volume is mimeographed, paper bound, too expensive, and probably hard to get. But it is an indispensable compact digest of Emmett's mammoth treatise and should be in the library of any chemist dealing with catalysis and of any scientist who is curious to know about knowledge in a field that is frequently and unjustly associated with commercial rather than scientific values.—*M. Boudart*

Ostracod Taxonomy by HENRY V. HOWE; 366 pages; $10.00; Louisiana State University Press, 1962.

When Howe's *Handbook of Ostracod Taxonomy* appeared in 1955 it became a most important reference book to ostracod taxonomists, and the edition was soon exhausted. Since then, several volumes (Pokorny's *Grundzüge der zoologischen Mikropalaontologie*, 1958; the Russian *Basic Paleontology*, 1960; and Kesling's ostracod volume of the *Treatise on Invertebrate Paleontology*, 1961) have dealt comprehensively with ostracods. The last two of the volumes mentioned provide descriptions and illustrations of almost all fossil and living ostracod genera, but their systems of classification differ radically, and they do not provide extensive bibliographies. Howe's *Ostracod Taxonomy* is an up-to-date revision of his *Handbook*. It offers an alphabetical list of 1936 genera and suprageneric taxa, with complete references to type species and with notes as to classification in the important reference works. It also provides a biblography of 959 titles. It thus helps to bridge the gap between the *Treatise* and the Russian *Basic Paleontology*, provides additional bibliographic information, and will be welcomed by all ostracod taxonomists. —*A. G. Fischer*

Evolution and Genetics (The Modern Theory of Evolution) by D. J. MERRELL; 420 pages; $6.00; Holt, Rinehart & Winston, 1962.

Professor Merrell finds that "doubts about the validity of evolution by intelligent and supposedly educated people" are still not uncommon, and he has written this college-level text "primarily for those who wish to know more about the theory of evolution and the operation of evolutionary forces." The 36 chapters and two appendices cover quite a variety of topics, from the Origin of the Universe, the evidences of evolution, brief surveys of the principal animal and plant groups, a concise yet quite inclusive outline of genetics (Chapters 15–32, including the transmission, population, and chemical genetics), origins of man, the population problem, dangers of radiation damage, and eugenics. The presentation is throughout simple but not oversimplified, clear but not side-stepping unsolved and controversial problems, often flavored with a kind of astringent humor, and attesting an experienced teacher who has dealt with many a class and knows how to make a difficult point seem tolerably easy.

The subject of evolution can be presented in countless different ways, and fortunately, no particular one has yet become standard or even customary. The book begins with a discussion of organic adaptation (or, rather, adaptedness), followed by a survey of the history of evolutionary thought, the fossil record, origins of the universe and of life, and only then passes to the evidences of evolution, the mechanisms

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Human Nature and the Study of Society

THE PAPERS OF ROBERT REDFIELD—VOL. I

Edited by Margaret Park Redfield. The three sections of this collection of papers by the distinguished anthropologist deal with (1) general theory; (2) his work in Mexico and (3) the question of whether a common "nature" is shared by all humans. Includes writings formerly available only in widely scattered sources, and unpublished lectures which illustrate especially well Redfield's interest in the general scope of social science. 560 pages. $10.00.

The Cholera Years

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By Charles E. Rosenberg. When cholera first reached the U. S. in 1832, it was widely regarded as a deserved scourge of sin. By 1866 the creation of New York's Metropolitan Board of Health indicated a rational approach to the problems of an industrial society. This study focuses on New York City to trace the advances in both medical knowledge and social thought that ultimately defeated the "plague of the 19th century." 336 pages. $5.95.

The Structure of Scientific Revolutions

By Thomas S. Kuhn. In this brilliant analysis of the nature and function of basic scientific revolutions, Professor Kuhn traces the past three centuries of physical science to find a pattern. He argues that practice within the accepted framework of "normal science" evokes crises which cannot be resolved until a new conceptual structure is established which can again govern the search for novel facts. 200 pages. $4.00

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of evolution, and concludes with chapters dealing with man. Let us hope that it will dispel many doubts about the validity of evolution by making some intelligent people better educated.
—*Theodosius Dobzhansky*

Notes on Molecular Orbital Calculations by JOHN D. ROBERTS; 156 pages; $4.95; W. A. Benjamin, Inc., 1961.
Molecular Orbital Theory for Organic Chemists by ANDREW STREITWIESER, JR., 489 pages; $14.50; John Wiley and Sons, 1961.

These are much needed, fine books, which should have a great impact on chemistry.

Molecular orbital theory in the simple LCAO or "linear combination of atomic orbitals" form has become so useful for understanding properties of organic molecules that, in the words of Roberts: "There is no excuse for a modern organic chemist not to be able to use the LCAO method." Until very recently, however, even the most earnest student of organic chemistry has had a most difficult time learning LCAO theory, for the only route for him was through long books or courses on quantum mechanics, which at the end perhaps just touch this method and its applications. The 1959 book *Quantum Chemistry*, by Daudel, Lefebvre, and Moser, partly filled the gap, but it had a broader scope and a physical chemical emphasis.

Roberts is a short book, ostensibly intended simply to help a reader learn to calculate the pi-electron energy levels, resonance energies, bond orders, free valences, localization energies, and related properties for simple unsaturated hydrocarbons and molecules related to them. I suspect that a chemically inclined reader will get a great deal more from the book than just calculative skills, however, especially if he follows the author's full suggested course of study. Included are sixty graded exercises, the last of which well illustrates the lot: "Use Dewar's method in combination with other more qualitative reasoning to estimate the relative ease of displacement of chlorine by ethoxide ion for the monochloroquinolines and the monochloroisoquinolines." There is a general bibliography with 16 references, and in the text proper more than 80 authors are cited more than 150 times. Two molecular orbital papers by Roberts himself are included as an appendix.

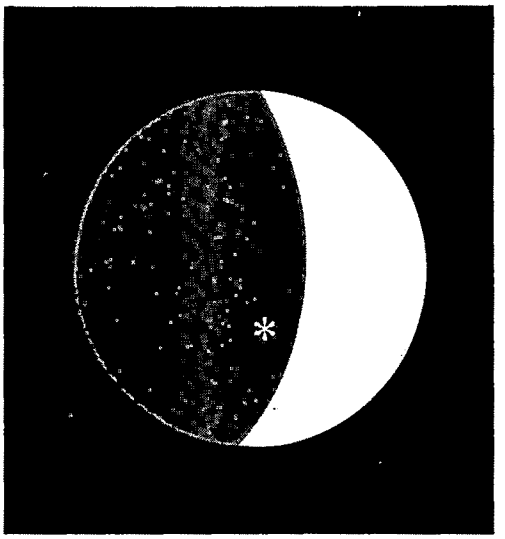
The book by Streitwieser is truly extraordinary, I believe a work of first importance. The simple LCAO method is taught, with illustrative examples and exercises. But the book accomplishes much more than this. Streitwieser makes frequent references to more advanced theoretical techniques, and he tries to assess the merits of competing methods in each new context. Applications to physical properties of molecules and chemical reactivity are extensively treated, and prefacing the discussion of each application is exposition describing the phenomena or measurements involved. The whole book is written critically and painstakingly, and it incorporates much research work of Streitwieser himself. Chapters 5 and 10, on Heteroatoms and on Aromaticity and the 4n-2 Rule, are especially fine. Documentation is outstanding: in the author index there are more than 3200 citations to the research papers of no less than 1200 authors.

Both books include some elementary group theory. Neither treatment would suit a purist's tastes, but that was not the authors' intention.

I find little wrong with these books. Roberts differentiates x^2 and gets $\frac{1}{2}x$; he uses words like "starrable" and "constructable" while criticizing someone else's "esoteric verbiage"; he uses the term "self-consistent field" in a way that today is unacceptable; he includes many figures in which neighboring pi orbitals are pictured as perfect dumbbells with nothing in common; in a three-page final chapter entitled Higher Order Calculations he gives no references. Streitwieser calls something "skullduggery" which some of his students would spell "skulduggery" and consider routine mathematics; he includes a number of theoretical "non sequitors" (e.g.,

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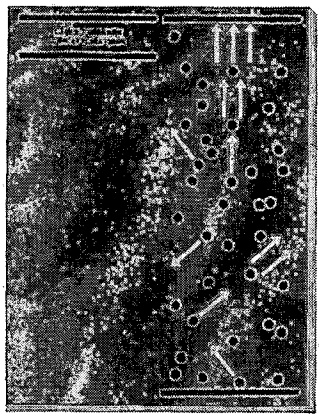
On Wednesday, May 9th, a team of Massachusetts Institute of Technology scientists illuminated the moon's surface and detected and recorded the reflection. The new first was accomplished with 13 bursts of 6934-angstrom light from a 50-joule, 4-barreled Raytheon ruby laser. The new Raytheon laser is the most powerful yet reported. Raytheon is currently investigating laser systems in the areas of communications, ranging, detection, space vehicle guidance, weapon systems, and medicine.* Write for further information to Martin B. Curran, Raytheon Company, Lexington, Massachusetts.

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pages 23, 139, 148, 392); he terms some approximations "dillies" which really may not be so bad (and in any case are the basis for most of the book!); he makes no attempt to discuss the validity of the pi-electron approximation and does not adequately reference the literature on this subject; in a nine-page final chapter, Advanced MO Methods, he gives a too cursory treatment of a great deal of recent theoretical work. But these are but minor detractions.

These books bring molecular orbital theory certainly within the survey and probably within the reach of all organic chemists. To learn molecular orbital methods is not, to borrow a phrase from Roberts, a job to tackle "while swinging in a hammock beside a mountain lake." But what job in science is?—*Robert G. Parr*

American Forestry: Six Decades of Growth, edited by HENRY CLEPPER and ARTHUR B. MEYER; 319 pages; $5.00; Society of American Foresters; Washington, D.C., 1960.

Some ten years ago the Society of American Foresters commemorated its semicentenary by sponsoring the publication of a 385-page history of forestry in this country during the fifty years of its existence. The nineteen chapters of the book were written by as many distinguished foresters. The chapters dealt in adequate fashion with the various disciplines coming under the heading of forestry as well as with the several agencies and organizations involved with the profession. It therefore came as a mild surprise that the Society was able to find sufficient justification for celebrating its sexagesimal in like manner.

The present volume is patterned very closely after the former one, but no attempt is made to build upon the information presented in the first book. While the chapter headings are for the most part the same (protection is now combined with management, recreation and literature have each attained chapter status), the present authors, with one exception, are new. There has been quite a duplication of effort.

American Forestry can boast several outstanding treatises, among them G. A. Garratt's 23-page capsule history of the past six decades and C. R. Clar's succinctly presented account of governmental forestry on the state level. D. P. Duncan and F. H. Kaufert have a good discussion on the education of a forester, even touching upon the prestige and professional status lent a person by a degree in forestry. A. B. Meyer's discussion of the literature in the field is quite informative.

Overall, the book lacks the coordination and integrated treatment which is perhaps best supplied by a single author. A qualified historian could place the development of American forestry in the proper framework of the growth and development of the nation (and of the world) during this time. Single authorship would eliminate needless repetition—the history of our federal forestry legislation, for example, is repeated time and again—and would be better able to place proper emphasis. In the present volume, for example, the major impact of the C.C.C., of the chain saw, and more recently, of electronic computers on the shaping of forestry development in this country has been largely neglected.

The recently popular slogan "multiple use" is glibly mentioned in several places, but several questions about it are never thoroughly explored nor critically analyzed. Is multiple use of our public and private forest lands really just around the corner? Are our foresters trained to cope with multiple-use management? Does the concept really provide a panacea? The enormous future importance of our forests in contributing to the nation's water supply and the perhaps equally important exploitation of forest lands for recreation are certain to be of such magnitude as to overshadow their utilization for wood production over vast areas. This fundamental change in forest land use, with the concomitant change in basic outlook, is appreciated by C. F. Brockman but overlooked or barely hinted at by the remaining contributors.

Forest tree improvement, proclaimed by S. H. Spurr in his chapter to be the

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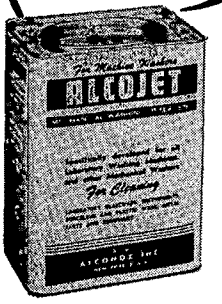


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greatest development in silvicultural research in the twentieth century, receives no attention in the chapter on research and but a page elsewhere. B. Orell stands almost alone in stressing that utilization is the heart of forest production and that tax considerations have an important bearing on forest management. Scant mention is made of the problem of small forest ownerships in the chapter devoted to farm forestry. Are the forest holdings of less than 100 acres (perhaps 85% of the nation's total forest land area) really under management—or even manageable? From the range management chapter, one would gather that there has been no friction between private range interests and the Federal Government. Nowhere is there a discussion of the role the timber barons played in the development of forestry, of the problems of forest insurance, of land planning or zoning, of the declining quality of the nation's saw timber, of the conflict between sound forestry practices and our system of free enterprise.

For a book of this nature to be a true contribution, a dispassionate authorship certainly would be more desirable. Thus, for example, while in many respects no one is better qualified to write on the U.S. Forest Service than its chief R. E. McArdle, a more significant appraisal would perhaps have been forthcoming from a "disinterested" outsider.

The main value of *American Forestry* will perhaps be to provide the forester with a nostalgic review and optimistic prognosis of the development of his profession in the United States. For more scholarly approaches to forest history and policy the reader is referred to the 1920 treatise by J. Ise or to the one written in 1956 by S. T. Dana.—*Arthur H. Westing*

Introduction to the Mathematical Theory of Genetic Linkage by NORMAN T. J. BAILEY; 298 pages; $8.80; Oxford University Press, 1961.

Although most geneticists will disagree with some of his reasons, nearly all geneticists will agree with C. D. Darlington's statement that crossing-

over is "the central fact of genetics." There is bound to be, in the near future, a recrudescence of interest in the more formal aspects of Mendelian genetics and for this reason a book like Dr. Bailey's is of real value.

The title of the book is likely to give rise to misgivings on the part of geneticists, who will assume it to be an abstruse and rarified mathematical exercise. But they will be wrong! While I have some quarrels with the book, it discusses matters of very direct interest to anyone concerned with experiments of formal genetics.

The book treats two matters. One is covered by the title of K. Mather's earlier book, *The Measurement of Linkage in Heredity*. A very complete discussion is given by Bailey of the way in which the various statistical problems of estimating linkage and testing the significance of observations can be handled. His treatment is *very* lucid and covers all of the disturbing factors such as differential viability and non-random segregation. There are nearly a hundred pages on the statistical problems which arise in human genetics and the book is worth while for this reason alone. There are, in addition, chapters devoted to the statistical problems which arise in bacterial recombination and in polysomic inheritance. All of this material is of great value to the experimentalist.

The other matter discussed is the theory of chromosome mapping, the mathematical models that have been evolved as description of the known patterns of interference. This is beautifully done, but seems unlikely to give us any insight into the mechanism of recombination so that it becomes model-making for its own sake.

The only significant omission in the book is the failure to discuss the statistical problems of tetrad-analysis in fungi and algae. This is a large and important field of linkage studies with its own special problems.

There is one serious misunderstanding. At several points Bailey says that the distinction between two-strand and four-strand models of crossing over is

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FROM REINHOLD

A review of chemical protection from radiation in living tissue

RADIATION PROTECTION IN MAMMALS

by JOHN F. THOMSON, *Division of Biological and Medical Research, Argonne National Laboratory, Argonne, Illinois.*

May 1962. 220 pages. $7.75

Few scientific subjects are of greater potential value for the future of mankind than those which will increase his ability to protect the body against ionizing radiation. In recent years considerable progress has been made in the study of several groups of chemical compounds. Chief among these are sulfur derivatives such as the thiol groups. The purpose of this book is to review the work done in radiation protection —most of which has been carried out on laboratory animals.

The author's experience in the field of pharmacology—evaluating and testing hundreds of chemicals for noxious, protective, or therapeutic properties in chemical warfare, eminently qualifies him to review the present knowledge of the mechanisms involved in the radiation damage of living systems and in the modification of this damage.

This important survey of the literature of radiation protection by chemicals will be of compelling interest to all biologists, biochemists, and health physicists.

CONTENTS: Preface/Introduction/Physiological Effects of Total Body Irradiation/Testing of Protective Agents/Protection by Environmental Modification/Protection by Thiols—Cysteine, Cysteamine, AET, and Their Derivatives/Protection by Other Sulfur Compounds/Pharmacologic Agents/Other Protective Agents/Mechanisms of Protective Action/Treatment of Radiation Injury/Current Status of Radiation Protection/Index.

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irrelevant until one considers the phenomenon of interference. In fact this is not true for the observation that recombination frequencies never (except in rare cases) exceed 50% leads us to the four-strand model. Otherwise we need suppose that for some mystic reason the probability of exchange between two strands is never greater than one-half.

When all is said and done, however, this is a well-written book which deserves the attention of geneticists.—*R. C. Lewontin*

Social Life of Early Man, edited by S. L. WASHBURN; 299 pages; $7.50; Aldine Publishing Co., 1962.

This book contains eighteen papers that are expanded and revised versions of those presented and discussed at a conference on the Social Life Of Early Man, held at Burg Wartenstein in 1959 under the auspices of the Wenner-Gren Foundation for Anthropological Research. The conference was attended by an extremely distinguished group of scholars who came from many different countries and who represented a variety of disciplines.

Since studies of Early Man cannot be undertaken through direct observation, evidence is necessarily scanty and every possible avenue must be explored; accordingly, a multiple-disciplinary approach is particularly desirable. Data relating to the life ways of other primates are of importance since the roots of human social life must be sought in prehuman contexts, and one third of the reports are concerned with apes and monkeys. Other papers deal with skeletal evidence, tool making, hunting, the use of fire, theories relating to the mentality and ideologies of Early Man, art, environment, genetics, and evolutionary processes. For those who do not read Russian and who are curious about the conceptual framework of Soviet anthropology, the report by G. F. Debets on "The Social Life of Early Paleolithic Man as Seen Through the Work of the Soviet Anthropologists" will be of special interest.

It is highly regrettable that the limitations of space do not permit the

listing of the titles in this book, which throw new light on the vastly complex problem of man as a social and physical being. It is a most stimulating volume that will be of value, not only to anthropologists, but to many other scientists as well.—*H. M. Wormington*

Amphibians and their Ways by H. Rucker Smyth; 292 pages; $6.50; The Macmillan Company, 1962.

Not many writers are able to deal entertainingly and accurately with biological subjects. George Gaylord Simpson, Marston Bates, and Rachel Carson are among the gifted few. But if biologists find it difficult to abandon their technical jargon and shift to a style acceptable to the general reader, amateur naturalists with literary ambitions seldom write in terms acceptable to the biologist.

Unsophisticated readers eager to learn something about frogs and salamanders will conceivably appreciate the chatty, unpretentious, personal style a Connecticut housewife employs in this book. Amateur naturalists may not be disturbed when they read that during the courtship of salamanders "it is the male's object to make himself irresistible to the female." The student of animal behavior, however, may wince at this, or shudder to note that the male "may even invite her to a dance if that is the accepted thing with this particular species." In the ponderous rhetoric of the specialist the female salamander responds to visual or olfactory stimuli, but in the less inhibited lingo of Mrs. Smyth, the female must be " in a receptive frame of mind" if she is to pick up the packet of sperm that will fertilize her eggs.

As an enthusiastic amateur naturalist Mrs. Smyth probably read the dozen authoritative books on amphibians she lists with other references in a short bibliography. Unfortunately, some of these were written several decades ago, and Mrs. Smyth evidently does not realize that what a competent authority writes one year may be outmoded the next. Consequently, a fair amount of the information in Mrs. Smyth's account is

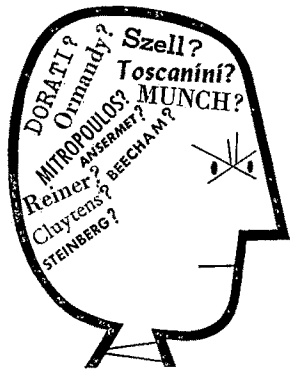
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out-of-date. Anyone familiar with the literature will be amazed to note that at times she raises questions when she should have provided answers.

There is no reason why a housewife who is familiar with the local amphibbians, especially if she has Mrs. Smyth's enthusiasm for her subject, and who has access to a good library, might not write a good book. It might be more readable than one written by the harried specialist. Mrs. Smyth would have been more prudent, however, had she asked someone with the insight and understanding of the specialist to read her manuscript. Readers undisturbed by her anthropomorphic phraseology will be distracted if not puzzled when they read that an eel-like salamander has "external gills and lungs." They may bog down completely when she adds that the same salamander has "four toes, ranging in size from 6 to 36 inches."

It is inexcusable for any book to be so liberally sprinkled with such egregious errors. With more knowledgeable and judicious editing the text might well have provided information with considerable appeal to the amateur naturalist.—*Charles M. Bogert*

The Integrity of the Body (A Discussion of Modern Immunological Ideas) by F. M. BURNET; 189 pages; $4.75; Harvard University Press, 1962.

In the preface the author says his book "was written primarily to provide the information about immunology that I should like to have if I were a botanist or a geneticist or a physician. . . ." Not being an immunologist, but feeling strongly that immunological information is of vast importance in problems of development, I decided to see whether or not Professor Burnet was able to achieve his goal. I am happy to report that he has done so quite completely and that not only is the book well written and interesting, but extremely valuable to a general biologist (and undoubtedly to a physician as well). Immunology is one of those subjects that can be made confusing and frustrating to the nonspecialist with the greatest

of ease, In this book the jargon and the techniques are kept to a minimum or clearly explained so that it is possible to follow the author.

This is a short book and undoubtedly many will not agree entirely with the emphasis. There is, for instance, a large amount of space devoted to Burnet's own clonal selection theory, and even though he presents opposing ideas, his own bias is very clear. However, from a developmental biologist's point of view, it seems to me that irrespective of whether or not the theory stands the tests of time, it is profoundly stimulating. This is equally true of his general discussion of the problem of the body knowing itself immunologically, as well as his all-too-brief discussion of the evolution of immunological mechanisms. It is fortunate that Burnet (like his co-Nobel laureate Medawar) is capable of seeing his own work in a wider context and at the same time can communicate it to others.—*John Tyler Bonner*

Propagation of Electromagnetic Waves in Plasma by V. L. GINZBURG, edited by W. L. SADOWSKI and D. M. GALLIK; translated from the Russian by ROYER and ROGER, Inc.; 822 pages; $38.00; Gordon & Breach, 1962.

Plasma physics is the protean and fashionable discipline concerned with the behavior of systems composed mainly of a neutral collection of electrons and ions in which the electromagnetic (EM) forces between the charged particles and the effect of external EM fields and boundaries that may be present are extremely important in determining the behavior of the systems. The adjective "fashionable" is used advisedly, for the number of people actively engaged in plasma physics and the number of papers appearing on this fascinating and important subject have increased exponentially during the last decade. As proof, one can point to the appearance of several new journals devoted almost exclusively to research in plasma physics, the creation of a Division of Plasma Physics by the American Physical Society in 1959, and the

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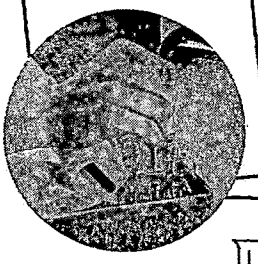
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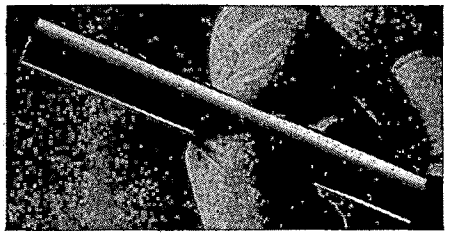
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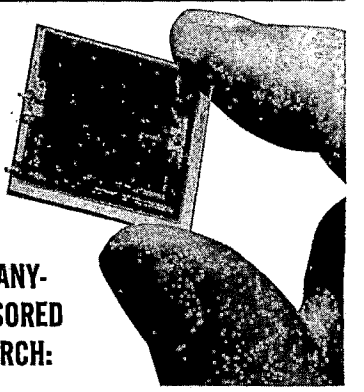
establishment of Plasma Physics Laboratories in numerous industrial and academic organizations.

By virtue of his numerous important papers in plasma physics and his well-known book, *The Theory of Radio Wave Propagation in The Ionosphere* (1949), Professor V. L. Ginzburg has long been regarded as one of the world's authorities on the propagation of EM waves. In this definitive book, in the interest of simplicity and the desire to obtain useful results, the author confines himself to the case of a plasma in equilibrium or quasi-equilibrium in velocity space. This rules out consideration of beams of charged particles in the plasma and questions of the stability of the system consisting of the beam and the plasma, and the study of turbulent plasmas. Chapter 1 is devoted to the fundamentals of the theory of the propagation of EM waves, with restriction of the exposition to the study of plane EM waves incident normally on layered plasmas. Chapter 2 is devoted to the propagation of waves in homogeneous and isotropic plasmas. The complex dielectric permittivity of a plasma is derived first using the macroscopic equations of classical EM theory and then, by use of the distribution function, by statistical mechanics. The Debye length is introduced and microprocesses in the plasma are treated briefly. Transverse and longitudinal waves are distinguished and their propagation studied. In Chapter 3, the complication of a static magnetic field permeating the plasma is introduced so that the plasma is anisotropic or magneto-active. With the Larmor frequencies of the electrons and ions as measures of frequency, the discussion is divided into the cases of high frequency and low frequency EM waves. Spatial dispersion is treated in an exceptionally clear fashion and the MHD approximation discussed in great detail.

In Chapter 4, the plasma is allowed to be inhomogeneous but is isotropic. The inhomogeneity is required to be so slight that the geometrical optics approximation is valid. Exact solutions of the wave equation are given for numerous interesting profiles of the

complex dielectric permittivity tensor. Applications to the case of the ionosphere are given, and the interaction of EM and plasma waves is studied. The chapter concludes with an analysis of the propagation of pulses and a good discussion of the energy density in a dispersive medium. In Chapter 5, the plasma is allowed to be both inhomogeneous and anisotropic. The same subjects are treated as in the previous chapter but with the added complication of anisotropy due to the presence of an external magnetic field. The reflection and transmission coefficients are calculated when singularities are present in the refractive index and subtleties such as the limiting polarization of waves emerging from a layer of inhomogeneous, anisotropic plasma, the effect of "tripling" the reflected signals and oblique incidence of waves on a layer are studied. In Chapter 6, the author treats his favorite subject, the propagation of EM waves in the ionosphere. This is a very complicated subject but with the background of the previous chapters he manages to do an amazing job in about ninety pages. Chapter 7 is devoted to the propagation of EM waves in the sun and in interstellar medium. In all the previous chapters, no steady, strongly homogeneous electric field permeated the plasma and only linear phenomena were studied. In Chapter 8, nonlinear phenomena in a plasma situated in a variable EM field are studied. The criteria for a strong electric field are first established and the problem treated by the elementary theory and then by kinetic theory, for both highly ionized and weakly ionized plasmas. The book concludes with a treatment of nonlinear effects of EM waves in the ionosphere.

This reviewer warmly recommends this book written with such great pedagogical skill by an incomparable master of the subject. It will profit workers in this field to consult it regularly in order to learn how to treat complicated problems by breaking them down into a series of simpler problems. There is no comparable book in English on this subject, and the publishers should be congratulated for



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The value of the book would have been greatly enhanced if a detailed index had been provided. The glossary is complete and very useful. Unfortunately, the translation is not very facile because the translators are obviously not experts in plasma physics. The print is not of the highest quality and occasionally cannot be read without undue eyestrain. The high price of the book would undoubtedly deter all but those deeply interested in the subject from purchasing it for their personal use. The reviewer is happy to report, however, that the publisher, Mr. Gordon, has informed him that an academic edition of Ginzburg's book is available to students and teachers at the greatly reduced price of $19.50. At this price, everyone interested in plasma physics can afford to own this important and useful book.—*Howard Chang*

Phosphorus and Its Compounds, Volume II: Technology, Biological Functions, and Applications, edited by JOHN R. VAN WAZER; 1107 pages; $35.00; Interscience Publishers, 1961.

Volumes I and II have been edited by Dr. Van Wazer of the Monsanto Chemical Company. Volume I was concerned with the modern chemistry of phosphorus and its compounds, and was meant primarily for the professional scientist. This volume deals with our knowledge of phosphorus from technological and biological aspects, and with applications in some ten manufacturing fields. It will have a wide range of readers including administrators, salesmen, and technologists as well as scientists. At the end, various patents dealing with the industrial uses of phosphates are listed. While all of this volume will not be of interest to any one person, those who deal with phosphorus in any applied field will find at least one section to read with profit.

The first six chapters are concerned with mining and manufacturing of phosphorus and its compounds. Three chapters are devoted to the biological functions of phosphorus. These chapters

are not applied in the sense the others are, in that they deal with the physiology and biochemistry of phosphorus in plants and animals. The last ten chapters are on food and dentifrices, detergent building, phosphorus in metallurgy and other topics of concern to the industrial chemist.

The three sections dealing with the biological side were of most interest to your reviewer. While it was impossible, in the space alloted, for these chapters to be exhaustive, he was impressed by the amount of information provided. The first chapter deals with the phosphorus cycle in nature, the occurrence of phosphorus in animal and plant tissue and in microorganisms, the ecology of phosphorus, and its role in life processes. The second chapter is concerned with all the synthetic processes in which phosphorus plays a part, such as phosphorus in genetic materials and replication, group-transfer agents, and energy processes in biological systems.

The third chapter deals mainly with the chemistry and physics of the apatite crystal and its functions in bone formation. The organic aspect of bone is rather sketchily dealt with, but enough is there to make the story intelligible. The so-called photomicrograph of normal human bone that has been included is actually of rat mandibular condyle! Among the industrial sections, the biologist will find two sections well worth reading, on plant and animal nutrition with respect to fertilizers and phosphates in feeds. The other chapters are written at the same high standard as the biological essays.

This book is dedicated to the "salesmen of phosphorus products who..... are expected to employ expert knowledge in the ever-widening fields of technology described herein." Our admiration of salesmen is greatly increased by this simple statement.

The price of the book will unfortunately restrict its sale, but is should certainly be on the library shelves of institutions dealing with any aspect of phosphorus, academic or industrial.—*James T. Irving.*

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A Flora of the Alaskan Arctic Slope by IRA L. WIGGINS and JOHN HUNTER THOMAS; 425 pages; $9.50; University of Toronto Press (Arctic Institute of North America Special Publication No. 4), 1962.

Our purchase of Alaska from Russia in 1867 made this great territory, and especially that part beyond the Arctic Circle, no more accessible to botanists. For generations, the botany of the Arctic Slope remained practically unknown except for casual and random collections made in great part near the coast by nonspecialists. The coming of the bush plane to arctic Alaska a few years before World War II, and especially the establishment of the Arctic Research Laboratory at Point Barrow in 1946 by the Office of Naval Research, finally opened this area to the professional botanist. As Scientific Director of the Arctic Research Laboratory in 1950 and for several seasons thereafter, the senior author had available to him unprecedented opportunties and facilities for the first intensive study of the flora of arctic Alaska.

This book, restricted in its geographic scope to the summit-line of the Brooks Range and its drainage northward into the Chukchi and Beaufort seas, provides the first comprehensive treatment of the higher plants of the largest tundra area of the United States. The Brooks Range consists of the northernmost projection of the Rocky Mountains that, in Alaska, lie completely north of the Arctic Circle. Chapter I, The Area, gives an extremely illuminating account of the physical conditions that affect and control the growth of plants, under the headings Geographic Limits, Edaphic Factors, Climatological Factors, Permafrost, Biological Factors, and Habitats. The main body of the book, consisting of keys for the identification of and descriptions of all taxa, covers 435 species in 153 genera and 57 families. Of the species, 16 are pteridophytes—ferns and their relatives—and only one is a conifer—a juniper. All the rest are flowering plants, with 109 monocotyledons and 309 dicotyledons. The largest genera are *Carex* (38

species), *Salix* (20 species), and *Saxifraga* (19 species).

It seems almost unfair to point out any real or fancied shortcomings in a pioneer work of this sort, especially in view of the many difficulties inherent in the original field work and of the frustrating delay in publication. However, many further collections by other botanists might with benefit have been cited in the text, even if not added to the distribution maps, if the manuscript had been updated just before publication. These omissions, probably beyond the control of the authors, may have the happy result of bringing about a revision of this book somewhat more promptly. The inclusion of the more important synonyms of plant names to relate this book to the works of Hultén and Anderson would have been very useful to the non-taxonomist who needs to include plant names in his reports. As one example, the generic name *Therefon*, used here for a handsome and conspicuous tundra plant without relating it to another name for the same plant, *Boykinia*, that appears in Anderson's widely-used "Flora of Alaska," is certain to confuse the non-taxonomist. Typographical errors are amazingly few in a work so replete with technical terms and Eskimo geographic names—the only major one noted is the misspelling of *Aconitum* on page 184.

For the first time, the working biologist who needs to identify plants from arctic Alaska has available a book designed expressly for this purpose by experts familiar with the area. The citation in the text of specimens actually examined by the authors and the known distribution of species shown by maps will be useful not only in the identification of plants but also in developing specific ideas of plant distribution in the Arctic, especially when related to the statement of the general range of each species. This book will undoubtedly take its place as one of the most original and most needed floristic works to be published during the current year.
—*William C. Steere*

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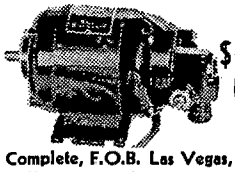
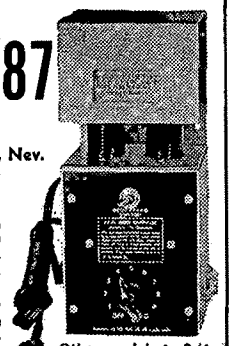
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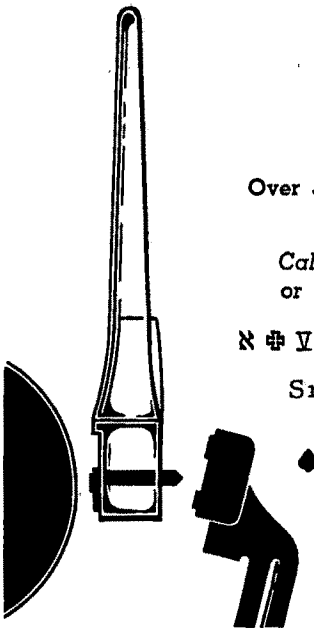
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Electrolytic Dissociation by C. B. MONK; 320 pages; $10.00; Academic Press, 1961.

This book systematically puts forth the evidence for the theory that, in strong electrolyte solutions, electrolytic dissociation, especially for polyvalent ions is not complete. The evidence is extensive, covering a wide variety of experimental techniques. The theory of incomplete electrolytic dissociation is shown to account for activity coefficients which are lower than are predicted by only considering the interionic attractive and repulsive forces as interpreted by Debye and Hückel.

The author intended this book to be of greatest value to people who are not already deep in research in electrolytic behavior. He has written the book, "... for those who are commencing research work involving ionic equilibria," and, it might be added, for those who want to know if they should be concerned about electrolytic dissociation in their present research. The first half of the book reviews the properties of electrolytes, their theories and their measurement: conductance, transference numbers, activity coefficients, reversible e.m.f. cells, solubility, freezing point, vapor pressure, partial molal quantities, and diffusion. This treatment is more detailed than that of most general physical chemistry textbooks. With this background in electrolytes the reader is prepared for the second half of the book—the interpretation of experimental data leading to the postulation and evaluation of incomplete dissociation. The data treated in this section are derived from conductance, e.m.f., pH, spectrophotometric, solubility, freezing point, ion exchange, solvent extraction, polarographic, Raman, and NMR measurements. The book concludes with a discussion of the kinetic and thermodynamic effects of incomplete dissociation.

The author has achieved his purpose in this book. The subject is well presented and does not require a special knowledge in the field. The effects of incomplete dissociation are made clear. In addition, the book has extensive



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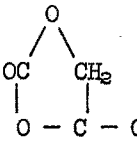
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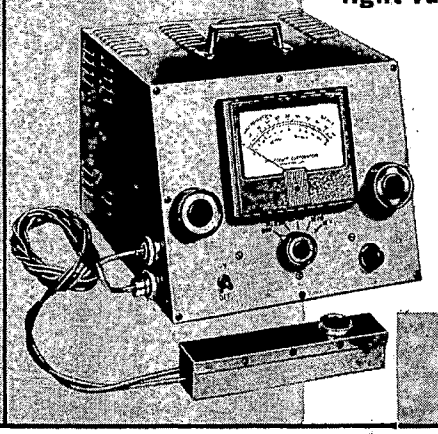
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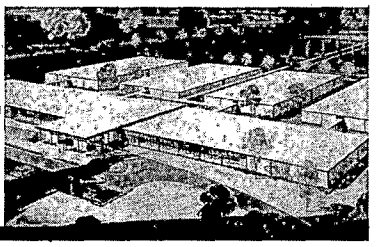
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Rhythmic Activity in Animal Physiology and Behaviour by J. L. CLOUDSLEY-THOMPSON; 236 pages; $6.80; Academic Press, 1962.

This is the first volume of a projected series of monographs on Theoretical and Experimental Biology. The book is concerned with those biological rhythms which have periods matching the diurnal, tidal, and lunar rhythms of the environment as well as with seasonal and reproductive rhythms and other related topics. Much of the recent work in this field was summarized in the 1960 Cold Spring Harbor Symposium and it is also unfortunate that the bibliography in Cloudsley-Thompson's book does not cite references more recent than 1959.

Space does not permit us to document fully the criticism which must be levied against this book. We can only call attention to a few examples of its shortcomings. The author's discussions of the major problems and ideas in the field can hardly do anything but confuse the reader. In some cases, such as the discussion in Chapter 9 on the inheritance or learning of rhythms, or the treatment of the independence of rhythms within the organism, the problems themselves seem not to be understood by the author. In other cases the author's apparent unfamiliarity with the literature has led him to make some rather extravagant statements. The statement, made on page 133 in discussing the time-sense of bees, "... one might infer that sun-navigation does not play an important role in the foraging of workers," might not be a typographical error for nowhere does one find a mention of the work of Lindauer. Again, the statement on page 42 that "... nearly all protozoan rhythms have been shown to be exogenous ..." following a discussion of *Paramecium* without any mention of the work of Ehret, Sonne-

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born and others on endogenous rhythms in this protist suggests an unfamiliarity with the literature. Some discussions, such as the treatment of the statistical analysis of rhythmic data given on page 22 are so inadequate that it would have been better either to have omitted them entirely, or to have expanded them to do justice to the topic.

Parts of the book are better than others and the chapter reviewing diurnal rhythms and animal ecology contains a great deal of information on this subject. Cloudley-Thompson's book deals with a subject for which there has existed a need for a good monograph, but unfortunately we must conclude that the need remains.—*Victor G. Bruce*

Comprehensive Analytical Chemistry, Vol. I-B: Classical Analysis, edited by C. L. WILSON and D. W. WILSON; 878 pages; $30.00; D. Van Nostrand Co., 1961.

This rather large book is the second in the series by the Editors designed to catalog the established science as well as much of the useful art of Analytical Chemistry.

The material presented in Volume I-B continues the discussion of classical analysis and is about equally divided between Inorganic Titrimetric Analysis (Chap. VII) and Organic Quantitative Analysis (Chap. VIII).

The latter provides a comprehensive description of organic elemental analysis including a section on organo-metallics. In addition, an excellent, detailed discussion (G. Ingram) of Functional Group Analysis is offered. This is followed by a treatment of titrimetry in non-aqueous media including a brief but provocative discussion of redoxometric processes.

Chapter VII is somewhat singular in its coverage of redoxometric, acidimetric, and precipitimetric methods with much of the discussion sharply bent toward the artful achievement of meaningful end points; many analytical procedures are listed at the expense of the chemistry involved.

Section 10 of Chapter VII provides an interesting survey of specially used titrimetric reagents. The descriptive section on Apparatus (Sect. 3), as far as it goes, should be thoroughly unenlightening to the average analyst. A discussion of volumetric calibration and errors caused by temperature effects on volumetric glassware might have proved quite useful here. Almost identical discussions of the theory of adsorption indicators appear on p. 110 and p. 226. Although this does save some effort on the part of the casual reader, such duplication in a single volume seems unnecessary and only contributes further to the high cost of the book.

The discussion by E. Bishop covering theory and principles is excellent for the degree of exactness with which it treats the limiting conditions of the relationship between equivalence point and end point. However, the practice of selecting titrand volume and titrant strength to conform neatly with percentage titrated subtracts from the general rigor of the discussion. This

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might have been overcome by adopting at the outset the exact and generally well-understood notion of fraction titrated, f.

An honorable but awkward-to-use sign convention (p. 117, *et. seq.*) for tabulated half-reaction potentials (really electrode potentials) has been employed which is quite likely to be a point of immediate concern for American readers.

Flaska and Barnard present a very good account of complexiметric EDTA titrations in Section 9; their well-developed analogy of α and β effects, their discussions of masking, rate of complexation (too brief) and indicator systems should prove of immediate utility to the analyst.

The present volume lays considerable stress on the "practical" aspects of chemical analysis; indeed as a reference to selected classical procedures, it would seem to be of genuine value to the practicing analyst.—*Edward D. Moorhead*

Quantitative Chemical Techniques of Histo- and Cytochemistry, Vol. I by D. GLICK; 470 pages; $14.50; John Wiley & Sons (Interscience Division), 1962.

Faced with the enormous expansion of the literature during the decade since his first book on this subject, the author has drastically reduced the scope of this edition by eliminating microscopic (qualitative) histochemical procedures for which other works are now available. This first volume of the revision deals with basic apparatus and methods for determination of mass and volume, microrespirometry, designs for glass electrodes, polarographic electrodes and oxygen electrodes, dilatometers and methods in fluorimetry. Volume II will cover titrimetry, spectrophotometry, flame photometry, and microbiological techniques.

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to K. Linderstrom-Lang of the Carlsberg Laboratory who laid the foundations of quantitative histochemistry during the 1930's. Many of the extensions of his ingenious devices have been made by investigators who were directly influenced by him, and these are the aspects of the field which are emphasized in this book.

More than a third of the text is concerned with "Gasometric Techniques." Microrespirometer designs are probably overemphasized, but the procedural details for manipulating Cartesian divers are extremely valuable. Unfortunately, a section on micro-methods for gas analysis is omitted.

Electrophoretic methods as applied to histochemistry are illustrated by only the older examples, and recent applications to fractionation of isozymes are not included. A good bibliography, list of manufacturers of special equipment, selected buffer tables, and subject index occupy fifty pages.

It is obvious that an author must compromise between his personal bias and comprehensive coverage. There are some notable gaps in this work, but an outstanding feature is the reliability of procedural detail stemming from firsthand experience. For the topics covered, this volume will be indispensable in the histochemical laboratory. —*William L. Doyle*

Fundamentals and Techniques of Mathematics for Scientists by M. M. NICOLSON, edited by D. R. HARTREE and D. G. PADFIELD; 526 pages; $7.50; John Wiley & Sons, 1962.

This book is the outgrowth of a British course intended to train scientists and engineers in applied mathematics to an advanced level. The subjects treated are similar to those in many American courses on applied mathematics beyond the level of elementary differential equations. They include: differential equations, series solutions, complex variables, Laplace transforms, special functions, vector analysis, matrices, tensors, and dimensional analysis. But, unlike American texts containing similar topics, the book under review also attempts to cover more basic material, i.e., elementary functions, differential calculus, (Riemann) integration, and infinite series. Since almost half of the 526 pages are devoted to the basic subjects, each of the advanced topics is necessarily given only brief treatment. On partial differential equations there is nothing, save for a few scattered remarks.

Reading this book requires considerable maturity. The exposition is terse, and incorporates a wealth of proofs and results not found in competing domestic texts. The reader is offered few (but well-chosen) examples, and no problems. In some sections, he is aided by explanations which are models of lucidity; in others, the exposition is clumsy and obscure. This uneven merit is a consequence of the death of the author, who left many entire sections to be completed by the editors.

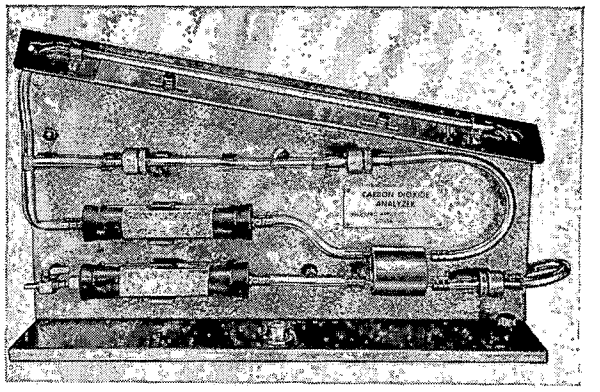
For those needing an occasional reference to the topics covered, this could be a useful and valuable book.

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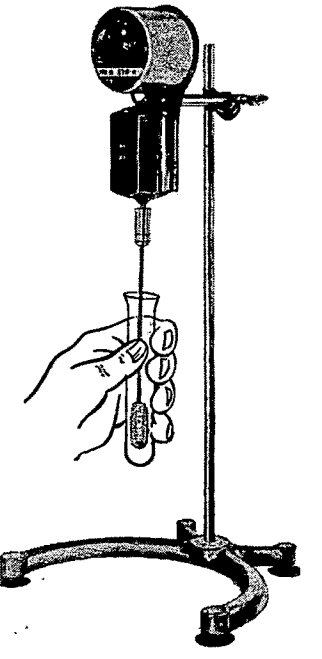
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Paleoecology of the Llano Estacado, assembled by F. WENDORF; 144 pages paperbound; no charge; one in a series of irregularly published publications on research in history and ecology of New Mexico and adjacent areas of the Fort Burgwin Research Center, Museum of New Mexico Press, Santa Fe, 1961.

The Llano Estacado (the Southern High Plains) includes some 20,000 square miles of eastern New Mexico and western Texas and presents to most travelers an excruciatingly dull countryside. The Spanish named the area the Llano Estacado or Staked Plains because, it is said, they had to stake their trail or lose their way back across it, so flat was it and so without landmarks.

Whatever his reaction to the modern landscape, the reader interested in the history of the last 30,000 years will find this an intriguing publication which sets out some new and important landmarks for the late Pleistocene of the Southwest.

Probably the most important contribution lies in the pollen analysis of geologic sections over a wide area. Such palynologic studies provide one of the most powerful tools in the reconstruction of the history of the Pleistocene. But, in the United States, we have lagged far behind our European colleagues and only within the last decade has there been much interest in the subject among American workers. Even this study of the Llano was done by a Norwegian, Ulf Hafsten of the University of Bergen, who came here particularly for this project. The pollen profiles of geologic sections have been made doubly valuable by the companion study of the patterns of modern pollen rain across the Llano. These studies of the present provide a firm point of departure for the historical analysis.

F. E. Green presents chapters on sand dune stratigraphy and on the geologic environments of pollen bearing sediments. Fred Foreman and Kathryn Clisby correlate a pollen profile with grain size and draw some environmental conclusions. Matthew Hohn and Joan Hellerman contribute an analysis of the diatoms from the Lubbock, Texas, area. Fred Wendorf assembles the data on the various collections of invertebrates. Kenneth Oakley contributes a brief appendix on radiometric assays and F. B. Lotspeitch, a somewhat longer one on soils.

The writers admit that they do not all agree on the final interpretations of the data—there is no reason why they should at this stage. Wendorf, however, offers a general interpretation of Late Pleistocene environments of the Llano Estacado and seems fair in presenting the various alternatives.

This is a welcome publication in late Pleistocene history, first because it is an honest attempt to bring a variety of data to bear on the historical reconstruction, and secondly because many of the data are new and badly needed.—*Sheldon Judson*

Immunodiffusion by A. J. CROWLE; 333 pages; $10.00; Academic Press, 1961.

The author of this book had the obvious and commendable objective of compiling the wealth of information which has accumulated on the technique of immunodiffusion. Immunodiffusion is a broad term used to describe a wide range of techniques by which antigens and antibodies are arranged to diffuse towards each other through suitable media. The antigen-antibody reactions occurring in all types of immunological procedures are subject to physical and chemical laws. Therefore, the author wisely included chapters in which such principles are stressed and related to immunodiffusion processes.

Among the problems encountered by those engaged in studies involving immunodiffusion is that of interpreting the precipitin patterns which appear as a result of antigen-antibody reactions within gels. A solution to this problem is

approached in this book through excellent illustrations which accompany material in the text. These descriptions provide a reference for the analysis of precipitin patterns which occur under the conditions imposed by a variety of techniques. By devoting approximately twenty-five per cent of the book to the applications of immunodiffusion techniques the writer emphasizes with examples their importance in effecting basic studies of many different antigens of interest to biologists. Furthermore, the potential value of such methods in separating and identifying antigens for diagnostic purposes in medicine is discussed.

Finally, the major techniques and modifications of techniques which have appeared in the literature have been assembled. Furthermore, the procedures are sufficiently detailed and concern the facets of immunodiffusion from the initial steps of antigen and antisera preparation to the final staining and recording processes. This information would aid the reader in selecting and implementing a method suitable for the solution of a particular problem. A glossary and a series of appendices contribute to the objectives of this volume.

This book would be a valuable adjunct to anyone either currently involved in immunodiffusion studies or contemplating the use of such techniques in the solution of biological problems.—*J. H. Gregg*

Mining Research (Proceedings of International Symposium held at The University of Missouri, February 1961), edited by GEORGE B. CLARK; two volumes; 859 pages; $30.00; Pergamon Press, 1962.

These volumes contain the papers delivered at the symposium held under the auspices of the Missouri School of Mines and Metallurgy, Rolla, Missouri. The symposium was international in scope: of the 50 papers presented, 25 were from foreign contributors representing 11 countries. A broad review of many of the areas in which mining engineering is interested were ably presented.

The investigation of mining problems commonly requires full scale field applications. Many of the results given at the symposium were arrived at through field observations.

Many factors contribute to the cost of mining. Important among these may be mentioned explosives and ground support (need for the latter is usually explained by using rock mechanics). Contributions discussing explosives and rock mechanics predominated. Several papers were devoted to advanced methods of ore analysis by spectroscopic techniques.

Since the middle 1950's, the use for commercial blasting of ammonium nitrate with an oxygen balancing agent has been widely investigated by manufacturers, users, and college researchers. In Volume I, some 12 or more papers, wholly or partly, are devoted to the use of ammonium nitrate blasting agent. Its use for surface and underground blasting, under widely varying restrictions as practiced in many countries, is discussed. Limitations of drill hole size; mechanical devices for loading holes; methods of priming; and optimum ratios between ammonium nitrate and oxygen balancing agents (usually a diesel fuel oil) may be cited among the factors receiving attention.

In Volume I are given data on presplitting—a preliminary blasting procedure used to prevent overbreak by the main blast; a comparison between chemical explosives and nuclear blasts which developed interesting comparative costs; and several contributions using the statistical approach for geological exploration with drill holes, for sampling, and for designing mine workings.

The contents of Volume II are confined mainly to rock mechanics, ground support and subsidence, and properties of rocks. Recent accomplishments by jet piercing (a drilling method which melts, granulates and ejects the particles to produce a bore hole) in the Lake Superior, low grade iron ores are explained. Later in the volume statically loaded, wedge shaped drill bits are quite thoroughly covered.

Physical properties of rocks, their

behavior, strength, and the stresses and strains developed through loading are investigated by applying various methods of measurement. Photoelastic coatings, strain gauges, time-relationship to recovery studies, statistical analysis, and shock waves generated by explosives are applied to laboratory specimens and rock *in situ* to interpret static and dynamic loading.

These two volumes present many of the problems under investigation by the mining industry. With insignificant exceptions the material is clearly presented. Minor irritations arise from the assumption that all readers will be familiar with the nomenclature and formulation used by the many contributors. But, considering the highly technical and mathematical nature of the many contributions, *Mining Research* is remarkably free from errors, typographical and otherwise. A more generous index would have been a decided improvement.

Dr. Clark, the Symposium Committee, and the many contributors providing finances for the Symposium should be commended. Pergamon Press has done a nice job of book making.—*W. W. Staley*

The Structure and Biosynthesis of Macromolecules, edited by D. J. BELL and J. K. GRANT; 132 pages; $5.50; New York; Cambridge University Press, 1962 (Biochemical Society Symposium No. 21).

In March 1961, the Biochemical Society (Great Britain) held a symposium in celebration of the Society's 50th anniversary. The symposium consisted of seven contributions on the structure and biosynthesis of nucleic acids, polysaccharides, and proteins with an introduction by F. G. Young.

Paul Doty reviews the interactions of synthetic polynucleotides extending the information gained from such studies to further knowledge of RNA and DNA secondary structure. Nucleic acid biosynthetic systems are described by J. N. Davidson with particular attention to the types of nucleotide addition to growing polymer chains and to primer requirements.

The primary structure of numerous polysaccharides is reviewed by E. L. Hirst with explicit emphasis on what is not known about the structure of these important substances. W. Z. Hassid discusses the enzymic synthesis of polysaccharides by transglycosylation from uridine diphosphate sugars and includes a discussion of the stereochemistry of such reactions.

The primary and secondary structures of myoglobin and hemoglobin, elucidated by high resolution X-ray diffraction analyses of the crystalline proteins, are described by M. F. Perutz. E. L. Smith, A. Light, and J. R. Kimmel examine the relationship between enzyme structure and specific catalytic function based on their work with the proteolytic enzyme, papain. Jacques Monod, Francois Jacob, and Francois Gros present a new hypothesis for biosynthesis of specific proteins and its genetic control.

The articles are all well written and up-to-date at the time of presentation, but they are all superficial because one cannot treat these subjects in depth in such short papers. With the exception of Hirst's paper, the references cited would enable the reader to examine the recent literature. Several typographical errors are present but most are not serious. The book is recommended to those with a general interest in these subjects, who have a knowledge of biochemistry, and who would like brief discussions of these important problems in molecular biology.—*David B. Straus*

Letters of the Lewis and Clark Expedition With Related Documents, 1783–1854, edited by DONALD JACKSON; 728 pages; $10.00; University of Illinois Press, 1962.

America's most famous and most successful exploring expedition was that led by Meriwether Lewis and William Clark in 1804–06 from St. Louis to the mouth of the Columbia River. The "writingest explorers of their time" returned with extensive written records,

but the whole story of the expedition was almost a hundred years in appearing, and even 150 years later some of the documents concerning the organization of the project and the printing of the reports remained unpublished, or in some cases were only published in partial form in widely scattered places. Professor Jackson's book now brings together these illuminating documents, which take us behind the scenes and give us a view of the origin, the execution and the aftermath of the great expedition that we could never get in a narrative account, or from the journals themselves.

On February 23, 1801, Thomas Jefferson wrote to ask General James Wilkinson to release his paymaster, Lieut. Lewis, to act as the president's secretary for, "In selecting a private secretary, I have thought it would be advantageous to take one who possessing a knolege of the Western Country... might sometimes aid us with informations of interest..." After what we may be sure were long discussions about an expedition to the Pacific coast, on June 20, 1803, Jefferson gave Lewis his written instructions not only to observe and accurately map the geography, but also to pay attention to anthropology, "the soil & face of the country, it's growth & vegetable productions... the animals ... the mineral productions of every kind... volcanic appearances; climate..." Probably Jefferson never intended to attach to the party any professional scientists, for four months earlier Jefferson had written to Benjamin Smith Barton that Capt. Lewis was to conduct the as yet unannounced expedition and that "It was impossible to find a character who to a compleat science in botany, natural history, mineralogy & astronomy, joined the firmness of constitution & character, prudence, habits adapted to the woods, & a familiarity with the Indian manners & character, requisite for this undertaking. All the latter qualifications Capt. Lewis has." Jefferson went on to assert that Lewis had a good working knowledge of some of the sciences, and asked Barton's help in advising Lewis on some important points. (Professor Jackson has written elsewhere of the dozen reference books taken and of their contribution to the scientific parts of the journals.)

Lewis and Clark were superb organizers and excellent geographers; Lewis was a good botanist and passable zoologist but, like Thomas Jefferson, had little interest in geology. The "do it yourself" science of Lewis and Clark is in sharp contrast to the work of the whole staff of botanists, zoologists, mineralogists, and geologists assigned to expeditions only a few years later and especially to those of Major Long. We now hope that Professor Jackson will perform a similar service for Major Long and show us by means of original documents why such a change in emphasis in science for exploration came about in less than two decades.—*George W. White*

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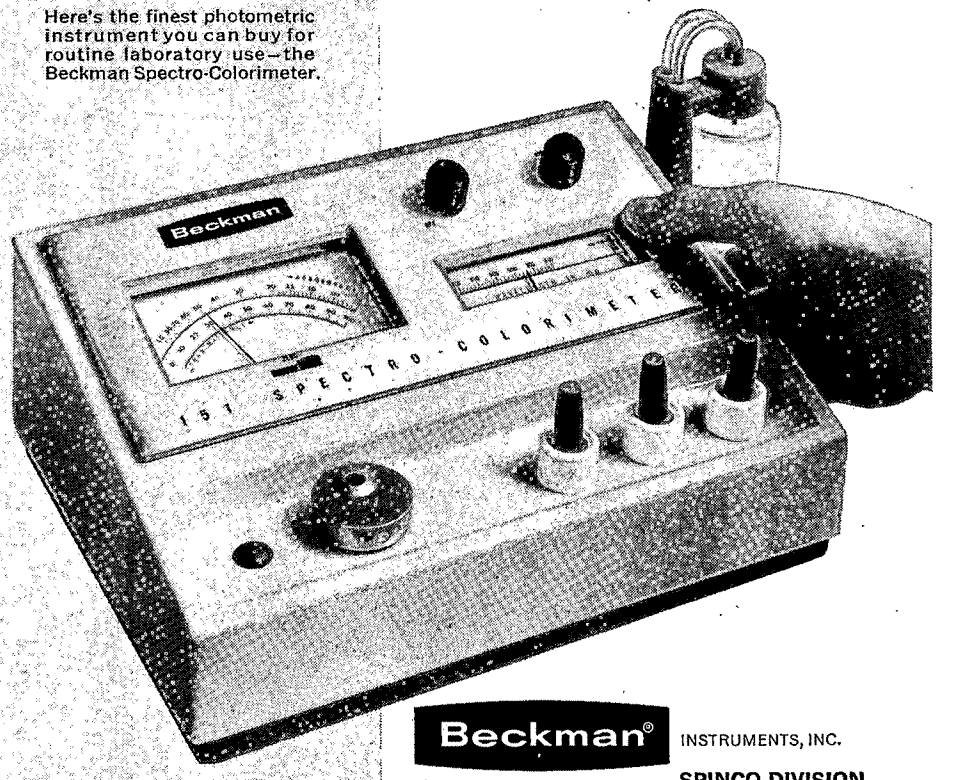
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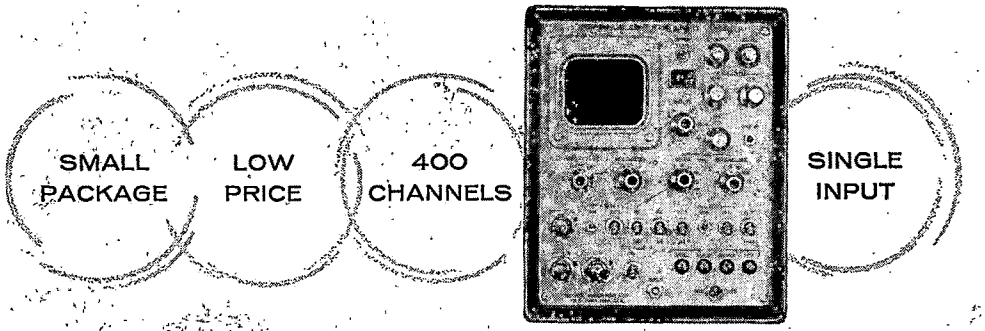
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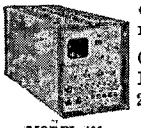
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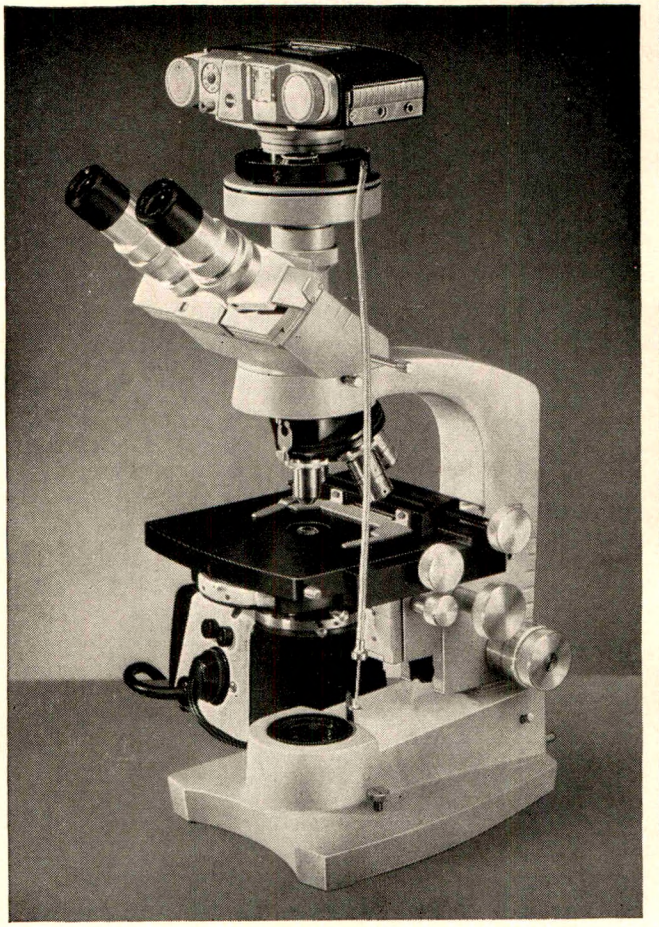
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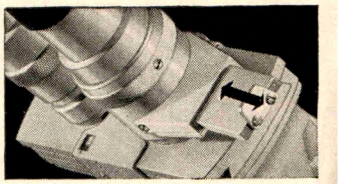
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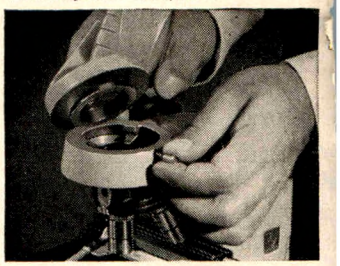
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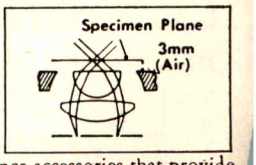


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Dr. Leopold, Chief Hydraulic Engineer for the U. S. Geological Survey, holds degrees in Civil Engineering, Physics-Meteorology, and Geology, the last from Harvard in 1950. His analytical Studies of Rivers have attracted much attention and earned for him (jointly with Thomas Maddock) the first award of the Bryan medal from the Geological Society of America. Address: U. S. Geological Survey, Washington 25, D.C.

WILLIAM SEEMAN and PHILIP A. MARKS, *The Behavior of the Psychologist at a Choice Point* 538

A Professor of Psychology at the University of Cincinnati, Dr. William Seeman, and an Assistant Professor of Psychology in the Department of Psychiatry in the University of Kansas Medical Center, Dr. Philip A. Marks, invest in this article a white rat, Herman, with something of the same quality that made Harvey a very real stage character some years ago. Through Herman we get an inside view of stresses and strains not only in the area of psychology but across the whole spectrum of sciences from the ultra-rigorous to those with "less rigor, less elegance, less precision, and perhaps a good deal more discouragement." Addresses: Dr. Wm. Seeman: Dept. of Psychology, University of Cincinnati, Cincinnati 21, Ohio; Dr. Philip A. Marks, Dept. of Psychiatry, University of Kansas Medical Center, Kansas City 3, Kansas.

TALBOT H. WATERMAN, *Revolution for Biology* 548

Professor of Zoology at Yale University, he has had a long-time interest in the eyes of invertebrates, and in particular in their reaction to polarized light and to such polarization as it exists under water. In this article, initially presented to the Yale Chapter of Sigma Xi, he provides a send-off for a special one term course in theoretical and mathematical biology that should be of interest to the readers of AMERICAN SCIENTIST.

PETER T. MORA, *Directiveness in Biology on the Molecular Level?* 570

Peter T. Mora, at present at Cambridge University at the Virus Research Unit on assignment from the National Cancer Institute, NIH, in Bethesda, where he is Head of the Macromolecular Chemistry Section, presents in this paper a heretical point of view for a chemist, clearly more influenced by his residence at Oxford University than by the time he spent at the du Pont Laboratories in Wilmington, Delaware. A chemist, who would prefer now to call himself a biologist, received his doctorate in 1948 at the University of Budapest, was a post-doctoral fellow at Princeton University, where he worked with a fellow Hungarian, Professor E. Pacsu.

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MONICA TAYLOR, *Ways and Waywardnesses of Amoeba* 576

A protozoologist, a member of the Community of the Sisters of Notre Dame of Namur, Belgium, has spent upwards of fifty years of her scientific life at the Dowanhill Convent, Glasgow, W. studying amoebae, their habitats and habits. Amoebae from Dowanhill were made available to biology laboratories during World War I and research workers across the world have similarly been supplied with Glasgow material.

DOUGLAS E. KELLY, *Pineal Organs: Photoreception, Secretion, and Development* 597

Dr. Kelly is an embryologist who has in recent years used an array of modern techniques to attack an ancient problem: the structure and the function of the pineal gland. In this article he gives us a comprehensive summary of his work and that of all the other recent contributions to this interesting subject. The author is presently on leave from the University of Colorado to continue these researches at the Anatomy Department of the University of Washington Medical School.

CHARLES C. GILLISPIE, *Perspectives* 626

The Professor of the History of Science in Princeton University returns here to the columns of *Perspectives* with a critique of *The Mechanization of the World Picture* by E. J. Dijksterhuis, recently translated from the Dutch by C. Dikshoorn and published by Oxford University Press in 1961. Director of a program for the doctorate in the history and philosophy of science, he is known also for his book, *The Edge of Objectivity*.

ANDREW T. HEREIM, *Are Scientists Becoming Administrators?* 358A

A Supervisory Operations Research Officer in the Logistics Operations Studies Division, at the U. S. Army Transportation Materiel Command, St. Louis, Mo., inquires into the problem of scientist, administrator or something of both.

IRA M. FREEMAN, *Populations: Particles and Physicists* 360A

A professor of physics in Rutgers, the State University, New Brunswick, N. J., examines what might happen on the basis of history since the 1890's if extrapolation governed the finding of elementary particles and the production of physicists in the years ahead.

DORIS H. CALLOWAY, JOHN J. P. BOSLEY and ORR E. REYNOLDS, *The Emergence of a New Technology* 362A

The authors who present their News and Views on the launching pad of Gastronautics are spatially separated. Drs. Calloway and Bosley are affiliated with the Stanford Research Institute in Menlo Park, California. Dr. Reynolds is Director of Bio-Sciences Programs of the National Aeronautics and Space Administration in Washington, D. C. The three authors combine interests in nutrition, social implications of space feeding, and aviation physiology respectively.

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G. MILES CONRAD, *New Developments in the Merchandising of Biological Research Information* 370A

Dr. G. Miles Conrad is the Director of *Biological Abstracts*, Philadelphia. He, more than anyone, is responsible for putting into effect new and significant ways of coping with the vast influx of scientific information. In this article he describes some of these new ventures.

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NEWS AND VIEWS

By the Board of Editors and the Membership of the Sigma Xi-RESA Societies

The Editorial Board welcomes the return of Professor Sheldon Judson, after a year's leave of absence. His work in the interest of the geological sciences on behalf of AMERICAN SCIENTIST has been ably assumed in his absence by Professor A. F. Buddington to whom we wish publicly to express our best thanks.

ANNUAL CONVENTIONS

In accordance with Article III, Section 1 of the Constitution, notice is hereby given that the 63rd Annual Convention of The Society of the Sigma Xi will be held on December 29, 1962, at 2:00 P.M., in association with the A.A.A.S. meetings at the Sheraton Hotel, Philadelphia, Pennsylvania. Each Sigma Xi Chapter and Club is entitled to be represented at the Convention by not more than three delegates. The Membership-at-Large is entitled to be represented at the Convention by one delegate for each member of the Committee on Membership-at-Large. All delegates shall be appointed in accordance with the provisions of the Constitution.

Convention Program

December 29—12:30 P.M.—Joint Sigma Xi-RESA Luncheon—Sheraton Constitution Room—RESA Address and Award of William Procter Prize

December 29—2:00 P.M.—63rd Annual Convention—Sigma Xi—Sheraton Constitution Room

December 29—8:00 P.M.—Joint Sigma Xi-Phi Beta Kappa Address—Sheraton Ballroom

Speaker: Dr. Loren C. Eiseley

Address: "Man: The Lethal Factor"

Sigma Xi Convention business will include:

I. Annual Reports of Officers and Committee Chairmen

II. Consideration of Executive Committee recommendations regarding:
   (*a*) Petitions for Chapter Status from:
      1. Medical College of Virginia
      2. North Dakota State University
      3. University of California—Riverside
   (*b*) Petition for dividing University of Arkansas Chapter into:
      University of Arkansas—Fayetteville Chapter
      University of Arkansas—Little Rock Chapter
   (*c*) National assessments and fees for 1963
   (*d*) Proposed revision of Article VII Section 2(*c*) of the Constitution. (This statement below)

III. Election of Officers:
   (*a*) President-elect for 1963–64
   (*b*) Member of the Executive Committee—term expiring 1966—to replace Dr. Harry F. Harlow whose term expires this year—1962
   (*c*) Member of the Executive Committee—term expiring 1966—to replace Dr. C. M. Jansky, Jr., whose term expires this year—1962

Suggested candidates for the above offices may be sent to any member of the Nominating Committee:

Dr. Herbert E. Longenecker, Tulane University

Dr. George Boyd, University of Georgia

Dr. Gerald J. Lieberman, Stanford University

Dr. George Moore, University of New Hampshire

Dr. Marsh W. White, Pennsylvania State University, Chairman

IV. General Business



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PROPOSED REVISION ARTICLE VII, SECTION 2(*c*) OF THE CONSTITUTION

Any investigator connected with a nondegree-granting institution not having a chapter who would otherwise be eligible for membership as stated in Section 2(*a*) of this Article, when the nomination by the Chapter's Committee on Admissions has been approved by a three-fourths vote of the (National) Executive Committee. *In those cases where the individual being nominated holds a doctor's degree from an institution now having a chapter of Sigma Xi it should be clearly indicated that the election is being proposed subsequent to having extended the privilege of election to the chapter at that original institution.*

RESA CONVENTION

The Fourteenth Annual Convention of the Scientific Research Society of America will be held at the Sheraton Hotel, Philadelphia, Pennsylvania, at 10:00 A.M. on Saturday, December 29. Each branch or club is entitled to be represented by three delegates but has only one vote. The agenda will include reports by the national officers and recommendations from the Board of Governors, the report of the Nominating Committee, and the election of three members of the Board of Governors to serve three-year terms. Reports on the AMERICAN SCIENTIST by Dr. Hugh Taylor, Editor-in-Chief; on the lecture program by President H. A. Neville, of Lehigh University, Chairman of the Lecture Committee; on the Grants-in-Aid of Research by Dr. Harlow Shapley; and on *Science in Progress* by Dr. Wallace Brode, will be included in the agenda. Fees for 1963 will be set by the convention.

The meeting will be followed at 12:30 by luncheon with the delegates to the Sigma Xi Convention. At 1:30 an address, open also to all those attending the A.A.A.S. Convention, will be given by Dr. Joel H. Hildebrand. The 1962 award of the Procter Prize will be made.

ARE SCIENTISTS BECOMING ADMINISTRATORS?

By ANDREW T. HEREIM

Are you a scientist working as an administrator of a technical group? Are you expected to be a proficient scientist as well as a competent administrator? How well do you do both? Are you a leader? There are those who feel that a person expected to be both is successful at neither. Perhaps it is possible to accomplish the administrative aspects of the job without knowing the technical portion, that is, be a good administrator only. Conversely, it is possible to be a capable scientist but not know the aspects of administration. Each has its forte and demands recognition.

Past experience seems to favor the administrator with greater promotional opportunities. Many scientists and engineers have been forced to assume administrative duties to qualify for promotion even though diffident about their abilities in performing this function. Take the case of a competent biochemist working in a research laboratory. After several years of devoted work he reached a "ceiling" of promotional opportunity. He was offered the alternative of an administrative position which allowed little if any time for research. He accepted because it included a promotion. In the case of a microbiologist, nearly the same thing happened. He accepted a promotion from the research field for which he was extensively trained to the administrative field with opportunities to advance. Should these men and others like them have stayed as enthusiastic, inhibited scientists, or become reluctant administrators? We appear to have reached the point which requires many scientists to make a choice—promotional limitations in their vocations, administration for which they may be incompletely prepared, or getting out entirely.

There appears to be a trend toward remaking scientists into administrators. Where is this trend leading us? It is depleting the supply of competent scientists by dividing their attention. It



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is reducing the supply of scientific talent by removing it from the area of most remunerative application. What does it cost to train a scientist, then retrain him as an administrator and lose part or all of the application of his scientific experience? An estimate of this cost should consider the loss to the scientific community and the frustration and limited productivity of the scientist during the transition. What can be done about it? A possible solution is suggested—a type of duality in supervisory responsibility.

First, recognize the contribution the scientist has made in his field and his future potential if he remains in his field. Then provide him with promotional growth opportunity. Do not set a low ceiling beyond which he cannot rise without becoming an administrator. Make the horizon equal to that of the administrator, but leave the scientist within his discipline. That is, generate a different status system and eliminate that which attracts or forces the scientist into administration. Allow him to remain a scientific specialist if he chooses. Do not force him to abandon his field; let him be a leader in it if he can.

Let the administrator be a specialist and leader in his own right. Provide a talent source by special training in this field; another talent source is the scientific field if apt candidates are available and willing to make the change under the different system.

Establish a supervisory role for the scientist and for the administrator along parallel lines and at equivalent levels. Make the scientist responsible for scientific decisions and the administrator responsible for administrative decisions. Let them each be leaders.

Perhaps this solution is incomplete, but it will alleviate some of the present limitations to growth of scientific talent in Civil Service and in industry. Expense of duality may be a limiting factor, but the release of scientific talent appears to be a great redeeming feature. We need leaders, men and women with talents in each individual field. Change to a new field will dilute individual talents. We are losing leaders this way.

POPULATIONS: PARTICLES AND PHYSICISTS

By IRA M. FREEMAN

In his delightful little book, "Elementary Particles,"[1] C. N. Yang lists the number of particles experimentally known at various epochs in the relatively brief history of this branch of physics. These figures, plus the very recently announced discovery of the second variety of neutrino,[2] which brings the total number of particles and antiparticles up to 32, prompted me to examine the figures with a view to seeing whether they exhibit any regularity. The results were surprising.

The following data were used:

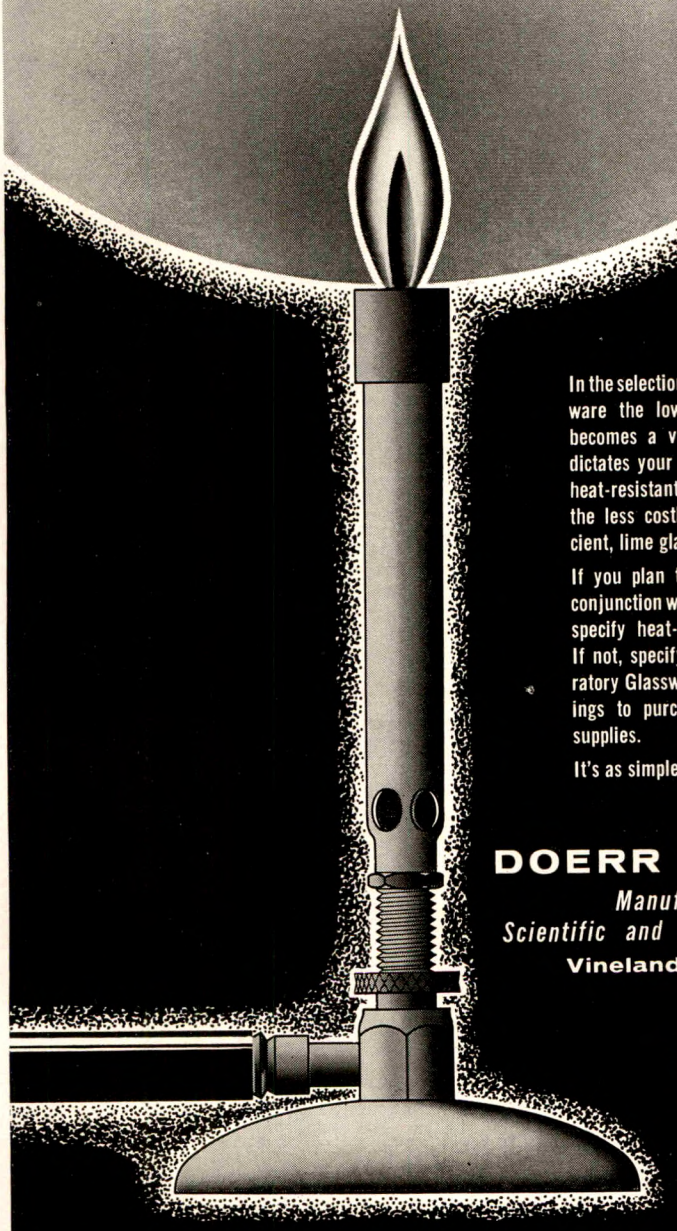
Year	1897	1913	1933	1947	1962
Number of particles, n	1, 2	3	7	14	32
Time since 1897, t, yr	0	16	36	50	65

The three middle entries are from Yang's charts. In the case of the very first entry, there is some question as to the propriety of stating that the electron was the only elementary particle known at that time. Evidence of a definite nature concerning the existence of the proton had been accumulating since Goldstein's discovery of canal rays in 1886, and further confirmatory work by J. Perrin and by W. Wien extended just about to the beginning of the century.[3] In reality, the choice of the figure to be used here does not affect the subsequent discussion to any appreciable extent.

The data were plotted on semilogarithmic paper, with n along the logarithmic scale as ordinate and t along the linear scale as abscissa. In order to allow for the virtual cessation of fundamental work in this field during the two great wars, the 1933 point was shifted toward the lower end of the time scale by 5 years and the 1947 and 1962 points were shifted in this direction by an additional 5 years.

The points were found to fall very close to a straight line whose doubling time turns out to be just about 11

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years—obviously identical with the sunspot period!

But, while the number of known elementary particles has been growing exponentially, the number of physicists has been growing, too. Exact figures are hard to obtain. However, by taking the number of American physicists to be of the same order of magnitude as the membership of the American Physical Society, it is possible to learn what the growth trend is. The data used were the following:[4]

Year					
1925	1930	1940	1950	1955	1962
Number					
1760	2480	3750	9470	11,700	18,600

Here again, the data were plotted on semilog paper. In this case, a correction was made for the stepwise increase in the number of physicists occasioned by World War II. The three data points preceding the war were found to lie quite nicely on a straight line. The last three points also fell on a straight line of about the same slope, but displaced upward by an amount equal to the normal increase that would have occurred during the war period, as given by extrapolating the first line. Within the errors of observation, *the corrected growth curve of the American physicist population was found to yield the same doubling time as the elementary-particle curve—11 years!* It is interesting to compare this figure with Weinberg's estimate of the doubling time of the U.S. research and development budget, 7 years, and that of the gross national product, 20 years.[5] The doubling time of the world population is presently estimated at 50 years, and that of scientific activity in general about 10 years.

The relatively short doubling rate of the elementary particles (and of the physicists) has some interesting consequences if it is assumed that this rate is apt to remain at about its present value for even a moderate length of time. For instance, the particle number will surpass the present number of known chemical elements before the year 1980 and will exceed the present number of nuclides (about 1300) not later than the first decade of the next century.

Another consequence of the above analysis is that the number of physicists will always comfortably exceed the number of elementary particles. The author was unable to find any reliable figure for the world physicist population, but by judiciously scaling up the American estimate, arrived at about 80,000. Thus there are some 2500 physicists to each particle variety, and presumably this will continue to be the case.

But suppose that the two growth curves turn out not to have exactly the same slope, but converge slightly. Then we have the possibility that, at some remote future time, the two numbers will attain equality. It is easy to calculate that if the particle curve were, say, 1 per cent steeper, equality would occur after a bit over 13,000 years—less than twice the time that human civilization has already existed. Thus, by the year 15,160, each physicist could be granted immortality by having a particle named after him.[6] Unless, of course, the particles should meanwhile get the upper hand through atomic chaos and give all mankind, physicists included, another kind of immortality.

REFERENCES AND NOTES

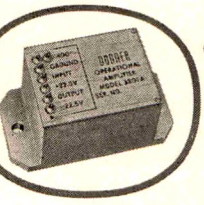
1. Princeton University Press, 1962.
2. M. SCHWARTZ, Report to Eleventh International Conference on High-Energy Physics, Geneva, July 1962.
3. GLASSTONE, S.: Source Book on Atomic Energy, van Nostrand, 1958, p. 41.
4. Bulletin, Amer. Phys. Society, II, 7, April 1962, p. 16.
5. WEINBERG, A. M.: *Science*, July 21, 1961.
6. The growing tendency toward large-team research may offer a complication here.

THE EMERGENCE OF A NEW TECHNOLOGY

By DORIS HOWES CALLOWAY, JOHN J. P. BOSLEY, and ORR E. REYNOLDS

Assuming that the environmental problems presented by travel in space, such as radiation and weightlessness, have been satisfactorily resolved, the

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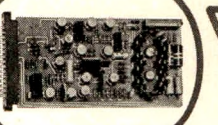
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remaining problem that we can anticipate is time.

Travel of man to even the nearest planets, Venus and Mars, will require several months in travel time and, if a deputation from Earth is to land and explore the extraterrestrial geography, the tour may be expected to consume the better part of a year. Whereas astronauts engaged in Earth orbital flight, or even taxi flights to rendezvous with Earth satellites for repair and maintenance missions, can undoubtedly survive on tubes of baby food or bonbons that taste like bacon and eggs, it would require a notable change in our concept of human needs for a mission to the planets to be consigned to such a dietary regimen.

Discussion of the ramifications of the problem led one of us (OER) to suggest that space travel of long duration may well require the development of a new interdisciplinary technology—gastronautics.

The essence of the subject is:

Now that we have assured ourselves that life in space is possible, what, in terms of the honored cliché, is going to make the living worthwhile? Deprived of most of the elements of human society, food might seem to offer happy prospects of ties with familiar experience and a minimum sort of reward. However, most of the programs suggested for feeding man in space have tended to ignore this very real and important role of food; happily, most of these suggestions can be dispatched on biological grounds without staging a hand-to-hand in the psychological arena.

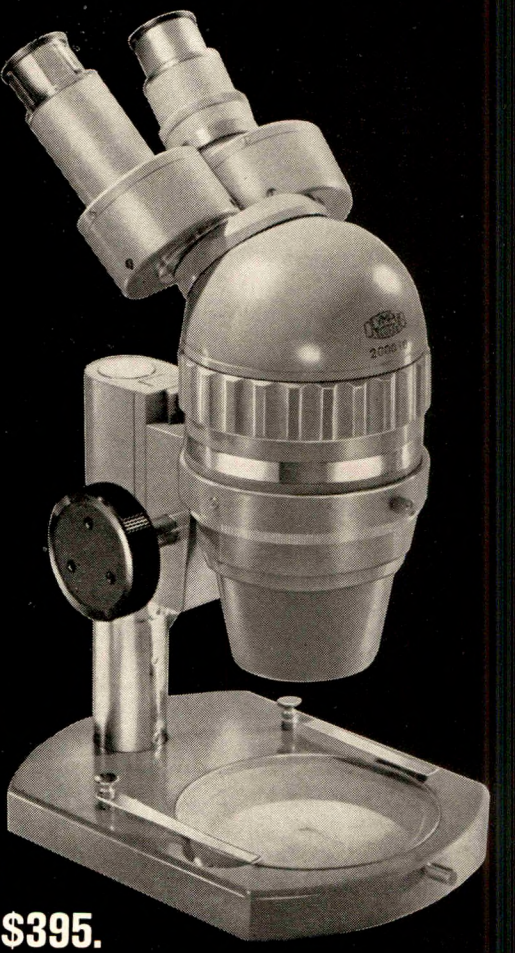
The State of the Art

First is the energy pill, famed in the annals of science fiction. This is pure fiction today and will probably remain so long after the technologic advances necessary to lift a three-year supply of steak-and-potatoes into space are reality. All attempts to supply fuel in biologically functional, energy-rich phosphate forms, such as adenosinetriphosphate and creatine phosphate, have proved futile; the compounds are immediately broken down in the body or internal manufacture is reduced until the normal balance is again achieved. So food must be provided.

About 1500 calories are needed in support of the requirements for such acitivities as respiration, digestion, and cardiac work. This basal requirement could be reduced about ten per cent by careful preconditioning with caloricallyinsufficient dietaries or by partial suppression of the thyroid gland. (Artificial hibernation achieves this same result at an even lower level.) Since reduction in the basal metabolic rate accomplishes a second useful purpose, that of increasing tolerance to hypoxia, such conditioning might be considered. Assuming a conscious state and the little activity allowable in the confinement of a capsule, however, about 2200 to 2500 calories appears a reasonable estimate of the space man's energy needs.

There has been no clear demonstration of altered requirement for essential nutrients in the space situation, although there is evidence suggestive of increased need for vitamins B_1 and C. To meet the normal essential nutrient requirements, then, about 50 to 70 grams must be devoted to protein and about 10 to 20 grams to minerals and vitamins. Provision of these nutrients alone thus involves swallowing a three-ounce pill, or 90 large-size capsules. Making up the remainder of energy needs in the form of fat, which at 9 calories per gram is the most concentrated source of biologically available fuel, involves gulping another 250 capsules, totaling over a half pound in weight.

A second suggestion has been made which embodies the advantage of reducing food packaging and preparation requirements to the bone. Why not fatten the space traveler like a Strasbourg goose and allow him to use up his own fat reserves en route? In this scheme, all that need be provided are the minimum essential nutrients and sufficient carbohydrate, about three ounces daily, to prevent acidosis and the other sequelae of total starvation. At zero gravity, the man wouldn't even lose weight! The crew might have to take turns pulling the strings a bit tighter on each other's space corsets, due



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to the loss of mass, of course. Because, in this instance, even if the suit doesn't fit, you still have to wear it!

Unfortunately, in studies of prolonged semistarvation in man a number of behavioral effects have been noted which dim enthusiasm for such a scheme: significant deterioration in perception of spatial relations and in word fluency; also, in tests of speed, of complex reaction time and coordination, and of duration of errors. At best, this would seem a short-term solution and incompatible with sustained efficiency.

In addition to these perceptual-motor disturbances, a more general personality alteration occurs under these conditions. The semistarved individual becomes chronically fatigued and irritable, passive and resigned—food fantasies come to occupy the major portion of cognitive and affective mental activity.[1] Thus, even if the informational qualities of the situation, the mechanical aids to decision-making, were sufficiently elaborate and sophisticated to allow for the decrements in performance due to food deprivation, there is the problem of keeping men-in-space sufficiently oriented toward the machine portion of the system that any response could be made. Perhaps the controls could be shaped to resemble food objects, to hold the attention of our explorers. Some caution would be required, however, lest the resemblance came to be so real as to induce the pilot to attempt to eat the hard-boiled egg atop the attitude-control lever.

Another intriguing possibility lies in the closed ecologic system. In this system one is presented, in so intimate a fashion as to be aesthetically discouraging, a miniaturized version of the events taking place in the macrocosm. Man eats food; food residues—gaseous, fluid and solid—are more or less converted into plant food; plants grow and produce oxygen; man eats plants and breathes oxygen; and so it goes. Other than aesthetics, which our intrepid explorers can surely overcome, there are certain deficiencies apparent in the food portion of the system.

The plants voted most likely to succeed in this system are the green, single-cell forms, algae. *Chlorella* is the plant most widely studied, but other algae and more complex but still rapidly-growing types, such as duckweed, have been considered. Whatever the oxygen-regenerative capabilities of these plants, the harvested crop leaves something to be desired as food. As *Nutrition Reviews* (*17*, 238, 1949) so succinctly put it: "The obvious objections . . . are . . . the relative deficiency of certain vital nutrients such as methionine, the incomplete digestibility and poor acceptability due to odor, flavor, and color." Test subjects have variously described the flavor as grass-like or similar to raw lima beans or pumpkin and the over-all effect as reminiscent of poached frog. Techniques could probably be worked out for converting raw algae into human food. Application of the techniques that have been suggested —extraction of pigments, blanching, browning, curing with smoke or salt, fermenting—might even provide a nice recreational outlet for the crew!

Conventional broad-leafed plants, like endive and celery cabbage, have also been planted in a CO_2—O_2 exchange system. As a food supply, the salad looms appallingly large, since about 15 square meters of leaf surface is required to generate the oxygen needed for one man.

At this point it is usual to suggest that something be added to the system in the way of a biological converter: molds, fungi, water fleas, shrimp, or perhaps even a rabbit or a goat. These possibilities, while unquestionably attractive, do seem to complicate the system just a bit. Complicate it so much, in fact, in terms of biological control, materials, equipment and weight, that one begins to wonder if it isn't a good idea just to pack a knapsack—at least until we're ready to establish a branch laboratory of the U.S.D.A. at each way station.

[1] If food deprivation becomes severe enough, the astronaut may even ignore the pin-up pictures he will presumably have tacked to the cabin wall, or alter his perceptions of them to the extent that these dishes begin to look delectable in more than one sense of the word. While this may seem dismaying, one square meal after return to earth will probably restore the astronaut's pre-flight behavioral set.

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Probable Directions

Faced with the necessity of planning food for a camping trip, one will quite naturally inquire how many are going, how long they're going to be away, what equipment is available for food preparation and storage, and who likes what. Present questions are much the same, with the added consideration that the party won't pass a supermarket for some time to pick up what may have been overlooked and there'll be little opportunity for gathering berries and nuts if the planning falls a bit short of the mark.

If the menu is developed around a variety of natural foods, it is unlikely that essential nutrient elements will have been omitted entirely, thereby neatly ducking the issue of whether man's requirements are truly known. This issue would intrude if a formula diet of some type were to be constructed, particularly with respect to trace minerals. For a short journey it would scarcely matter, but over a period of months or years, influences of molybdenum, selenium, chromium, and other more esoteric elements might appear as detours in the metabolic pathways.

Within reasonable limits, the selection of foodstuffs should be left to the crew. Military research shows that dissatisfaction and monotony are less apparent when the consumer participates in menu planning, even though the variety is limited.[2] A diet high in carbohydrate increases tolerance to hypoxia; fortunately, studies of air crews indicate a preference for breadstuffs and desserts in in-flight meals.

The foods from which the crew will be permitted to select must pass several preliminary tests. Stability under long-term storage—retention of palatability and nutrients—is essential. Their consumption must not cause gastrointestinal distress, whether due to spoilage or to excessive gas production from the interplay of intestinal bacteria and food residues. (In this connection, it would be well to examine the possibility of introducing into the man organisms which might contribute nutrients useful to him and produce less of noxious end-products like methane gas.) Lest space become a free-floating garbage dump, wrappers have to be edible or readily degradable, and not all foods can be so packaged. Crumbling and dusting are to be avoided as hazards in air conditioning, both personal and environmental. Preparation must be possible with a minimum of equipment and dispensing cannot rely on gravity flow.

Since most of the weight of food is water, and water can be recycled once a sufficient balance is established, dehydrated foods are the prime candidate. In this form, about 500 lbs per man per year will suffice. Dehydrated foods are appearing in ever-increasing variety, they are easy to prepare and store, are quite stable, and can be flexibly packaged. Further studies of compression may effect additional savings in space. Psychologically there might be some aversion to diluting one's soup with one's own repurified dishwater—but this aversion must be overcome if the trip is to be made at all. In this connection, it would probably help to have the source of "fresh" water as far away as possible—physically and psychologically—from the "kitchen sink."[3]

In light of the physical and nutritional factors discussed so far, it is apparent that many of the psychological gratifications that accrue to the circumstances and activity associated with normal meals in every Western culture will probably have to be foregone for a long time to come. The usual image of a table set with solid crockery and silverware, and laden with a variety of attractively prepared foods, will be just a fond memory for the extraterrestrial traveler, with his carefully measured and economically packaged food, intended to be eaten as quickly as possible, with a minimum disturbance of routine.

[2] The limitations on variety, of course, would be placed there only after extensive investigation of the individual preferences of the small group to be sent aloft, and with due consideration of the necessity of having everyone aboard agree on a basic set of food items. De gustibus disputandum est, est, est!

[3] In some systems water may actually be present in excess as a by-product from fuel cells. We have taken the dim view, or kitchen-cynic concept.

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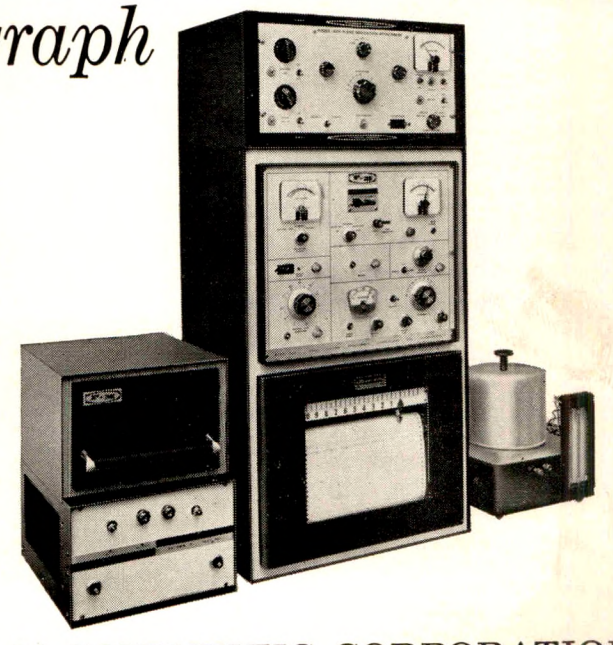
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One factor that is likely to detract from food-related gratification is decreased amount of food, perhaps mediated through decreased amount of behavior devoted to feeding, regardless of nutritional adequacy. One strong connotation of meal-taking in much of Western society is that it is a leisurely activity, a relaxing experience, and removal of this source of relaxation from the life of men in space would seem necessary with present plans.[4] The loss of a portion of normal sensory support and inability to dissociate consumption of food from other activity will also add to the burden of stress upon the voyagers, and may necessitate careful screening of astronauts to weed out persons who have a great deal of psychological investment in the process of nutrition. Otherwise, a retraining of candidates to encourage better integration of feeding with other leisure time activities might help, as would an attempt to accelerate the onset of relaxed, satiated feelings so that smaller quantities of food would do the trick. One intriguing innovation might be Freudian packaging, as an attempt to lick the relaxation problem; alternatively, we may have to develop an extremely durable chewing gum with especially lasting flavor (pastrami perhaps?) to supply the needed oral gratification.

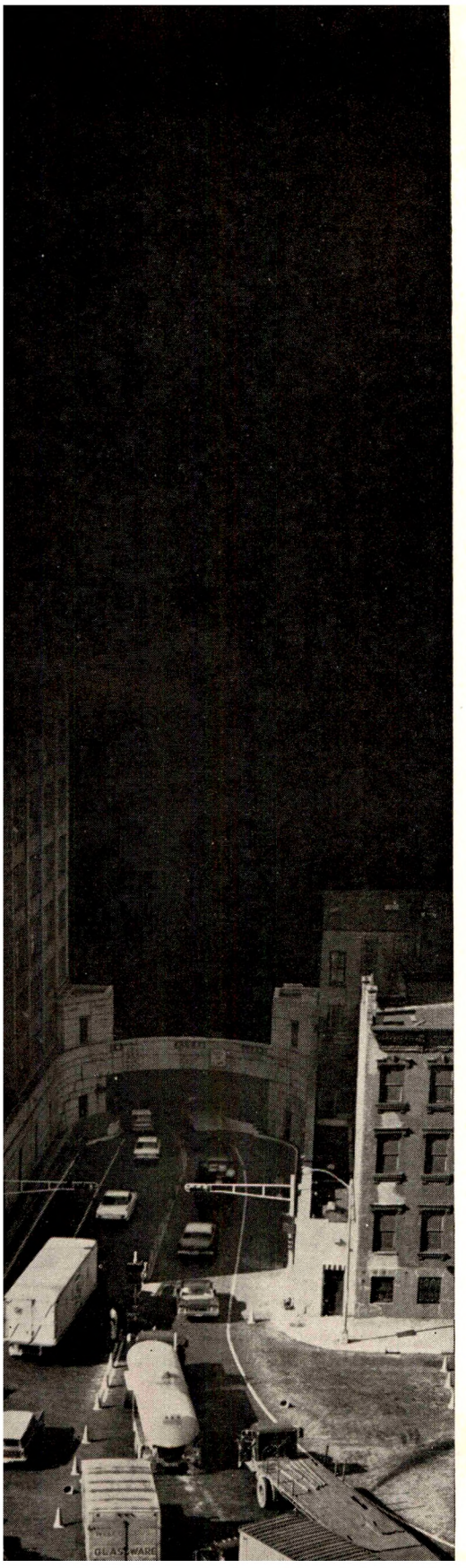
NEW DEVELOPMENTS IN THE MERCHANDISING OF BIOLOGICAL RESEARCH INFORMATION

By G. MILES CONRAD

As interest in the application of giant computers to the problems of the storage and retrieval of scientific information has flourished and grown in recent years, we at *Biological Abstracts* stood wistfully by hoping that we might somehow harness these phenomenal memories and manipulative skills and speeds to our service. However, most efforts to apply computers to the information problem had been directed toward the establishment of *static* information *centers* to which enquiries must be made one-by-one. It would be impossible to turn to such a central storehouse of knowledge on the spur of the moment — for not only must one seek out the appropriate center, but one's request must take its place in line, be programed, etc. While it is possible that some of the new machine systems being developed for the storage and retrieval of information might have been modified to serve our internal editorial processes, we were seeking for some machine system that would be of relatively direct value to our subscribers. A periodical "magazine form" information service must carry to each subscriber a complete information center for his personal use which is capable of providing instant service.

Realizing that the ever-expanding literature of biology would tend to create ever-increasing delays in conventional, manual index preparation, *Biological Abstracts* for a number of years had been searching for a way of producing a *current* subject index to its contents. We sought not merely for automatic data processing devices (which are legion), but rather for a system or method of subject indexing which would be susceptible to machine handling and which at the same time would be efficient economically and in the subject information it provided. The necessary attributes of the system we sought were (1) a minimum of editorial intervention; (2) adaptability to the printed page; (3) high speed of assembly in order to meet a semimonthly publication schedule; and (4) a reasonably high degree of usefulness to the reader. In some respects these desiderata seemed to be mutually incompatible and our search was prolonged and unrewarding until in 1959 H. P. Luhn of the IBM Corp. developed a new system of indexing which seemed to meet our requirements.

[4] Even more out of the question is the ability to provide alcoholic beverages as an accompaniment to meals, as is customary in several countries—unless a spacecraft that uses ethanol as fuel is developed, with small auxiliary tanks for storing various essences to be added in mixing drinks. To avoid quarrels among the crew, addition of essences should be programed electronically in advance. We recommend 4:1 but will entertain discussion on this point.



One of a series briefly describing GM's research in depth

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This new system, known as KWIC (Keyword-in-Context) indexing, is a "permuted" word index which arranges the titles of articles alphabetically under each of the words in each of the titles. While in appearance a permuted title index does not provide the same elegant arrangement we have become accustomed to in conventional indexes, it does meet the need for a self-contained "information center" which has been designed for the printed page and its speed of assembly—through the medium of the computer—makes it ideal for the prompt, indeed *rapid*, indexing of the huge volume of today's biological research literature.

Conceived as a means by which the author's own words in the title to his article would serve as the indexing rubrics and the accompanying words of his title would serve as the modifiers, the KWIC system was especially well suited to a world-wide and comprehensive service such as *Biological Abstracts*. Most other information systems, computerized or not, interpose between the author and the system some sort of third-party "regularizing" of input. That is, an indexer or coder converts the author's words or concepts into rubrics or codes that fit a predetermined scheme or arrangement of knowledge. Or, if the information is to be derived from a small controlled population, authors are instructed to word their titles or pick rubrics from a fixed list that is compatible with the prearranged scheme. Aside from the philosophic arguments pro or con the advantages of third-party indexing, from *Biological Abstracts*' point of view such intervention would be most costly and time-consuming and would keep us from our goal of a rapidly produced current subject index. Further, because of the world-wide spread of those authors whose work would eventually find its way into our pages, it was virtually impossible to provide each of them with instructions and, especially, lists of acceptable rubrics. We decided, upon practical grounds alone, that we would depend upon the author's desire to make known to the world the main lines of his thesis and, thus, depend upon his choice of appropriate keywords in his title. Our subsequent experience, we feel, has confirmed this assumption.

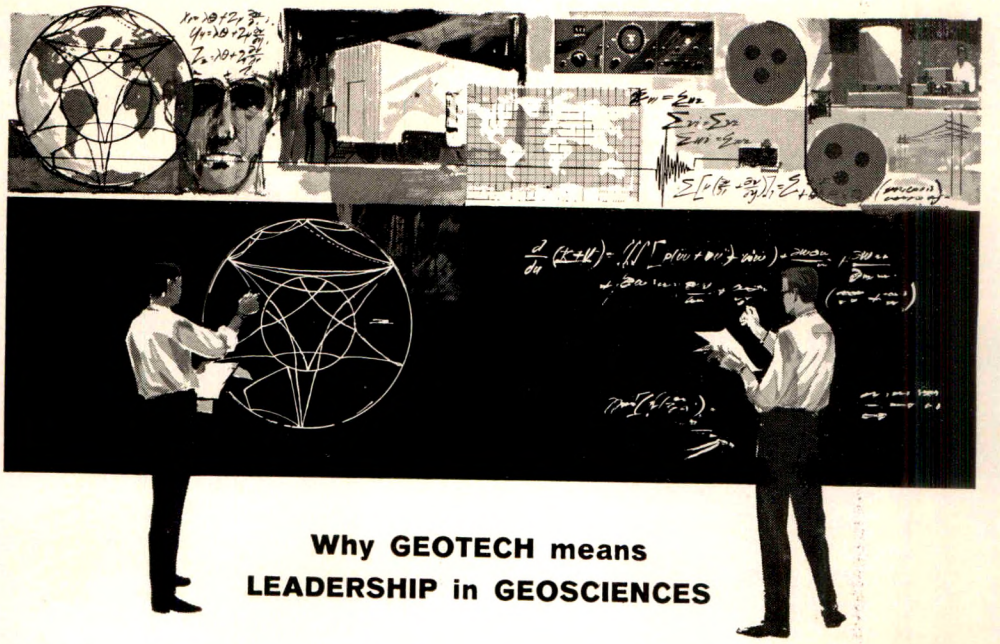
Detailed studies of the suitability of biological titles as keys to the information of biology led us to the conclusion that the relative specificity of most such titles would serve admirably for permutation. Our studies, conducted throughout 1960, led us to initiate on October 15, 1961, as a regular part of each issue of *Biological Abstracts* a concurrent permuted title subject index. This new index, which differs from KWIC in that it leads the user directly to an abstract rather than to a citation alone, we have called B.A.S.I.C., an acronym which stands for Biological Abstracts Subjects in Context. The reaction to this innovation from our users was immediate and enthusiastic.

Now we had found a way to harness the dramatic advantages of the computer to the service of *Biological Abstracts*. If it could solve so successfully the problem of rapidly indexing 100,000 and more abstracts per year, perhaps it could also help with some of our other problems.

One of the major stumbling blocks in the provision of information services is not to be found in the mechanics (hand or machine) of analyzing and indexing information, but rather in the merchandising of the information itself. Advertisers have proven many times over that the world does *not*, without urging, beat a path to the door of the maker of better mouse traps. Better mouse traps, like poor ones, must be carried to the market.

Among our merchandising problems, three have loomed importantly in the past—first, the reluctance of individuals to pay for information; second, the extremely limited market for any body of *highly* specialized information; and, finally, the market limitation imposed by the language barrier.

People, we find, just don't seem willing to pay for information until its lack is unquestionably related to their financial, legal, or physical well-being. To some extent, of course, career-related information as it pertains to the foregoing, is a matter of importance. But even here one is led to believe that it is only at that moment when a lack of information



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hurts or is an impediment to personal progress that we encounter a willingness to exchange money for information.

Because of this apathy regarding information—until it is needed, or seems to be—we came to the conclusion a number of years ago that the information in *Biological Abstracts* was not doing the job we felt it should and could. We wanted to see *Biological Abstracts* not as a relatively passive tool of research, but as an active and dynamic instrument for research progress. To achieve this we felt we must bring to the direct attention of the individual biologist a summation of the current research being reported for all of biology. This summation should be in such a form that he could easily and rapidly select from the total those items which were of direct and immediate interest to him. Finally, its price should be so nominal as to present no difficulty to the individual's purse—no more than five dollars per year and, preferably, one or two dollars.

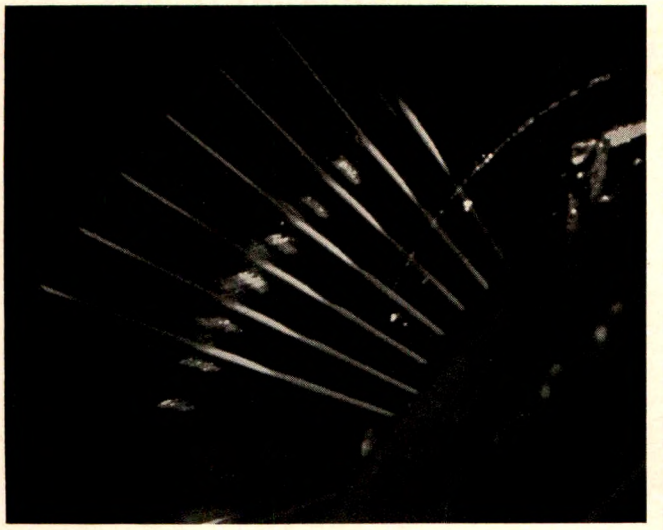
When we first began to think along these lines, there seemed no immediate hope that we could achieve such a service in the foreseeable future. However, Luhn's development of KWIC; its adaptation to the needs of *Biological Abstracts;* and, especially, its enthusiastic reception by biologists emboldened us to attempt the immediate achievement of our ultimate goal—the direct notification of as many individual biologists as possible of the existence of current information of interest to them.

In short, we would circulate to as many individual biologists as possible this new index to *Biological Abstracts*. As we reviewed our earlier studies and discussions, and fully aware of the reluctance of people (including biologists) to part with cash in exchange for anything but needed information, we decided to give *B.A.S.I.C.* away free of charge! This give-away would, in effect, be an ultra-detailed catalog to the information that could be purchased through a subscription to *Biological Abstracts* or by direct purchase from us of the desired abstracts. Daring (or foolhardy?) though this decision might seem, it was made on the basis of the sound and successful experience of a large group of "controlled circulation" periodicals of merit serving numerous professions and industries that give gratis subscriptions to influential persons in those professions or industries. Such journals realize their costs of operation and profits from the sale of advertising. Because such periodicals are read and used by specialists who directly influence the purchase of specialized materials, equipment and services, advertisers are pleased to use them as especially effective media for their advertising. There seemed to be no reason why we could not do likewise. As a result of this thinking, *B.A.S.I.C.* was launched as a separate controlled-circulation periodical on April 1, 1962. (B.A.S.I.C., as an index rather than as a periodical, continues to form part of each issue of the parent periodical, *Biological Abstracts*.)

Never in the thirty-seven year history of our organization has an innovation in service generated so much spontaneous praise and acclaim. Within a few weeks of the introduction of *B.A.S.I.C.* librarians reported substantial increases in the regular use of *Biological Abstracts* and individual research biologists began to write us with enthusiasm regarding this new concept in biological communication. During the three months following the first appearance of *B.A.S.I.C.* we received almost 600 letters and postcards commending this addition to the information services provided by *Biological Abstracts*.

This, then, is the way by which we hope to solve our first merchandising problem, *the reluctance of individuals to make a monetary exchange for information until it is needed*. If we can provide as many individual biologists as possible with a "catalog" to BA's informational wares via the medium of *B.A.S.I.C.* we believe we can ensure the necessary level of income to sustain all elements of our service on a continuing and sound financial basis. Some individuals will have enough use of information to warrant their personal or institutional purchase of *Biological Abstracts* upon a regular subscription basis, others may have only an occasional need and can take advantage of unit prices.

Our second important merchandising



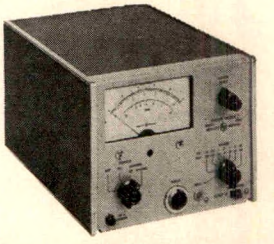
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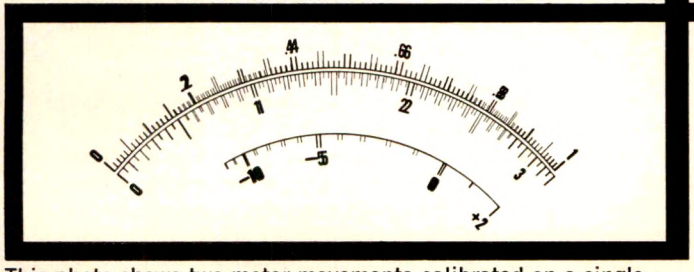
The universally accepted d'Arsonval movement used in modern electrical meters possesses an inherent flaw, "tracking deviation," which is the product of mechanical inconsistencies in the meter movement and which causes differences between the actual electrical quantity under measurement and the meter needle indication or deflection.

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problem is one of "packaging." Self-evident though it should be, it is important to reiterate that as a "package" of information becomes more highly specialized, the market for that package becomes smaller. In 1939 *Biological Abstracts* initiated five special sectional editions or "packets" of specialized information to meet the needs and pocketbooks of individual biologists—*General Biology; Basic Medical Sciences; Microbiology, Immunology, Public Health and Parasitology; Plant Sciences* and *Animal Sciences*. However, the ever-increasing emphasis on interdisciplinary research makes it ever more difficult to provide the ultraspecialized information required by individuals and special interest groups. To pre-package such specialized services is not only very expensive but very liable to the personal biases of the packagers. The only feasible way, we believe, to provide the biological community as a whole with the specialized information it needs, is to provide a rapid, efficient, and effective means by which each individual can choose the information he needs and wants from the entire body of the world's research in all fields of biology. This *B.A.S.I.C.* can, and is, doing. Because *B.A.S.I.C.* is demonstrating that it can do this job better and at far less expense to the individual, we are discontinuing the five separate sectional editions of *Biological Abstracts* at the end of 1962.

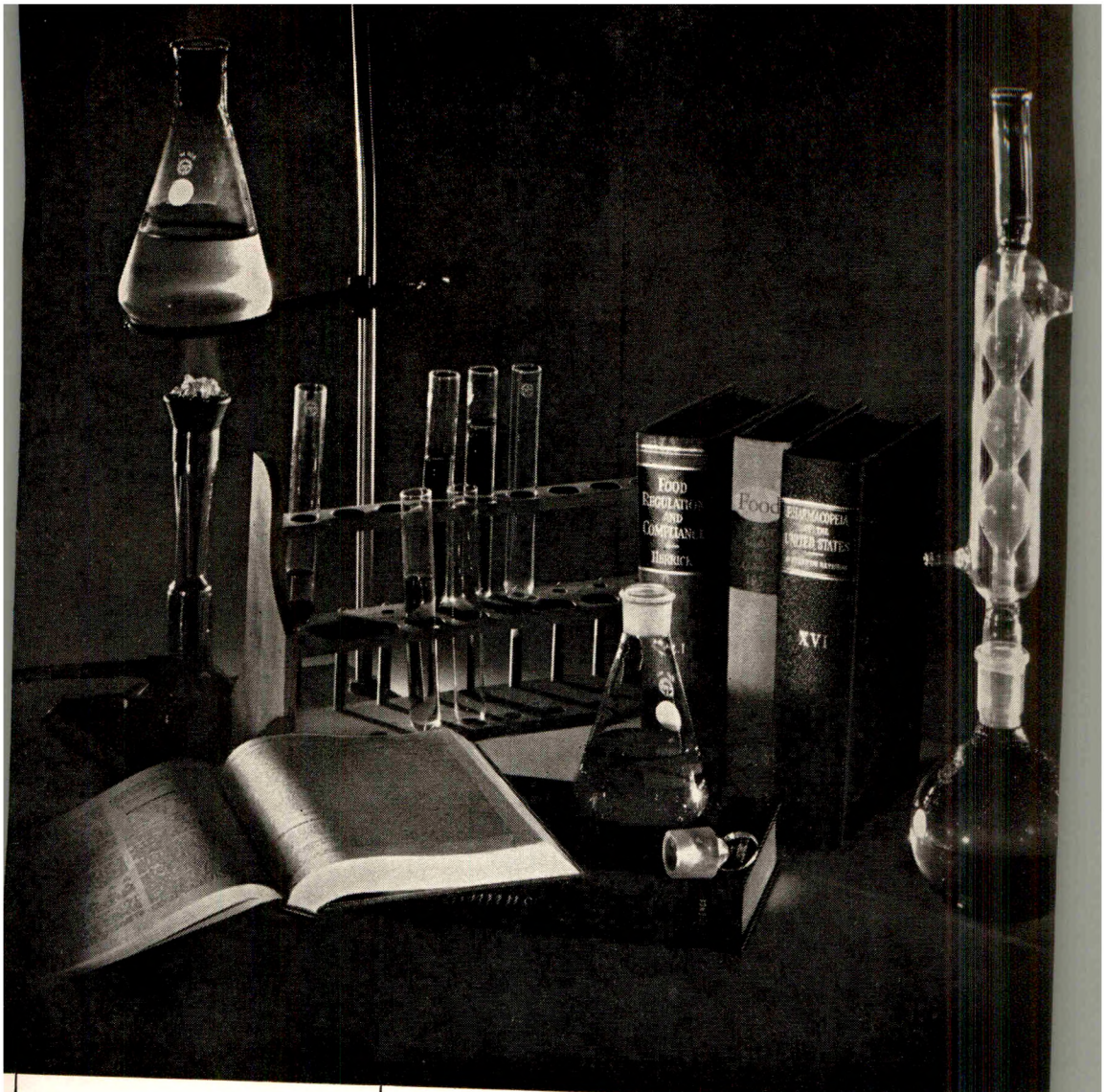
In place of these sectional editions we are, in effect, offering as many information packages as there are individual biologists. Using *B.A.S.I.C.* as a catalog it is possible for anyone to order for a small fee copies of any current abstract published in *Biological Abstracts*.

Further, although it is the primary function of an abstracting and indexing service to provide for the widespread dissemination of research information throughout the world, we are not unmindful of the growing responsibility of our organization in the area of customized literature service for individuals and special interest groups. To provide the greatest good for the greatest number of biologists it will probably be many years before the periodical format of *Biological Abstracts* can be supplanted by other means of storing, disseminating and retrieving information. However, the least common denominators of arrangement, indexing, etc., that must characterize a widely distributed abstract journal destined for the use of biologists working in all areas of biology must be supplemented by systems and devices which will enable us, upon demand, to provide highly specialized biological information.

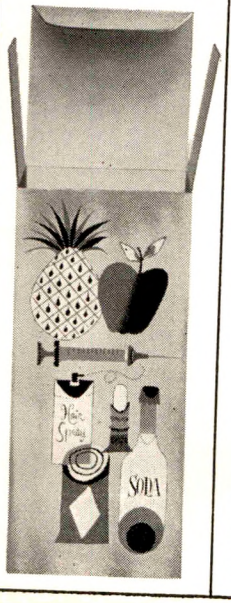
To this end we are currently experimenting with several partially automated procedures that will enable us to provide biologists with quite specialized information grouped in any of a great variety of ways. One of these systems, CROSS (Computer Rearrangement of Subject Specialties), is now being subjected to a series of tests to determine its value in providing service on a practical level. It looks most promising, and we hope to be able to announce its availability for customized service before many more months.

We have seen how one product of the computer—the permuted title index—has made possible two major changes in *Biological Abstracts*' methods of information dissemination, through the introduction of new merchandising approaches. Another advantage afforded by this same product deals with the limitation on our market imposed by the language barrier.

Although *Biological Abstracts* was originally established in 1926 to bring to the biologists of the United States news of research pursued throughout the world, it quickly became the principal international medium for the communication of biological research information. Today, approximately half of our distribution is outside of the United States. Abstracts drawn from the publications of 85 countries are distributed through the pages of *Biological Abstracts* to pure and applied biologists in 109 different countries. Because of this world-wide distribution—which seems to belie the notion that language is a barrier to our merchandising—we would like to improve our techniques so as to make our service more readily available to an ever-widening circle of biological research scientists and technologists in these many countries.



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Biological Abstracts has achieved this wide circulation and influence partly by the fortuitous accident that English has become the lingua franca of science, read and understood *in some degree* by a sizable proportion of the world's scholars. We have reaped the rewards of this situation with relatively little effort on our part. However, as world biology becomes increasingly dependent upon our service, it becomes *BA's* responsibility to make our services more easily used by those whose mother tongue is not English, for although many claim fluency in English use, our experience indicates that such claims are often somewhat exaggerated.

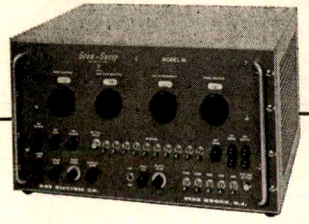
From time to time during past decades, for example, *Biological Abstracts* has been urged to provide a Spanish or an Interlingua edition for circulation south of the border. Investigation has always confirmed the *a priori* assumption that translation costs would be astronomically high and the time required, even granted the funds, so great as to make such an effort quite impractical.

Reception of *B.A.S.I.C.*, however, has been so extremely enthusiastic by English-speaking biologists that we are excited by the possibilities of producing permuted title indexes in one or more additional languages. As contrasted with the translation of the complete text of *BA*, the creation of a B.A.S.I.C. index in any language requires only that the titles be translated and punched on cards. Alphabetical arrangement, permutation and "typesetting" is completely automated and, for 5000 titles, takes only two hours to accomplish.

A Spanish *B.A.S.I.C.*—and this is the first foreign language with which we are experimenting—would enable the Spanish-speaking person to locate immediately those few specific abstracts of direct importance to him without the necessity of scanning many pages of information in a relatively unfamiliar language. It would pinpoint those items of interest on which he could concentrate and translate or have translated.

Although I cannot announce at this time that we shall commence the publication of a Spanish B.A.S.I.C.O. (for Biological Abstracts, un Servicio de Informacion Cientifica Organizada) at any definite time in the future, it is our hope funds can be found or earned to make such a project soon possible.

In summary, then, it is seen that the permuted title index—made economically possible only by means of the modern computer—has enabled *Biological Abstracts* to make a revolutionary shift in its role in communications from that of a relatively passive archive of past research to an active and dynamic instrument for research progress in biology.



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The Model M provides a complete measurement system, including—logarithmic, linear and manual sweeps, or a calibrated c-w signal; sharp, "crystal," pulse type frequency markers and precision step attenuator.

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The Model M is a double heterodyne sweep generator employing three crystal controlled oscillators. Either linear or log sweeps are available with center frequencies continuously variable between 20 cps and 200 kc. The swept output is blanked during retrace time, providing a zero-voltage base line. To eliminate phasing adjustments, a sawtooth voltage, synchronized with the swept output, is available to drive the X-axis of the scope.

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For checking high-Q circuits and low frequency response characteristics, variable rep rates down to 0.2 cps are available. This wide choice of rep rates, continuous to 25 cycles, plus a 30 cycle lock, permits selection of the optimum rep rate which gives an accurate response display of the circuit being tested, plus ease of viewing on the scope screen.

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Flatness: ±0.5 db over widest sweep.

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BOOKS RECEIVED

(1962 publications unless otherwise noted)

Proceedings of the Symposium on Electromagnetics and Fluid Dynamics of Gaseous Plasma, Brooklyn Polytechnic Institute, April 1961, edited by J. Fox; 468 pages; $8.00; John Wiley & Sons (Interscience Division).

Laboratory Planning for Chemistry and Chemical Engineering, edited by H. F. LEWIS; 522 pages; $20.00; Reinhold Publishing Corporation.

Laboratory Organization and Administration by K. GUY; 386 pages; $12.00; St Martin's Press; London: Macmillan & Co., Ltd.

The Science Myth by M. PYKE; 179 pages; $4.00; The Macmillan Co.

Methods of Forensic Science, Vol. 1, edited by F. LUNDQUIST; 659 pages; $19.95; John Wiley & Sons (Interscience Division).

Rapid Microchemical Identification Methods in Pharmacy and Toxicology by F. AMELINK; 121 pages; $9.00; John Wiley & Sons (Interscience Division).

Tales of Ten Worlds by A. C. CLARKE; 245 pages; $3.95; Harcourt, Brace & World.

Static Power Convertors, Performance and Application by R. WELLS; 276 pages; $7.00; John Wiley & Sons.

The Science of Ourselves by W. N. MCBAIN and R. C. JOHNSON; 217 pages; $3.50; Harper & Bros.

Russian Reader in Pure and Applied Mathematics by P. H. NIDDITCH; 166 pages; $2.25; John Wiley & Sons (Interscience Division).

Cambridge Papers in Social Anthropology (No. 3): Marriage in Tribal Societies, edited by M. FORTES; 157 pages; $4.85; New York: Cambridge University Press.

Electroluminescence by H. K. HENISCH; 368 pages; $12.50; Pergamon Press (The Macmillan Co.).

Chordate Morphology by M. JOLLIE; 478 pages; $8.75; Reinhold Publishing Corporation.

The Evolution of Living Organisms (Symposium marking centenary of Darwin's "Origin of Species" and of Royal Society of Victoria, Melbourne, Australia, Dec., 1959), edited by G. W. LEEPER; 459 pages; $22.50; Melbourne University Press; New York: Cambridge University Press.

Temperature, Its Measurement and Control in Science and Industry, Vol. 3, C. M. HERZFELD, Editor-in-Chief; *Part 2*, Applied Methods and Instruments, edited by A. I. DAHL; 1094 pages; $29.50; Reinhold Publishing Corporation.

From the American Association for the Advancement of Science, 1515 Massachusetts Ave., N.W., Washington 25, D.C.: *Spermatozoan Motility*, Symposium of the American Societies of Zoologists and General Physiologists at AAAS meetings New York, December 1960, edited by D. W. BISHOP; 313 pages; $6.50 to AAAS Members; *Great Lakes Basin*, Symposium of AAAS at Chicago, December 1959, edited by H. J. PINCUS; 308 pages; $6.50 to AAAS members.

Physical Properties of Polymers by F. BUECHE; 354 pages; no price given; Interscience Publishers.

Neurosecretion, Proceedings of 3rd International Symposium, University of Bristol, Sept., 1961, edited by H. HELLER and R. B. CLARK; 455 pages; $14.50 (90s.); Academic Press Ltd., Berkeley Sq., London, W.1, England.

Methods of Biochemical Analysis, Vol. 10, edited by D. GLICK; 399 pages; $14.50; John Wiley & Sons (Interscience Division).

Principles of Radioisotope Methodology by G. D. CHASE and J. L. RABINOWITZ; 372 pages; $6.00; Burgess Publishing Co.

Gas Chromatography by J. H. KNOX; 126 pages; $3.25; John Wiley & Sons.

Run, Dig or Stay? (A Search for an Answer to the Shelter Question) by D. BRELIS; 196 pages; $3.50; Beacon Press.

The Exploitation of Natural Animal Populations (Symposium of The British Ecological Society, Durham, March 1960), edited by E. D. LE CREN and M. W. HOLDGATE; 399 pages; $10.75; John Wiley & Sons.

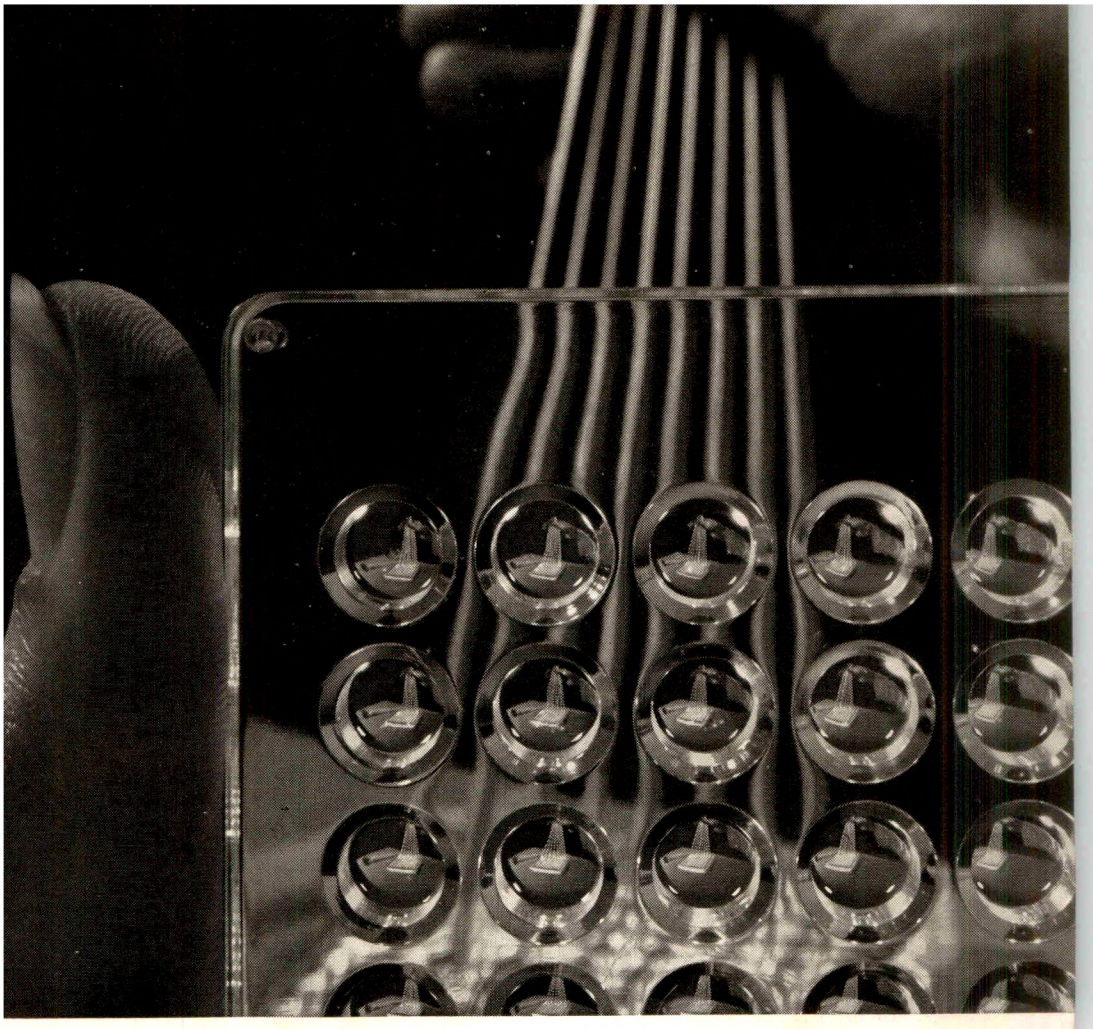
Luminescence of Organic and Inorganic Materials (N. Y. U. International Conference), edited by H. P. KALLMANN and G. M. SPRUCH; 664 pages; $16.00; John Wiley & Sons.

Astronomical Techniques, Vol. 2 of Stars and Stellar Systems, edited by WM. A. HILTNER; 635 pages; $12.50; The University of Chicago Press.

Gas-Liquid Chromatography, Theory and Practice by S. DAL NOGARE and R. S. JUVET, JR.; 450 pages; $13.95; John Wiley & Sons (Interscience Division).

The Design of Engineering Systems by W. GOSLING; 247 pages; $9.25; John Wiley & Sons.

Advances in Applied Microbiology, Vol. 4, edited by W. W. UMBREIT; 251 pages; $10.00; Academic Press.



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Advances in Petroleum Chemistry and Refining, Vol. 5, edited by J. J. McKetta, Jr.; 545 pages; $20.00; John Wiley & Sons (Interscience Division).

Underwater Medicine by S. Miles; 328 pages; $10.00; J. B. Lippincott Co.

Introduction to Polymer Chemistry by J. K. Stille; 248 pages; $6.95; John Wiley & Sons.

The Age of Electronics (M.I.T.'S Lincoln Laboratory Decennial Lectures), edited by C. F. J. Overhage; 218 pages; $7.95; McGraw-Hill Book Co.

Russian-English Dictionary of Modern Terms in Aeronautics and Rocketry by M. M. Konarski; 515 pages; $30.00; Oxford: Pergamon Press; New York: The Macmillan Co.

The Great White Mantle (The Story of the Ice Ages and the Coming of Man) by D. O. Woodbury; 214 pages; $4.95; The Viking Press.

Reflection and Refraction of Progressive Seismic Waves by L. Cagniard, translated and revised by E. A. Flinn and C. H. Dix; 282 pages; $11.00; McGraw-Hill Book Co.

Intelligent Machines (An Introduction to Cybernetics) by D. A. Bell; 98 pages; $2.95; Blaisdell Publishing Co. (Random House).

Man's Physical World by J. E. Van Riper; 637 pages; $8.50; McGraw-Hill Book Co.

The Opium Alkaloids by D. Ginsburg; 111 pages; $6.50; John Wiley & Sons (Interscience Division).

Brain and Behaviour in Cephalopods by M. J. Wells; 171 pages; $4.50; Stanford University Press.

Topics in Chemical Physics (based on the Peter J. W. Debye's Harvard Lectures) by A. Prock and G. McConkey; 277 pages; $11.00; Elsevier Publishing Co.

Treatise on Analytical Chemistry, Part 2, Analytical Chemistry of the Elements, *Vol. 2*, Gallium, Indium, Thallium, Silicon, Germanium, Iron, Cobalt, Nickel, edited by I. M. Kolthoff and P. J. Elving; 471 pages; $18.00; John Wiley & Sons (Interscience Division).

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Progress in Aeronautical Sciences, edited by A. Ferri *et al.;* 240 pages; $12.50; Pergamon Press.

Chemical Rocket Propulsion and Combustion Research by S. S. Penner; 158 pages; $5.75; Gordon and Breach.

Varactor Applications by P. Penfield, Jr. and R. P. Rafuse; 624 pages; $15.00; The M. I. T. Press.

Chemical Spectroscopy by R. E. Dodd; 340 pages; $8.75; Elsevier Publishing Co.

The Morphology of the Earth (A Study and Synthesis of World Scenery) by L. C. King; 699 pages; $13.50; Hafner Publishing Co.

High Speed Testing, Vol. 3 (Third Annual Symposium, Boston, October 1961) by A. G. H. Dietz and F. R. Eirich; 104 pages; $5.00; John Wiley & Sons (Interscience Division).

Conflict in Space (A Pattern of War in a New Dimension) by M. N. Golovine; 146 pages; $4.50; St Martin's Press Inc.

The Life of Birds by J. C. Welty; 546 pages; $9.00; W. B. Saunders Co.

Growing Up in River City by R. J. Havighurst *et al.;* 189 pages $4.50; John Wiley & Sons.

Psychoanalysis and Civilization by P. Rosenfels; 222 pages; $3.95; Libra Publishers.

Lost Tribes and Sunken Continents (Myth and Method in the Study of American Indians) by R. Wauchope; 155 pages; $3.95; The University of Chicago Press.

Shock Waves in Chemistry and Physics by J. N. Bradley; 370 pages; $11.00; John Wiley & Sons.

Acrolein, edited by C. W. Smith; 273 pages; $12.50; John Wiley & Sons (Interscience Division).

Principles of Aeroelasticity by R. L. Bisplinghoff and H. Ashley; 527 pages; $25.00; John Wiley & Sons.

Fundamental Concepts of Mathematics by R. L. Goodstein; 279 pages; $7.50; Pergamon Press (Macmillan Co.).

Foundations of Galois Theory by M. M. Postnikov, translated by A. Swinfen; 109 pages; $6.50; Pergamon Press.

Textbook on Mechanized Information Retrieval by A. Kent (Library Science and Documentation, *Vol. 3*, edited by J. H. Shera); 268 pages; $9.50; John Wiley & Sons.

The Eye, edited by H. Davson, *Vol. 2*, The Visual Process, *Vol. 3*, Muscular Mechanisms; *Vol. 2*, 796 pages, *Vol. 3*, 323 pages; no price given; Academic Press.

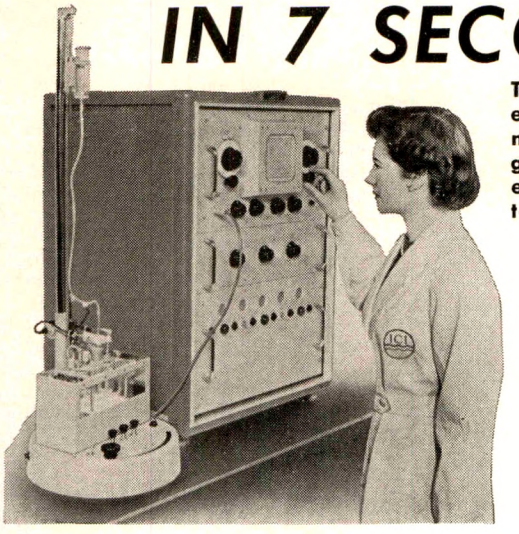
Flows in Networks by L. R. Ford, Jr., and D. R. Fulkerson; 194 pages; $6.00; Princeton University Press.

Inorganic Chemistry (A Textbook for Students of Pharmacy, Medicine and Allied Sciences) by L. K. Sharp; 339 pages; $8.00; The Williams & Wilkins Co.

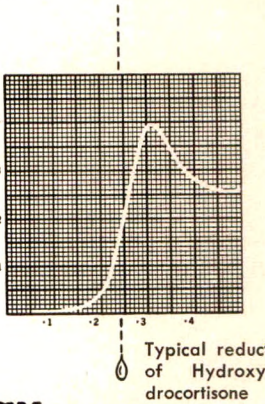
Magnetohydrodynamics (Proceedings of the Fourth Biennial Gas Dynamics Symposium), edited by A. B. Cambel *et al.;* 393 pages; $15.00; Northwestern University Press.

Scientific Photography (Proceedings of the International Colloquium, Liège 1959), edited by H. Sauvenier; 617 pages; $30.00; Pergamon Press.

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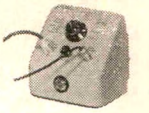
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The Evolution of Man by G. H. R. VON KOENIGSWALD; 148 pages; $1.95 paperback, $5.00 cloth; The University of Michigan Press.

Regeneration (Symposium of the Society for the Study of Development and Growth, Williams College 1961), edited by D. RUDNICK; 272 pages; $9.00; The Ronald Press Co.

Introduction to Semantics by A. SCHAFF (translated by O. WOJTASIEWICZ); 395 pages; $7.50; Pergamon Press.

Quantum Theory of Scattering by T. WU and T. OHMURA; 495 pages; $12.00 text edition, $16.00 trade edition; Prentice Hall.

The Structure of Scientific Revolutions by T. S. KUHN; 172 pages; $4.00; The University of Chicago Press.

Advances in Space Science and Technology, Vol. 4, edited by F. I. ORDWAY, III; 431 pages; $14.00; Academic Press.

Rare Earth Research (Proceedings of the Second Conference on Rare Earth Research, Glenwood Springs, September 1961), edited by J. F. NACHMAN and C. E. LUNDIN; 354 pages; $14.50; Gordon and Breach.

Data Acquisition and Processing in Biology and Medicine (Proceedings of the 1961 Rochester Conference), edited by K. ENSLEIN; 191 pages; $7.50; Pergamon Press (Macmillan Co.).

The Mosquitoes of the South Pacific (Diptera, Culicidae), *Vols. 1* and *2* by J. N. BELKIN, illustrations by C. L. HOGUE; *Vol. 1,* 1698 pages, *Vol. 2,* 412 pages; no price given; University of California Press.

Biometeorology (Proceedings of the Second International Bioclimatological Congress, London, September 1960), edited by S. W. TROMP; 687 pages; $25.00; Pergamon Press.

Advances in Catalysis (and Related Subjects), *Vol. 13,* edited by D. D. ELEY *et al.;* 458 pages; $15.00; Academic Press.

Music, Acoustics & Architecture by L. L. BERANEK; 586 pages; $17.50; John Wiley & Sons.

Clays and Clay Minerals (Proceedings of the Ninth National Conference on Clays and Clay Minerals, Purdue University), edited by A. SWINEFORD; 614 pages; $15.00; Pergamon Press.

Recent Developments in General Relativity, editors not given; 472 pages; $8.00 Pergamon Press.

Symposium on Adhesives for Structural Applications (Dover, N.J., September 1961), edited by M. J. BODNAR; 118 pages; $6.25; John Wiley & Sons (Interscience Division).

Relativity in Illustrations by J. T. SCHWARTZ; 117 pages; $5.00; New York University Press.

Activation and Behavior by E. DUFFY; 384 pages; $7.95; John Wiley & Sons.

The Cholera Years (The United States in 1832, 1849, and 1866) by C. E. ROSENBERG; 257 pages; $5.95; The University of Chicago Press.

Table of Sines and Cosines to Ten Decimal Places at Thousandths of a Degree by H. E. SALZER and N. LEVINE; no pagination; $10.00; Pergamon Press.

Introduction to Linear Systems Analysis by R. G. BROWN and J. W. NILSSON; 403 pages; $10.50; John Wiley & Sons.

Schools & Scholarship (The Christian Idea of Education: Part 2) (A seminar at Kent School 1960), edited by E. FULLER; 345 pages; $6.50; Yale University Press.

Physical Sciences (Some Recent Advances in France and the United States) (Proceedings of Symposium held at New York University 1960), edited by H. P. KALLMANN *et al.;* 243 pages; $7.50; New York University Press.

Introduction to Electron Microscopy by S. WISCHNITZER; 132 pages; $6.50; Pergamon Press.

Meteorites by B. MASON; 274 pages; $7.95; John Wiley & Sons.

Kill and Overkill (The Strategy of Annihilation) by R. E. LAPP; 197 pages; $4.95; Basic Books.

Rivers in Harness (The Story of Dams) by A. H. CULLEN; 175 pages; $3.95; Chilton Books.

Deoxyribonucleic Acid (Structure, Synthesis and Function) (Proceeding of the 11th Annual Reunion of the Société de Chimie Physique, June 1961), editors not given; 235 pages; $10.00; Pergamon Press.

Physics (For Students of Science and Engineering) by D. HALLIDAY and R. RESNICK, Part 1, First Edition and Part 2, Second Edition; 1177 pages; $11.50; John Wiley & Sons.

Techniques Variationnelles en Radioélectricité by L. CAIRO and T. KAHAN; 148 pages; 15 NF; Dunod, 92, rue Bonaparte, Paris 6, France.

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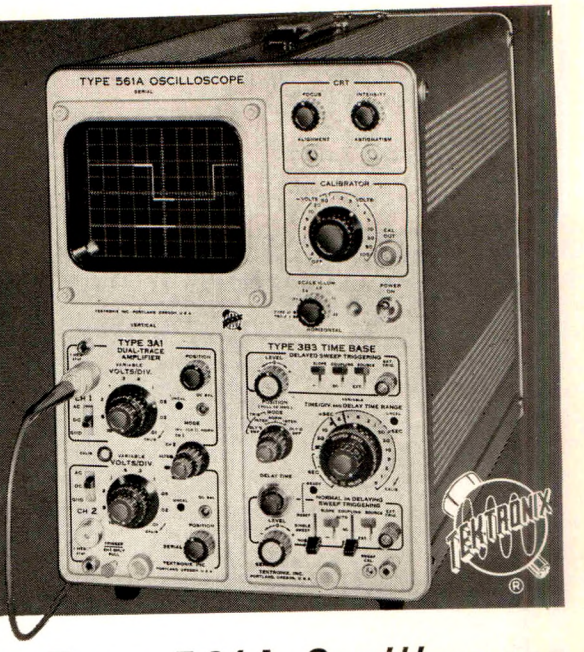
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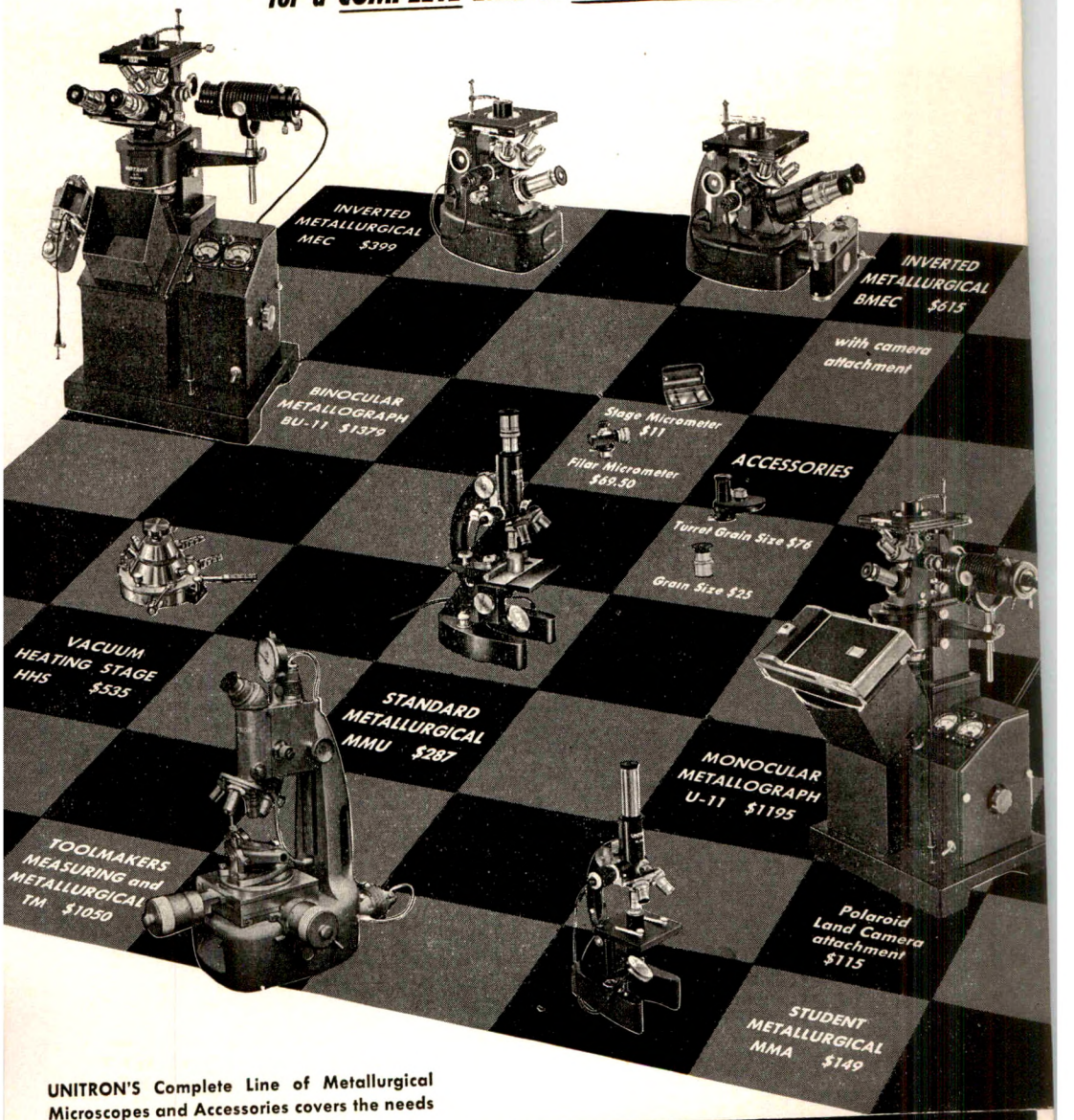
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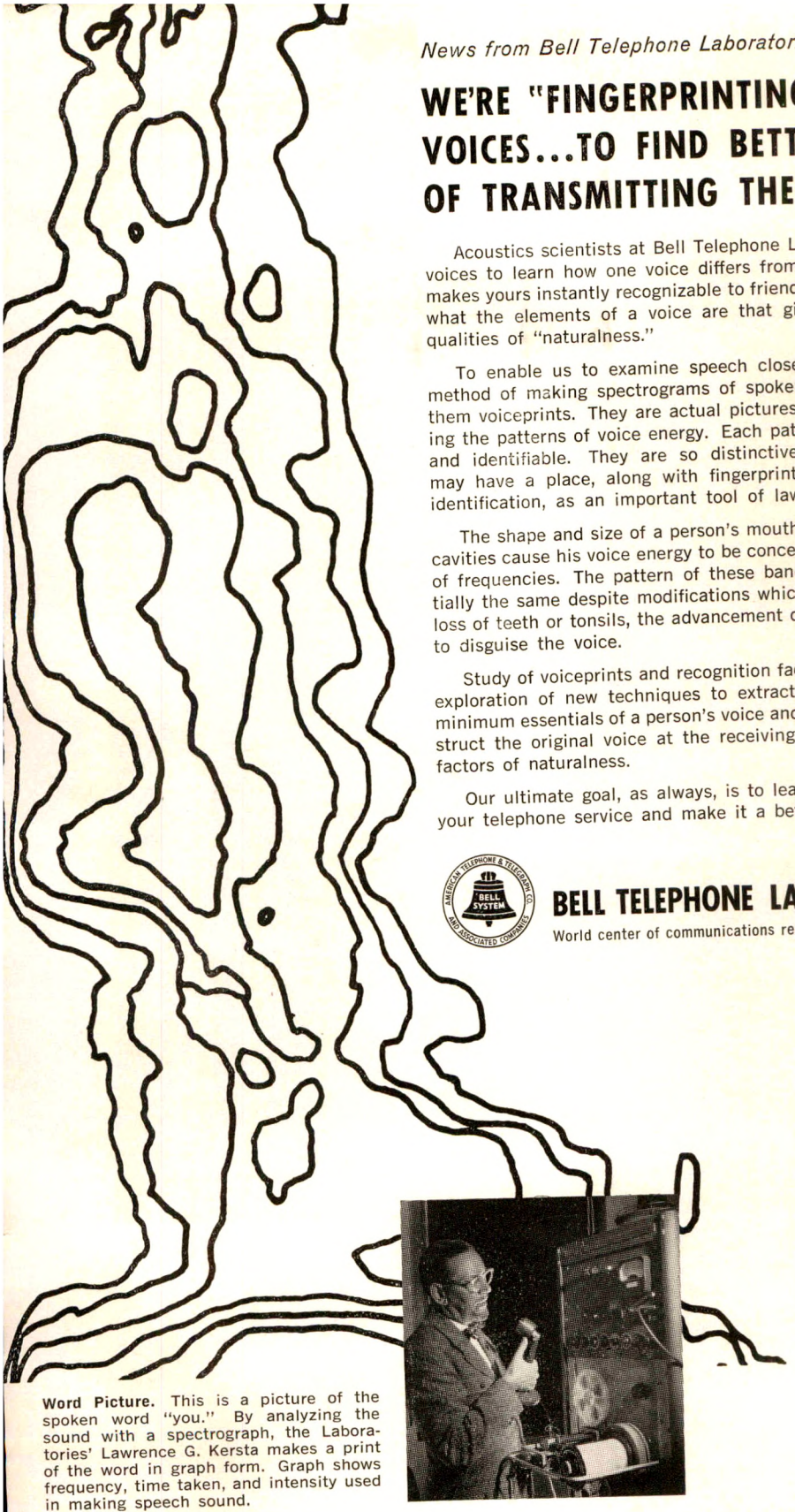
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AMERICAN SCIENTIST

WINTER · DECEMBER 1962



MESSAGE FROM THE INCOMING PRESIDENT THE SOCIETY OF THE SIGMA XI 1963–1964

To the Members of the Society of the Sigma Xi everywhere I extend sincere personal greetings as a fellow scientist. To have the distinction of being elected President of Sigma Xi is a great privilege and a high honor which I deeply appreciate.

Our predecessors have set very high standards of performance in the service of our Society, in building it up to an active membership of 80,000, distributed among 140 chapters, 123 clubs, and the membership-at-large. We must strive to do even better. Our target will be to solidify and improve the gains which have been made, and, at the same time, to be alert for new ways in which our Society can fruitfully serve science and scientists throughout the world in this new age. With the help and cooperation of all those associated with the Society, the national officers and members of the Executive Committee, the chairmen and members of our national committees, the officers and members of our local chapters and clubs, the members-at-large, and our associates in RESA, we will exert every effort to further the aims of our Society, to help develop Sigma Xi as a paramount image in the field of science, and to help create that climate in the universities of our country which will make association with Sigma Xi something most desired and much sought after.

We must strengthen and enlarge the good works and programs which our Society has developed over the years. First and foremost is our Journal, the AMERICAN SCIENTIST, which has become one of the leading scientific journals of the world. Second is our program of national Sigma

Xi-RESA Lectures, which are making an important contribution to the scientific culture of our country. Third is our program of Grants-in-Aid for Research, which are bringing much needed assistance to many small-scale research investigations in our colleges and universities. Our fellow-members who are the prime movers in these enterprises deserve great credit and much commendation. We need to provide more encouragement and increased fiscal support for all three of these programs, to keep pace with the growing needs of science and the increased importance of Sigma Xi. Our modest increase in dues of one dollar per year beginning in 1963 will make it possible for us to provide adequately for this needed additional fiscal support.

Finally, I express the hope that every Member of Sigma Xi will shoulder whatever responsibility is appropriate to his professional status in providing leadership in science and advice and guidance in the affairs of our human society, so that the fruits of our scientific work may be distributed equitably in a way that will help all the peoples of the world.

FREDERICK D. ROSSINI

RIVERS

By LUNA B. LEOPOLD

RIVERS ARE both the means and the routes by which the products of continental weathering are carried to the oceans of the world. Except in the most arid areas more water falls as precipitation than is lost by evaporation and transpiration from the land surface to the atmosphere. Thus there is an excess of water, which must flow to the ocean. Rivers, then, are the routes by which this excess water flows to the ultimate base level. The excess of precipitation over evaporation and transpiration provides the flow of rivers and springs, recharges groundwater storage, and is the supply from which man draws water for his needs.

The excess of precipitation over evapo-transpiration losses to the atmosphere is a surprisingly small percentage of the average precipitation. The average amount of water that falls as precipitation over the United States annually is 30 inches. Of this total fall, 21 inches are returned to the atmosphere in the form of water vapor through processes of evaporation and transpiration from plants. The balance of 9 inches represents that excess which contributes to the flow of rivers.

Interestingly, about 40 per cent of the runoff from continental United States is carried by the Mississippi River alone. Deep seepage from ground water to the ocean is not known, but it is believed to be quite small, probably much less than 0.1 inch per year.

It will be noted that for the land area of the continent the water cycle balances: credit, 30 inches of precipitation; debit, 9 inches of runoff plus 21 inches transferred to the atmosphere. However, looking at the atmosphere the budget appears out of balance because 30 inches are delivered to the land as rain and snow but only 21 inches as vapor are received back by evaporation and transpiration. This means that 9 inches of moisture are transported from the oceans to the continent to balance the discharge of rivers to the sea. It is estimated that each year the atmosphere brings about 150 inches from the oceans over the land area of the United States and carries back 141 inches.

A good deal of the water which appears as river flow is not transmitted into the river channels immediately after falling as precipitation. A large percentage of the fall is infiltrated into the ground and flows underground to the river channels. This provides, then, a form of storage or regulation that sustains the flow of streams during non-storm or dry periods. When one observes water flowing in a creek or river during periods of bright sunny weather it is obvious that this discharge represents water that had fallen during previous storm periods and had been stored in the rocks and in the soil materials of the drainage basin.

The 9 inches of average annual runoff of rivers of the United States amounts to a very large amount of water. If it could be visualized as a uniform flow during the whole year, which in reality it is not, it would amount to 1.4×10^{11} gallons per day. In fact, runoff is highly erratic in time and in geographic distribution. For large areas in the United States an important percentage of the total flow occurs during the late spring and early summer, the period during which the water which had been accumulating during the winter as snow appears as melt-water runoff.

Though the amount of water carried off the continent to the oceans each year is large, so also is the total length of stream channels. The writer has estimated that the total length of river channel in the United States, including all the minor creeks and draws, amounts to about 3,000,000 miles. Table 1 shows the estimated gross length of channels of various sizes in the United States, as well as the number of such channels and their average lengths.

TABLE 1.

NUMBER AND LENGTH OF RIVER CHANNELS OF VARIOUS SIZES IN THE UNITED STATES

Excluding Tributaries of Smaller Order

Order[1]	*Number*	*Average Length, Miles*	*Total Length, Miles*	*Mean Drainage Area, Including Tributaries, Sq. Mi.*	*River Representative of Each Size*
1[2]	1,570,000	1	1,570,000	1	
2	350,000	2.3	810,000	4.7	
3	80,000	5.3	420,000	23	
4	18,000	12	220,000	109	
5	4,200	28	116,000	518	
6	950	64	61,000	2,460	
7	200	147	30,000	11,700	Allegheny R.
8	41	338	14,000	55,600	Gila R.
9	8	777	6,200	264,000	Columbia R.
10	1	1,800	1,800	1,250,000	Mississippi R.

[1] The definition is that of Strahler: Order 1 is channel without tributaries; order 2 is channel with only order 1 tributaries, but includes only the length segment between junction upstream of order 1 channels and junction downstream with another order 2 channel.

[2] The size of the order 1 channel depends on scale of maps used; these order numbers are based on the determination of the smallest order using maps of scale 1:62,500.

The table presents estimates based on geometric relations among river system factors. The figures shown are derived from samples extrapolated to cover the whole United States and account for about 97 per cent of the continental area of the country.

Strangely, most Americans probably have never heard of many of the

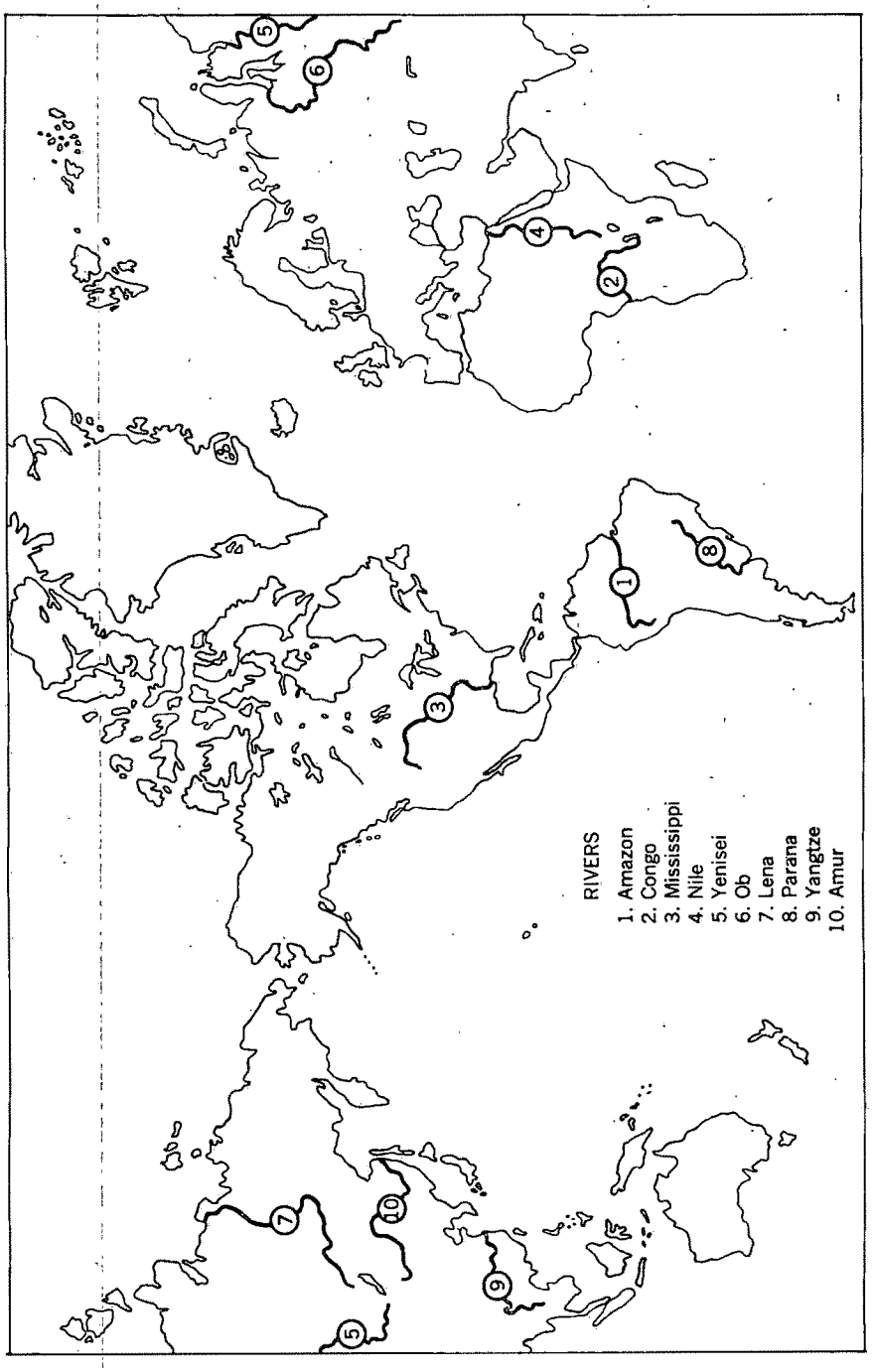


FIG. 1. Outline map of the world showing general location of the ten largest rivers.

great rivers of the world. Table 2 presents a listing of the largest rivers in each of the continents, though Europe is separated from Asia for purpose of clarity. The ten rivers draining the largest areas are indicated by numerals in the left-hand column, these numerals representing rank in terms of drainage area. This list of ten includes five that are seldom mentioned, the Yenisei, Ob, Lena, Amur, and Parana rivers. Four of these are in the Soviet Union, and indeed that country has more major rivers than the United States of America, China, and Brazil combined. The general location of these ten largest rivers is shown on the world map, Figure 1.

There is much one could write about the flow carried by rivers: the seasonal regimen, the occurrence and movement of flood waves, the

TABLE 2

GREAT RIVERS OF THE WORLD[1]

Rank in Order of Drainage Area	*River*	*Country*	*Drainage Area, Thousands of Sq Mi*	*Average Discharge at Mouth, Thousands of cfs*
		Asia		
5	Yenisei	U.S.S.R.	1,000	614
6	Ob	U.S.S.R.	959	441
7	Lena	U.S.S.R.	936	547
9	Yangtze	China	750	770
10	Amur	U.S.S.R.	712	338
	Ganges	India	409	660
	Bramaputra	E. Pakistan	361	700
	Indus	W. Pakistan	358	196
	Mekong	Thailand	310	390
	Hwang Ho (Yellow)	China	260	116
	Kolyma	U.S.S.R.	249	134
	Shatt-el-Arab[2]	Iraq	209	51
	Irrawaddy	Burma	166	479
	Indigirka	U.S.S.R.	139	64
	Krishna	India	119	69
	Godavari	India	115	127
	Salween	Burma	108	53
	Yana	U.S.S.R.	95	35
	Pyasina	U.S.S.R.	74	90
	Sankai (Si)	China	46	127
		Europe		
	Danube	Romania	315	218
	Dnieper	U.S.S.R.	194	59
	Dvina (Northern)	U.S.S.R.	139	124
	Pechora	U.S.S.R.	126	144
	Neva	U.S.S.R.	109	92
	Vistula	Poland	76	38
	Rhine	Netherlands	56	78
	Rhone	France	37	59
	Po	Italy	27	51

chemistry of river waters, and a host of other subjects that make river hydrology a valid realm for the pursuit of fundamental scientific inquiry. I shall limit this paper to a description of the form of rivers and river systems.

The three million miles of stream channels in the United States vary widely in size and occur in a wide variety of topographic and geologic circumstances. Included in this figure of total length are those high mountain streams epitomizing wilderness beauty, the dirty and trash-filled channels too often coursing through our cities, and the majestic but turbid large rivers flowing in wide valleys of central United States and the flat expanses of the coastal plain.

One might suppose then, from the variety of geographic and geologic environments through which these channels are carved, that there would exist such a variety of river types that but little could be said about river characteristics in general. Each geographic situation might be supposed to have its own special kind of river and each would be a law unto itself.

		Africa		
2	Congo	Congo	1,550	1,400
4	Nile	Egypt	1,150	100
	Zambezi	Mozambique	500	250
	Niger	Nigeria	430	215
		North America		
3	Mississippi[3]	U.S.A.	1,244	611
	Mackenzie	Canada	697	280
	St. Lawrence	U.S.A. and Canada	498	500
	Nelson	Canada	414	80
	Yukon	Canada	360	180
	Columbia	U.S.A.	258	256
	Frazer	Canada	92	113
	Mobile	U.S.A.	42	58
	Susquehanna	U.S.A.	28	38
		South America		
1	Amazon	Brazil	2,231	[4]
8	Parana	Argentina	890	526
	Tocantins	Brazil	350	360
	Orinoco	Venezuela	340	600
	Sao Francisco	Brazil	260	100
	Magdalena	Columbia	93	265
	Uruguay	Argentina and Uruguay	90	136

Note.—This table includes only rivers that discharge over 35,000 cubic feet per second at the mouth. It does not include many rivers like the Colorado for example, that drain large areas in an arid climate and therefore discharge relatively little water.

[1] Rivers with average discharge of 35,000 cubic feet per second, or greater; from information collected as part of study on world-wide runoff of dissolved solids.

[2] Tigris, Euphrates, and Karun Rivers.

[3] Includes Atchafalaya River.

[4] 3 million to 4 million cubic feet per second.

As a matter of fact, the subtle but pervasive unity that exists, despite the tremendous diversity, is probably the most important characteristic of river systems.

There is not only unity displayed by important similarities between rivers in different settings, but also an amazing organization of river systems. This in part results from a delicate balance between the forces of erosion and the forces of resistance.

The characteristics of river systems which can be used to demonstrate the unity among forms fall under three general categories, the river channel, the river valley, and the drainage net—the term applied to the system of branching channels ramifying to the smallest tributaries.

The first and most important aspect of the river channel is that it is self-formed and self-maintained. The flowing water carves the groove in which it flows. The water fashions the depth, the cross section, the areal configuration, and the longitudinal profile.

Existing river channels have had a history in many respects analogous to the history of a species in the biological world. An existing river is inherited from an earlier one, which in turn was derived from a still earlier progenitor. The sequence extends backward to that time when the continental area now being drained by the river was once under the ocean.

Thus the river channel usually cannot be spoken of as having been formed, in that no particular time or place of origin of a given river can be specified. Rather, it is best to think that a river has evolved through geologic time, and that the processes of change that may be observed at present are indeed the same processes by which this evolution took place. The changes during the river history in geologic time were brought about by changes in the external conditions, such as a change in climate (including glaciation), a change in elevation of the continental mass and the concomitant changes in vegetation, in rates and types of weathering, and in the amounts and kinds of materials flowing under the influence of gravity toward base-level—the ocean.

The forces involved in shaping and maintaining the channel are related to the fluid flow. The flow of the fluid exerts a shear on both bed and bank and it is this shear which causes particles comprising the bed and bank to be moved along in traction by the water, that is, by pushing, rolling, and skipping. This portion of the debris in transport is referred to as the bedload. The same shear creates the turbulent eddies in the flowing fluid which entrains some particles of matter and throws them up into the main mass of the fluid, thus carrying them as the suspended load.

The transport of sediment debris by rivers to the oceans is a phenomenon known to everyone. It is far less well known that the quantity of dissolved materials carried by rivers to the ocean is also very large. Because the transport process is not visually evident, nor are deposits of

dissolved materials obvious, it is hard to visualize that slightly more than half of all the materials carried by river water from continent to ocean is carried as dissolved load. This fact was quantitatively verified in results recently published by Durum, Heidel, and Tison, describing an international project designed to sample the dissolved load of the great rivers of the world.

Fluid shear shapes the river channel. It can be visualized in a general way that if a channel were very deep and very narrow there would be far more shearing action on the channel sides than on the channel bed. It seems logical that, as a result, there would be a tendency in such a channel for erosion to take place along the banks, whereas along the bed there would be a tendency for insufficient shear to carry along the eroded material. Such a channel would widen and become shallower.

An opposite extreme can be visualized, in which a channel were very wide and shallow. The great expanse of bed exposed to the shearing action of the flow would tend to erode and thus degrade the bed, establishing in time a channel of more modest breadth but greater depth. Indeed, some such action does take place, for the width-depth ratios of rivers of comparable size have a great consistency.

If the shearing force on the channel banks is sufficient to overcome the cohesion of the bank materials, erosion takes place, and the eroded particles are swept away from their original position and become a part of the bed materials, there either to be moved or temporarily lodged. On the stream bed, scour takes place when the shear exceeds some critical value, and this occurs during relatively high flow. At low flow it is usual for the shearing forces on the bed to be sufficiently small that the bed materials do not move. Scour, then, occurs primarily during high discharges. The relatively long time periods represented by modest and low flow are periods of but little movement of bed material.

A river increases downstream in size owing to the increase of drainage area as tributaries enter. This increase in size is manifested not only in the amount of water (discharge) but also by an increase in channel width and depth. To accommodate an increase in discharge resulting from tributary entrance, the channel may enlarge in width or in depth, or the increased flow could be achieved by an increase in flow velocity. Because discharge (cubic feet per second) is the product of cross-sectional area times velocity

$$Q = av$$

and area is the width times depth

$$Q = wdv$$

an increase in discharge may be accommodated by an increase in any of these three factors, or by some combination of them.

In actuality, the increase is nearly always divided among these three parameters in the same way. Figure 2 presents data for a number of sample river systems for which there are plotted against discharge increasing downstream, values of mean depth, mean width, and mean

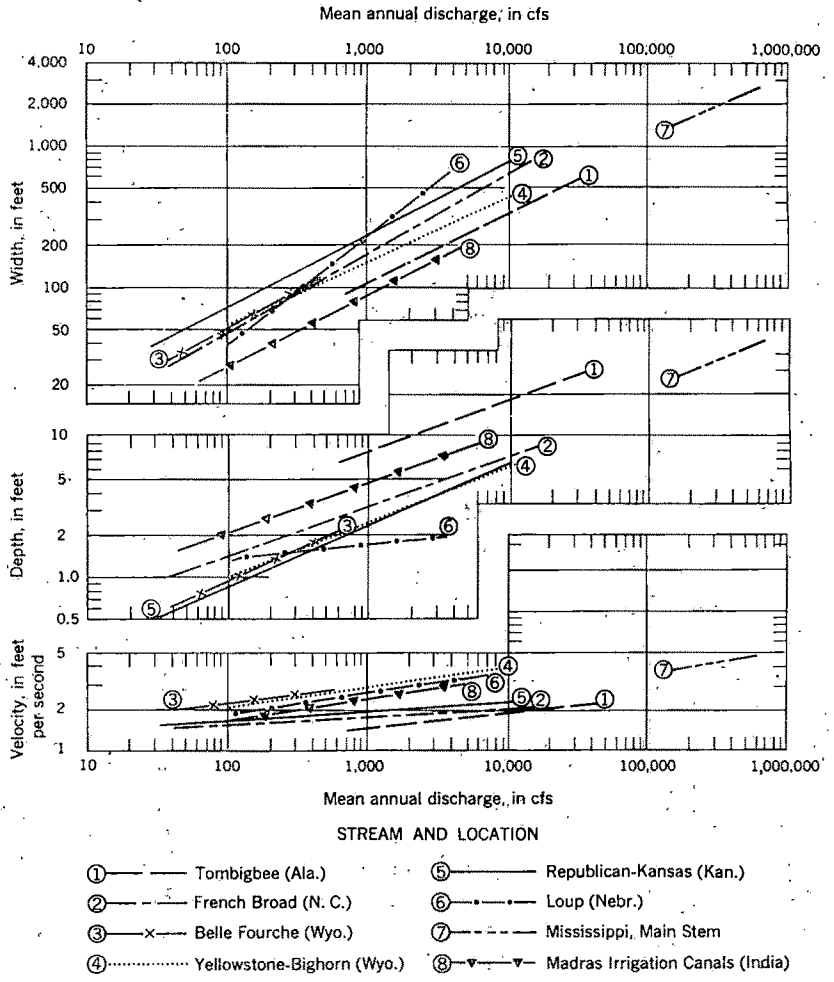


FIG. 2. Width, depth, and velocity in relation to mean annual discharge, as discharge increases downstream in various river systems.

velocity. The parallelism of lines in each of the plots shows that the rates of increase with discharge are similar among the various rivers.

Width increases downstream faster than depth, and depth faster than velocity, the latter being very nearly constant along the river length. The increase in width-to-depth ratio can be seen in Figure 3, in which cross sections of five channels are drawn with no exaggeration in

vertical scale but at different scales so that the widths appear equal on the printed page. In actuality the creek represented in the upper sketch has a width of only 24 feet whereas the river in the lower sketch is a large river having a width of 1153 feet.

It had long been supposed that the speed of water in a river decreased downstream. This supposition was based on the fact that river slope decreases downstream and, generally speaking, the size of bed material also decreases from boulders or cobbles in the headwaters to sand, silt, or clay in the downstream portions of large rivers. It had long been argued, without reference to measurement data, that the supposed downstream decrease in water velocity was causally related to the inability of a river to carry coarse material at the flat gradient existing in downstream

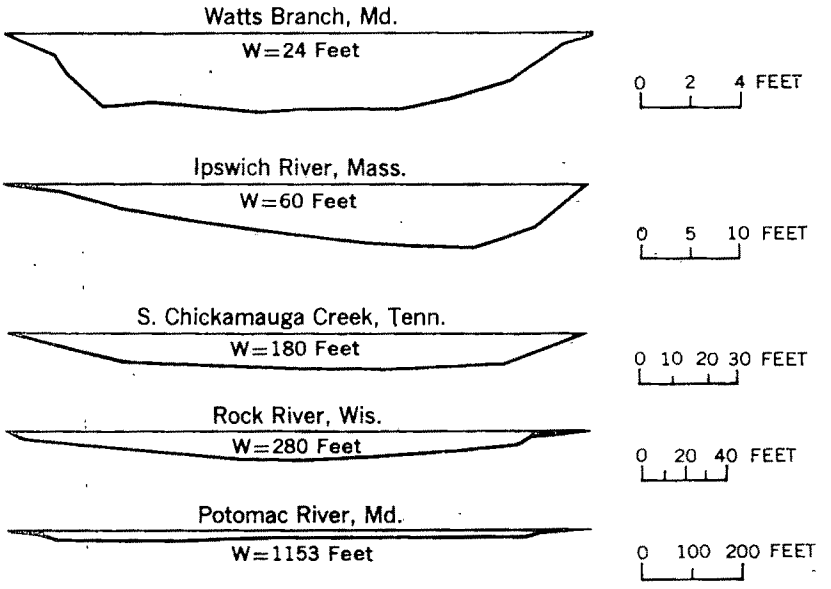


FIG. 3. Cross sections of five rivers of different size; no vertical exaggeration, but scaled so that width is equal on the printed page.

reaches. Mountain creeks appear to flow more swiftly than the deep water of large rivers, but the appearance of flowing water is a test rather of kineticity than of flow speed. When my colleagues and I showed by measurement data that, in fact, velocity tends to increase or remain constant downstream in most rivers, a new rationale was needed to explain the observed relationships.

It now appears that the constancy or slight increase of velocity downstream results from the fact that river depth increases faster than river slope decreases. But this still does not explain how the requisite changes are divided among the pertinent parameters, nor why this division is so

nearly the same in all rivers regardless of size or physiographic setting. This division turns out to be that which most nearly causes entropy to increase equally in each unit of length along the river as will be mentioned later.

That river channels should display such unity as exists is even more surprising as one inquires into the many changes that occur between low flow and high or flood flow. Channel scour during high discharge is an example. Though not universal, it is usual for the bed of a river channel to erode or temporarily to scour during floods, and as the flood recedes, to redeposit an approximately equal amount of material. The depth of bed scour may in some channels be as much as half the value of the increase in the elevation of the water surface. Thus if the water surface elevation increases, say 10 feet, above the level at low flow, the water depth at that high stage may well be increased not merely 10 feet but 15 feet. Figure 4 is an example. The passage of the high flow during the spring snow melt of 1956 past the measurement station on the Colorado River at Lees Ferry resulted at peak flow in an increase in water surface elevation of 8.2 feet but a concomitant scour of 4.8 feet. The bed elevation at the end of the high flow was within 0.5 foot of that existing before the event.

This return to a condition so nearly identical to that existing before the passage of the high flow implies a tendency for the maintenance, on the average, of a condition of dynamic equilibrium of an extremely sensitive nature. Scour tends to be balanced by fill on the average. Such a dynamic equilibrium between erosion and deposition depends on the operation of checks and balances the elucidation of which is essential to the understanding of river behavior.

The basic mechanics of scour and fill, as exemplified in Figure 4, are still only imperfectly understood. One of the most important concepts, recently developed by Bagnold, may elucidate the point.

Many rivers flow over deep deposits of relatively uniform material. Recognizing that the fluid shear on the granular bed material tends to increase with increased discharge, a threshold shear may be postulated at which the bed material will begin to move. When this threshold is reached a layer of grains at the surface of the streambed is peeled off and put into motion downstream. Owing to the fact that there is a considerable depth of similar granular material in the bed, it might be supposed that the shear capable of peeling off the top layer of grains would be similarly capable of peeling off the next layer of identical grains, and the next, ad infinitum. In reality, this does not happen, and for any given value of shear only a finite thickness of grains is set in motion. Continued application of the same shear does not result in further erosion and, instead, a temporary quasi-equilibrium exists. The transport of grains actually changes the forces within the fluid in such a manner that continued erosion of the bed becomes impossible.

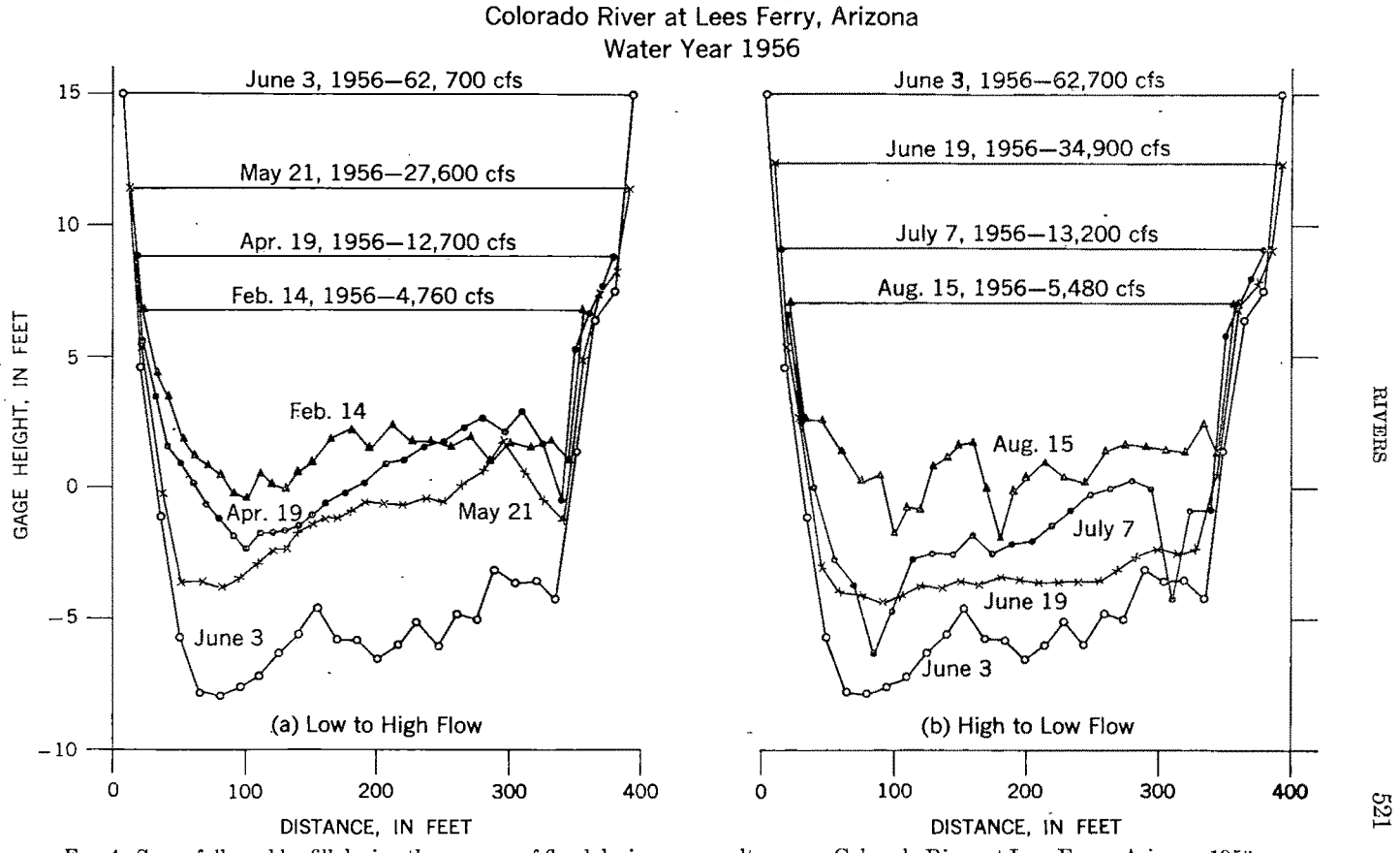


FIG. 4. Scour followed by fill during the passage of flood during snowmelt season, Colorado River at Lees Ferry, Arizona, 1956.

Bagnold's 1956 research has shown, first theoretically and then by experiment, that grains in traction create a downward-acting stress which actually tends to hold the exposed grains on the stream bed and thus prevent their erosion. This downward force results from grain collision during transport. Collisions between grains tend to knock an individual grain in a direction that may be forward, up, or down. There will be enough grains directed downward against the bed actually to create a downward stress which tends to prevent further erosion of bed grains.

Until Bagnold's work, it had not been recognized that hydraulic relations developed for clear water would not apply directly to water carrying a debris load. The forces within a fluid are altered by the transport of sediment debris.

Knowledge of sediment transport and its relation to river hydraulics has been slow to develop, both in theory and in practice, owing to the fact that it is still impossible to make consistent and accurate measurements of the amounts of debris being transported as bedload in natural rivers. Instruments have long been in use that measured adequately the sediment being transported in suspension, but bedload, being concentrated in a thin zone near the stream bottom, is interrupted by the insertion of any obstacle, no matter how streamlined. The insertion of a sampling device at the stream bed changes the conditions of transport locally in such a manner that a representative sample of the bedload in transport is impossible. Though rates of bedload transport can be successfully measured in experimental flumes, the conditions represented in the laboratory are much more limited than those extant in the field.

Water discharge in a river fluctuates widely from year to year, season to season, and day to day. Thus the forces governing erosion and deposition are highly variable. The channel flows at shallow depth most of the time and is filled to the top of the banks relatively seldom. On still less frequent occasions, the discharge exceeds the channel capacity and flows overbank. An overbank flow is a flood by definition. Recently it was shown that the frequency of the bankfull condition is surprisingly uniform among rivers in diverse settings and among rivers of widely different size. The bankfull stage may be expected on the average about once a year to once in two years.

To state this in another way so that the practical application is apparent, the river channel is large enough to accommodate all the water coming from the drainage area only in the relatively frequent event. The flat area bordering most channels—the flood plain—must flood to some extent on the average every other year. To overflow the flood plain is an inherent characteristic of a river.

It has been surmised that channel formation and maintenance are accomplished primarily during periods of flood flow. This would seem to follow from the general observation that many of the most obvious

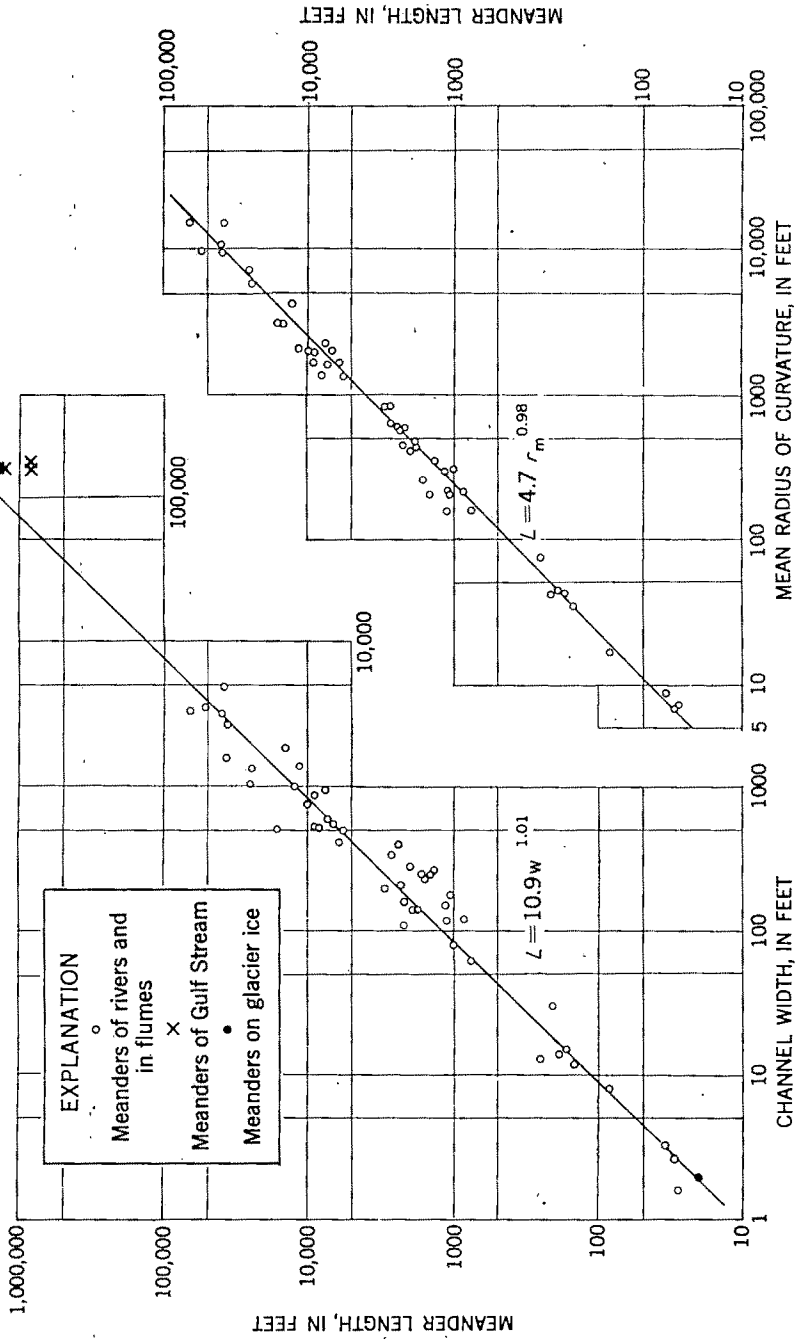


Fig. 5. Relations between meander length and channel width (left diagram) and mean radius of curvature (right diagram).

changes in form or configuration are accomplished by the catastrophic event—by the flood of large magnitude. To the extent this were true, it would be inferred that the events most worthy of study in attempting to isolate the general controlling principles would be extraordinary occurrences.

As recently as 1960, Wolman and Miller introduced another conception that has had an impact on thinking about rivers and, indeed, alters the view of the action of geomorphic processes in general. The principal work in fashioning landscape forms is not done during the frequent event or the predominant condition because the processes acting are incompetent to have any significant effect. Nor is the principal work accomplished by the very effective but very rare event—the catastrophic incident. Rather, it appears that the landscape is fashioned primarily by the event of intermediate frequency and effectiveness.

For example, the total sediment load transported by a river past a given point over a period of time is the sum of the amount transported during a variety of flow events; that is, during many days of low flow, a modest number of days of intermediate flows, and a decreasing number of increasingly great floods. The data indicate that the intermediate flows have a cumulative significance far greater than those of either greater or smaller magnitude, because the product of effectiveness times frequency is greatest. This concept is a fundamental contribution to thinking about landscape moulding processes.

There are other attributes of river channels which also demonstrate the unity that exists among channels, even channels widely different in size. Nearly all natural channels are sinuous to some extent. Indeed, natural channels are rarely straight for a distance greater than ten channel widths. Not only do channels exhibit a more or less regular aspect of sinuosity, but also the size of the curves assumed by a channel bear a constant relationship to the channel itself. Small channels wind in small curves and large channels in large curves.

Measurement data show that there is quite a constant relationship between channel width and the radius of the curves which the channel exhibits. Also, the meander length or wave length is generally proportional to channel width. These characteristics can be seen on Figure 5, where the meander length is plotted separately against channel width and against the mean radius of curvature. It can be seen that there is essentially a linear relationship between the wave length of the channel curves, channel width, and the radius of curvature. This linear relation occurs not only in most natural rivers but also to meltwater channels developed on the surface of a glacier and in the flow path of the gulf stream which is not confined by channel banks.

The consistency of the relation of radius of curvature to channel width is indicated by the following measurement data. In a sample of 50

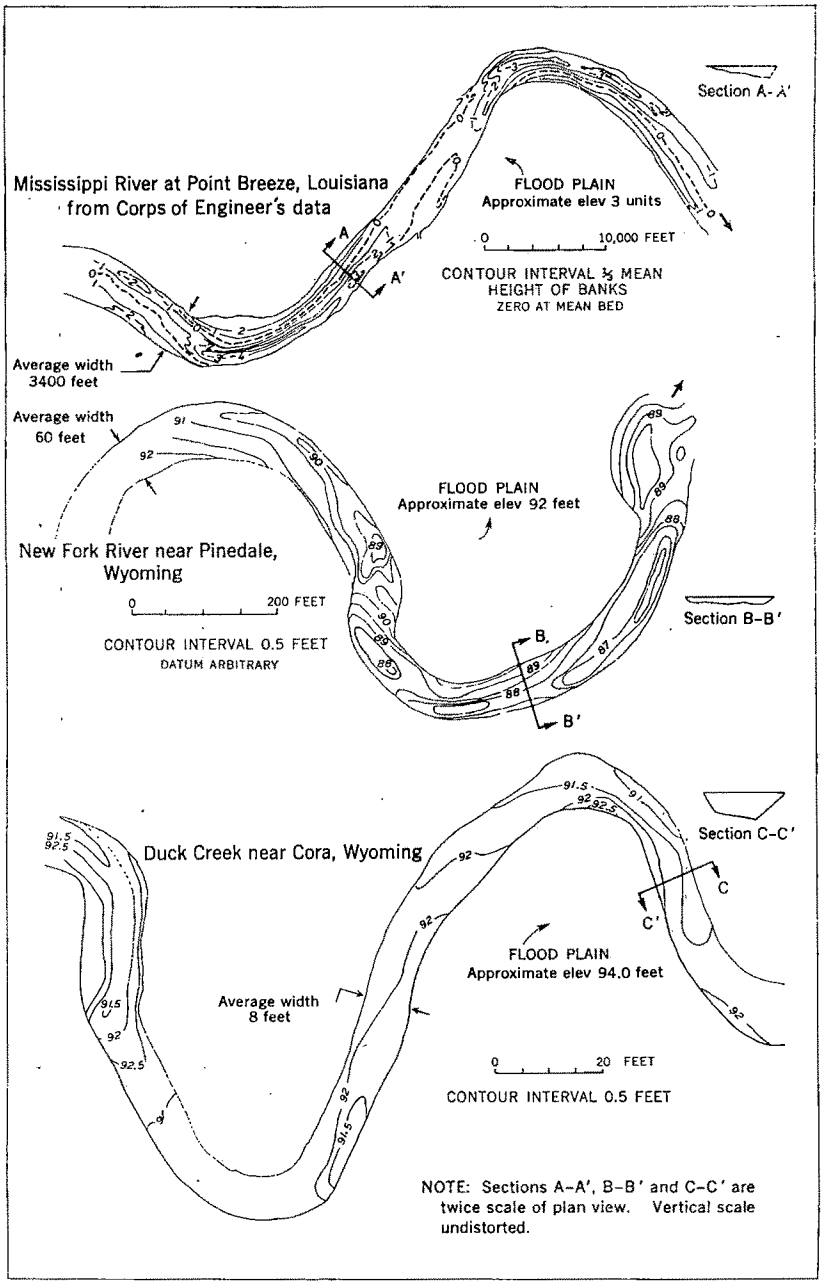


FIG. 6. Planimetric map of a meander bend on each of three rivers varying greatly in size; scaled so that meander length is equal on the printed page.

rivers of various sizes the median value of the ratio of curvature divided by width was 2.7, the mean 3.1, and two-thirds of the cases occurred between the values of 1.5 and 4.3. The tendency for a constant ratio of radius to width makes all rivers look quite similar on planimetric maps, as is suggested by Figure 6. In fact, when one inspects a planimetric map of a river without first glancing at the map scale, it is not immediately obvious whether the river is large or small owing to this tendency for a similar ratio of radius to width.

When the sinuous bends of a river are relatively symmetrical the channel is said to be meandering. Such a large variation in degree of sinuosity exists in natural channels that the definition of a meandering stream—in contrast with one which is merely sinuous—becomes a matter of arbitrary definition. There is represented in nature, then, an unbroken continuum between slightly wavy channels and well developed meanders which can assume the form of horseshoe loops. Owing to the fact that the meandering character of river channels has long been considered to be associated in basic mechanics with the movement of sediment, the observation that sediment-free channels carved by meltwater on the surface of a glacier can also meander is of considerable interest. Tentatively, the mechanical principles resulting in the meandering form of meltwater channels in the absence of sediment load and those causing the meandering of rivers are probably the same. The similarity in channel form implies that the basic cause is essentially hydrodynamic, rather than a direct result of bank erosion and the lateral transfer of sediment across the channel.

The mechanics of a meandering channel are by no means well understood. Certain physical characteristics have been known for a long time but the basic mechanism is still elusive. Measurements made in meandering streams and in curved flumes allow the construction of a generalized picture of the flow pattern in a meander which is presented in simplified form in Figure 7. The isometric view of the two principal components of velocity at various positions in the bend show the main features. The scale of the sketch is such that the super-elevation of the water surface in the bend does not show in the diagrams but is implied in the velocity distribution.

The velocity in a meander crossover, or point of inflection, is not symmetrically distributed. As would be expected, proceeding downstream from the axis of the bend the thread of maximum velocity is much closer to the concave bank than to the center of the channel. The high velocity, moreover, continues to hug this side through the point of inflection of the curve. At least in narrow channels, a cross-channel velocity component is directed toward the convex bank (usually called the point bar) near the bed and toward the concave bank near the surface.

Continuity requires, then, that surface water plunge downward near

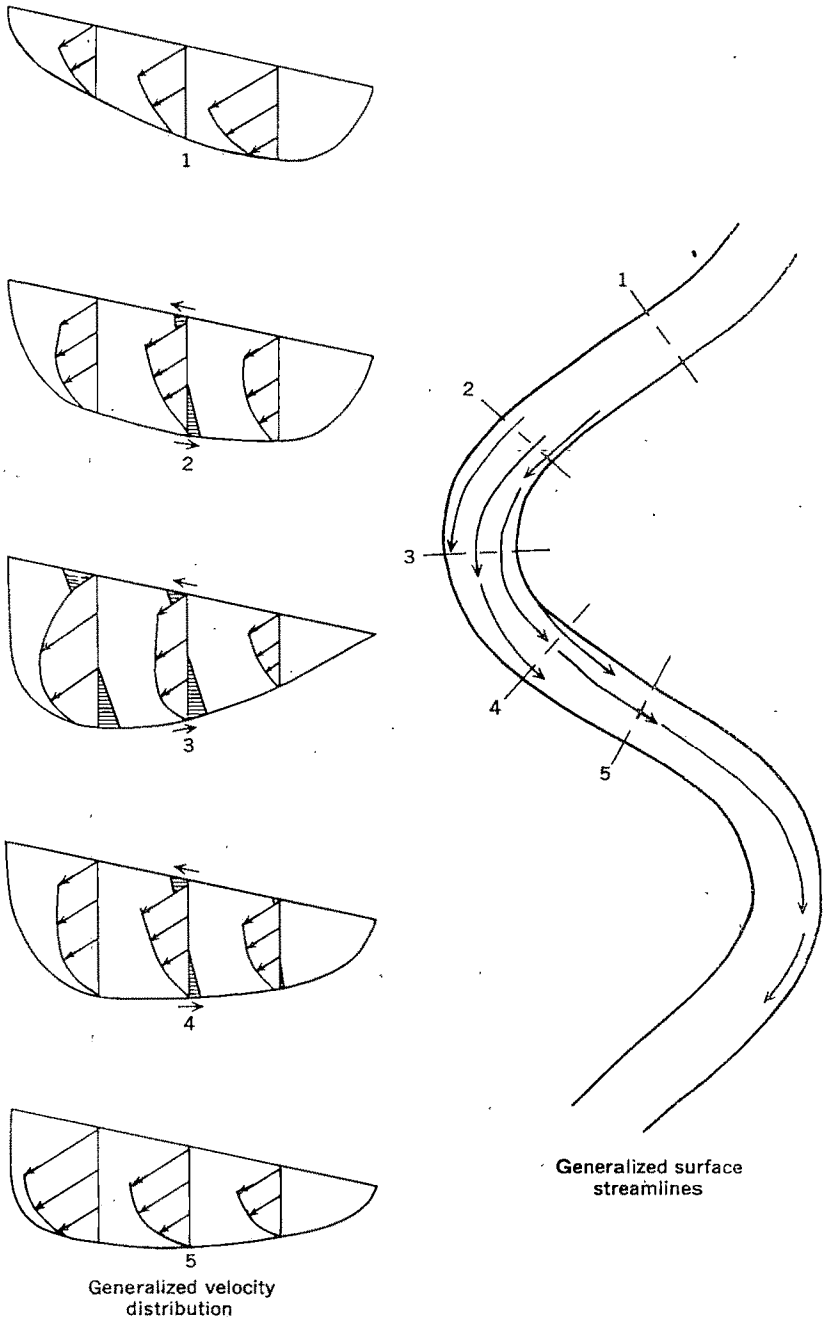


FIG. 7. Isometric view of generalized diagram of flow distribution in a meander; open parabolas with arrows indicate downstream velocity vectors; lateral component of velocity shown by closely lined areas; all sections viewed from a changing position to the left of and above the individual section.

the concave bank and that some bed water emerge at the surface near the convex bank. This circulatory motion in the cross-sectional plane of a channel was first observed and explained by Thomson in 1879. It results from the larger centrifugal force on fast-moving surface parcels than on slower-moving ones near the bed. The motion gives to an individual water parcel a path resembling a helix. As a result of this circulatory motion, material at the bed tends to be swept toward the convex bank, thus to be deposited in part on the point bar.

The accretion of material on the point bar gradually pushes the convex bank into the channel and this tendency is compensated by a tendency for erosion of the concave bank. Thus, it is usual for a river channel gradually to migrate laterally across the valley floor. During such lateral migration the channel width remains the same. An example of the building of the point bar balanced by bank erosion on the opposite side of the stream is demonstrated by a series of observations made on Watts Branch, a small stream near Washington, D.C., for which the cross sections during a period of nine years are presented in Figure 8.

The manner in which a channel moves across the valley floor, eroding one bank and building a nearly flat flood plain on the other, all the while maintaining a cross section similar in shape and size, is another aspect of the dynamic equilibrium that appears to characterize many channel systems.

Another aspect of the unity exhibited by stream systems is exemplified in the branching network of tributaries comprising what is called the drainage net. In 1945, Robert E. Horton showed that the size of a given stream is related to the number of streams of that size, their average length, and average slope. There is a very constant quantitative relationship between these factors which can best be demonstrated by some actual data. Figure 9 is a sketch to define stream order, a measure of stream size. Horton defined order in the following manner. The smallest unbranched tributary in the headwaters is, by convention, designated order 1; a stream which has only tributaries of order 1 size is called second order; a third-order stream is one in which all tributaries are of second order or smaller, etc. Figure 10 shows that there is a logarithmic relation between stream order and length of streams of a given order and between stream order and numbers of streams. After this logarithmic relation within the drainage network was pointed out by Horton, it has been tested by many workers and found applicable, essentially without exception, to drainage basins of all sizes, including the very smallest rills and large river basins.

One of the basic problems in the study of rivers is the identification of a general law or principle which provides a rationale in physical terms for the different types of unity exhibited by river channels and the networks which they comprise. It is only too easy to speak of the results of

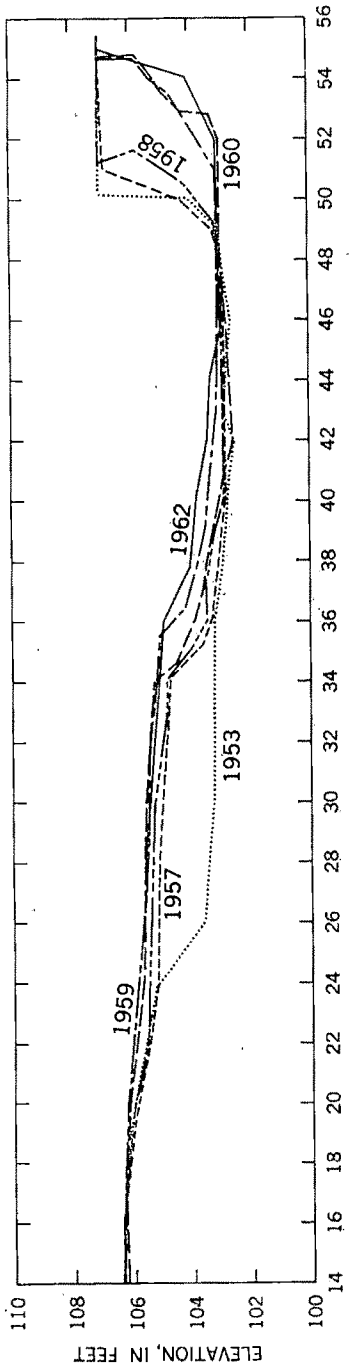


FIG. 8. Successive channel cross sections across a small channel, 1953–1962; Watts Branch near Rockville, Md.

the dynamic action of rivers in anthropomorphic terms for, indeed, rivers seem to have an inherent tendency to display many of the characteristics of an organism. There is first an organization of the various parts. All river systems appear to have basically the same type of organization, differ as they may in detail. The river system is dynamic in that it has portions that move and can cause events and create changes. We often

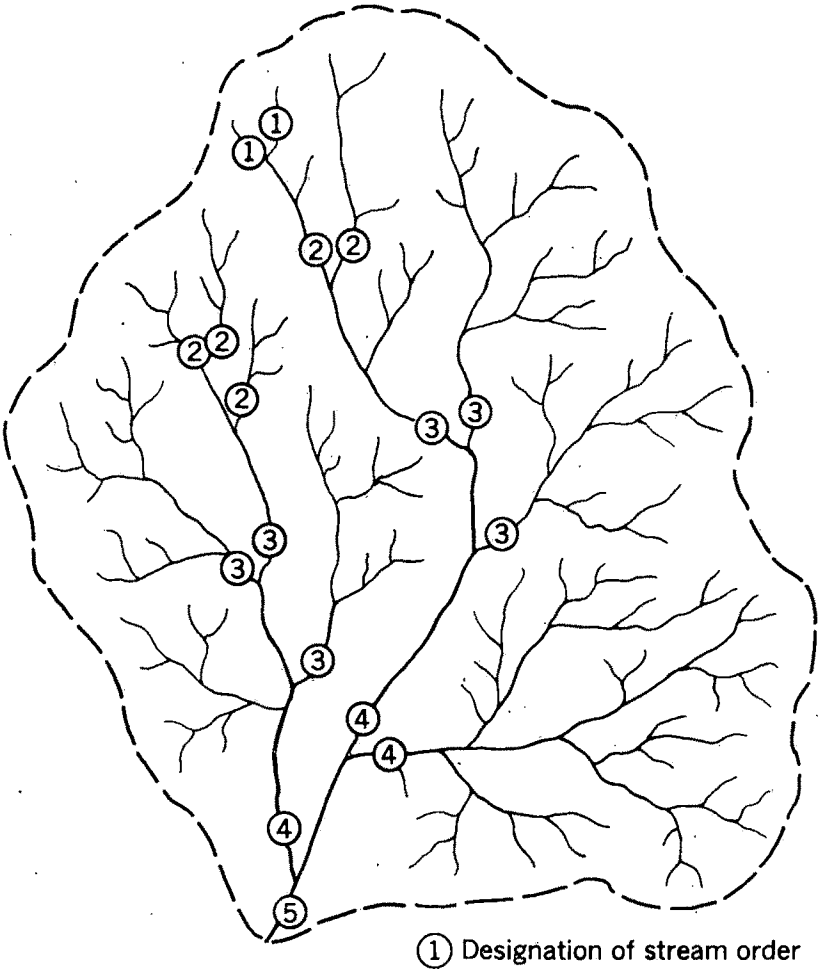


FIG. 9. A typical drainage net on which stream order, an indication of size, is shown by the numerals.

read in the popular press, as a result, that the river went on a rampage, overflowing its banks and wreaking havoc on man and his structures. Thus one tends to speak about the river as if it had a will of its own. It is interesting, then, from the philosophic standpoint, to consider some of the aspects of these dynamic characteristics that have been used to

express portions or the totality of the actions of the system which appear to have definite tendencies toward a direction of development, or tendencies which tend to maintain the average conditions observed.

One way in which this has been approached has been a description of the river system as having developmental stages through time. This was one of the basic concepts introduced by William Morris Davis, who described the geographical cycle as consisting of three principal stages comparable to the stages in the life of man. The stage of youth was

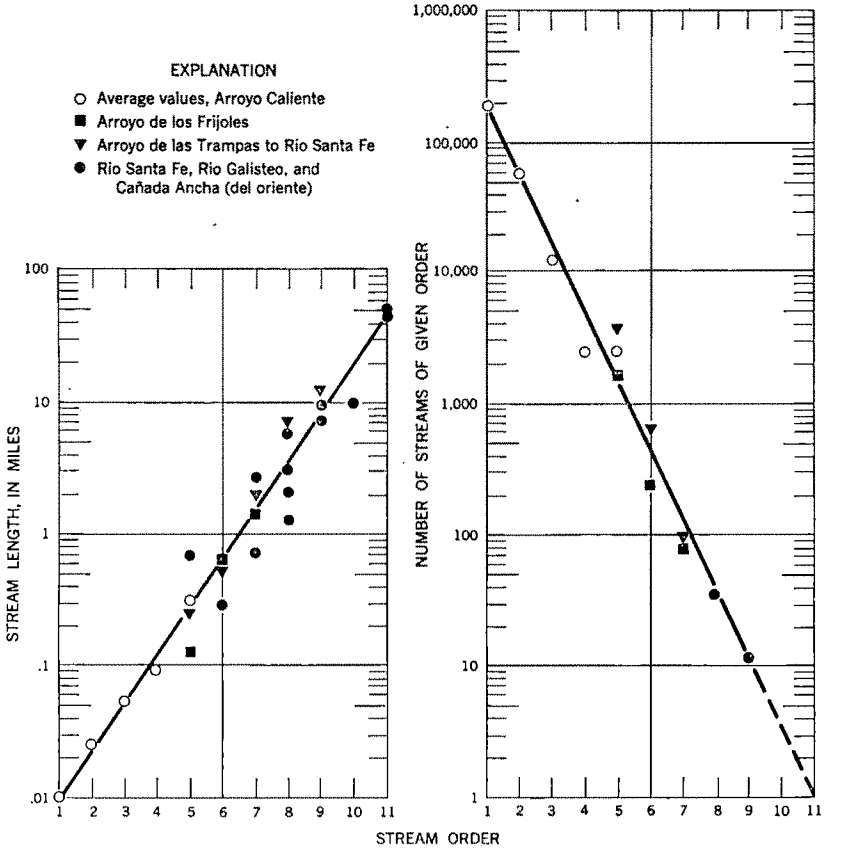


FIG. 10. Relation of stream length and number of streams to stream order in basins of 11th order near Santa Fe, New Mexico.

considered by him to follow initial uplift of a continental mass and to be characterized by deep V-shaped valleys having a tendency to downcut rapidly. The stage of maturity was described as a stage of equilibrium in which the work to be done in erosion was balanced against the capacity of the river system for doing this work. Broad valleys bordered by rounded hills characterized the stage of maturity. In the stage of old age, the river was pictured as flowing over a nearly flat plain or peneplain, and

during this relatively long period in the cycle the river was considered to be unable to do large amounts of work in erosion as a result of relatively low relief and flat gradients. Similarly, at any one point in time the headwaters of a river system could be in the stage of youth, the central portion in the stage of maturity, and the downstream portion in the stage of old age. This characterization of a river system was extremely useful for teaching purposes, but from a quantitative standpoint many aspects are difficult to particularize.

There followed, then, more detailed consideration of the concept of equilibrium, or grade, which Davis had thought of as characteristic of the stage of maturity. A graded stream was described by J. Hoover Mackin as one "in which, over a period of years, slope is delicately adjusted to provide, with available discharge and with prevailing channel characteristics, just the velocity required for the transportation of the load supplied from the drainage basin. The graded stream is a system in equilibrium; its diagnostic characteristic is that any change in any of the controlling factors will cause a displacement of the equilibrium in a direction that will tend to absorb the effect of the change (1948, p. 471)."

Wolman extended this concept and pointed out that the concept of grade had two aspects, involving adjustability and stability. In one respect, the quasi-equilibrium of a graded stream is, as Mackin pointed out, characterized primarily by its ability to adjust to changes in external conditions. Wolman believed that a concept of quasi-equilibrium may be characterized by the stability of the system, but that adjustability perhaps was no less important a criterion.

Another aspect which, under certain circumstances, had been used to describe the underlying tendency for a river toward a quasi-equilibrium condition was what might be called the principle of least work. There are certain characteristics, particularly hydraulic ones, which point to the tendency for minimization of work in a system.

Recently, Langbein and I considered the various factors governing the direction of river work and reasoned as follows. Inspecting the physical principles which recur most often in analyses of different aspects of river morphology, the basic equations which are most prominent are the equation of conservation and the equation of continuity. These laws are so obviously general that they characterize each element or reach in any fluvial system. They must also characterize each unit of any path and at each instant in time. However necessary these equations might be, they are insufficient to explain the paths of particles moving in the river systems, or the relation between one part of the path to another. Therefore, they can alone tell us nothing about the surface form of the landscape, nor can they treat completely the progressive development or change of form with time.

There must, therefore, be some other law of equal generality which

would deal with energy distributions and their relation to changes of land forms in space and time. We proposed that implications of the second law of thermodynamics satisfy these requirements.

The development of landscape involves not only the total available energy but also its distribution. The latter may be described by the concept of entropy, adapting that term from the comparable concept in thermodynamics. The essential idea is that the entropy of the system is a function of the distribution or availability of energy within the system and not a function of the total energy within it. Thus, entropy has come to concern order and disorder, an aspect of the conception which has been utilized in physical chemistry and information theory. The degree of order or disorder in a system may be described in terms of the probability or improbability of any observed state.

An increase in entropy is, then, a measure of the decrease in availability of the energy in the sense that a certain amount of energy is no longer available for conversion to mechanical work. The distribution of energy may be stated in terms of the probability of the given distribution occurring relative to alternative distributions possible.

The statistical conception of entropy is that aspect which appears to have application to geomorphic systems. The distribution of energy in a geomorphic system is one way of expressing the relative elevation of particles of water and of sediment which gradually will, in the process of landscape evolution, move downhill toward base level. The longitudinal profile of the river, for example, is a statement of the spatial distribution of stream-bed materials with regard to their elevation and, thus, with regard to their potential energy.

In thermodynamics, heat energy is referred to absolute temperature as a base. The absolute temperature defines an absolute limit or base datum, the situation in which molecular motion becomes zero. It is, then, the base level, or the datum, against which the energy content of a thermal system can be measured. Systems in geomorphology also have a base datum with regard to the distribution of energy. This base datum is the datum of elevation, in most cases represented by mean sea level. But the classical treatment of entropy in thermodynamics deals with closed systems in which entropy continuously increases to a maximum stationary level at equilibrium. In closed systems there is no loss or addition of energy.

Geomorphic processes operate, on the other hand, in open systems in which energy is being added in some places while in other places energy is being degraded to heat and is thus lost insofar as further mechanical work is concerned. A river system, then, we consider to be an example of an open system, defining the system as the water and the debris in the river channel. As the water flows down the channel it gives up potential energy which is converted first to kinetic energy of the

flowing water and which in the process of flow is dissipated into heat along the channel margins. Precipitation brings increments of energy into the system because water enters at various elevations and thus with various amounts of potential energy. Heat is lost by convection, conduction, or radiation, yet the channel may be considered in dynamic equilibrium.

The steady state possible in open systems differs from the steady state of static equilibrium of closed systems. We equate, therefore, the term steady state with dynamic equilibrium in geomorphology as defined early in the century by Gilbert and, more recently, by Hack. In an open system in dynamic equilibrium the rate of increase of entropy in the system is zero. The continuity of entropy then takes the form that the rate of outflow of entropy equals the rate of internal generation of entropy. In a river system in equilibrium, the rate of outflow of entropy is the rate of dissipation of energy as heat. This is equated to the rate of generation of entropy represented by the energy gradient toward base level. The stationary state of an open system is characterized by the conditions in which the rate of production of entropy per unit volume of flow is a minimum compatible with the conditions imposed on the system. Hence a stable system corresponds to one of least work, a conception we were able to demonstrate in mathematical terms for certain conceptual models.

The argument was developed that, with respect to the longitudinal profile of the river, the probability of the particular combination of values of energy in various unit distances along the course of the open system of the river could be described as proportional to the negative exponential of its ratio to the total energy of all possible states. This led to the conclusion that the most probable sequence of energy losses in successive units of river lengths corresponds to a uniform increase in entropy in each unit length along the river system. When this specification is fulfilled without constraint on river length the longitudinal profile of the river tends to become exponential in form, a result in agreement with many actual river profiles.

In the development of this probabilistic argument, random-walk models were constructed to demonstrate several aspects of the river system. Longitudinal profiles comparable to those of rivers may be constructed by random walks. As one example, beginning at an arbitrary point on cartesian graph paper, the walk moves from the initial point in unit steps to the right with various probabilities of a downward movement simultaneously. Such random walks may be constructed from a series of random numbers or with special packs of cards, and the results from such mechanical construction of random walks were compared with a larger number of random walks developed on a digital computer.

An example is shown in Figure 11. In this model an initial point was

set at an elevation of 5 units above the base level. In successive moves, the walk proceeds to the right one unit at a time. There are 6 levels (0, 1, 2, 3, 4, 5). The deck of cards used to draw the random numbers was so arranged that the probability of a downward step during a unit step to the right was $H/6$. Thus the probability of a downward step decreases with elevation.

On Figure 11 two sample random walks are shown marked by crosses and circles respectively. The average vertical position at any abscissa value was determined by averaging values for a large number of individual walks, and the average values define the profile drawn in a heavy line. This average profile is exponential in form, a general form exhibited by many natural stream profiles.

Certain characteristics of drainage networks were also exemplified by random walks in various types of models. Figure 12 shows a random-

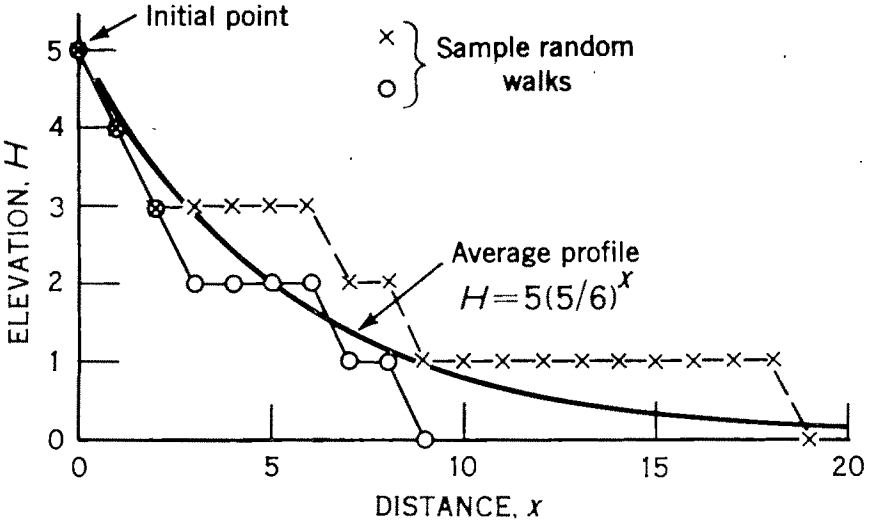


FIG. 11. Sample random walks used in the generation of an average longitudinal profile of a river.

walk drainage network. The random-walk models developed relationships between average stream lengths and stream order very similar to those presented in Figure 10.

In summary, it was shown by mathematical models, and checked by construction of average position of random walks under specified conditions, that the most probable river profiles approached a condition in which the downstream rate of production of entropy per unit mass is constant. Other demonstrations of the applicability of the entropy concept to geomorphic features were also developed.

It tentatively appears that the second law of thermodynamics pro-

vides a general rationale for some aspects of landscape evolution and for certain important landscape forms. The probabilistic aspect of entropy gives a general reason for the variation among many examples of any given landscape feature—a variability, however, which does not obscure a unity among the many slightly different examples.

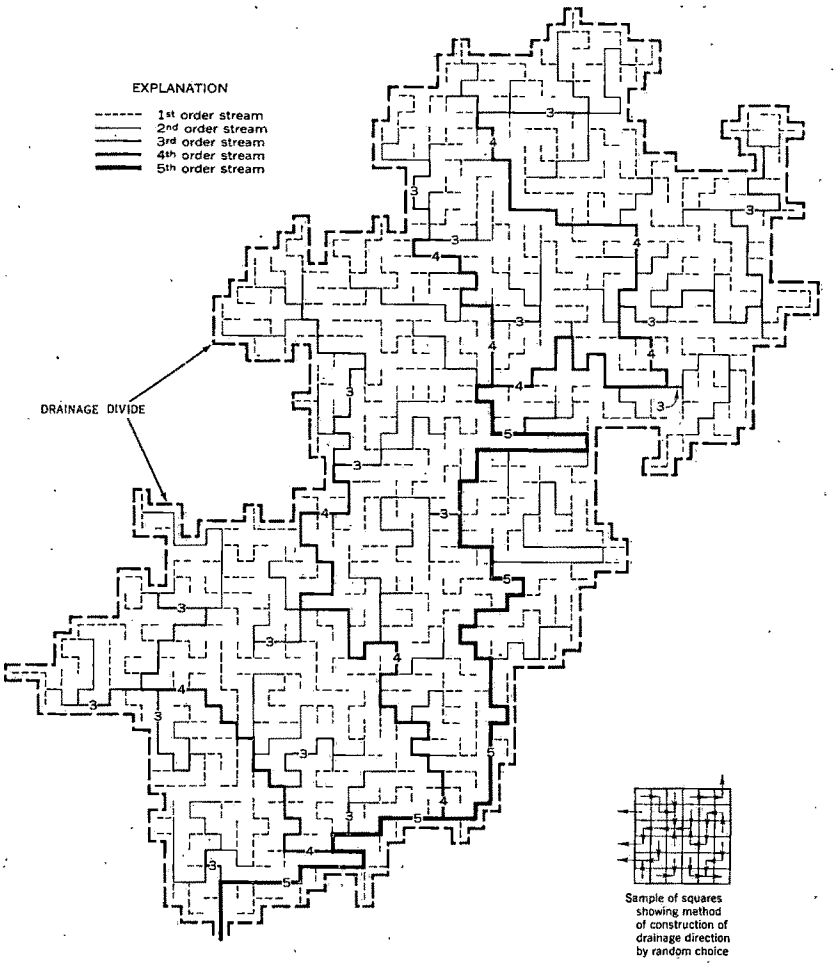


FIG. 12. Development of a random-walk drainage basin network.

In this brief résumé some of the relatively recent developments in thinking about rivers and river morphology have been mentioned. The contributions in the past decade have been centered around the mathematical analysis of river processes and of channel and drainage net characteristics. This work, contributed by a variety of scientists in different disciplines, has resulted in a quantitative description of many

of the physical characteristics of rivers and, at least to some extent, a description of some processes in basic physical terms.

The most immediate general problems still facing the geomorphologist are centered around three types of questions. The first concerns the mechanics of sediment transportation, particularly related to the movement of bedload, which at present still can not be measured satisfactorily in real rivers under field conditions. The second general area seems to center in the question of the location and mode of energy dissipation in rivers and the effect of the energy utilization on erosion and deposition. This particular area, therefore, involves the relation of hydraulic factors to soil or lithologic factors. Among the latter is the erodibility of different types of materials particularly in relation to their stratigraphy and sedimentational characteristics.

The third general area concerns the mode of transport of materials from slopes into the river channel. To a great extent, recent work in river morphology has been more concerned with river channels than with the hillslopes shedding debris toward the channels. Thus slope development, slope processes, and the relationship of slopes to the channels to which they are tributary remain an integral part of the study of rivers. These subjects encompass an area of nearly unlimited scope for research by engineers, geologists, soil scientists, botanists, physical chemists, and those possessing a variety of other skills.

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THE BEHAVIOR OF THE PSYCHOLOGIST AT A CHOICE POINT

By WILLIAM SEEMAN and PHILIP A. MARKS

THIS IS A story about a white rat who wanted to be a psychologist. His name was Herman. As Herman was at the upper end of the rat-intelligence continuum, and was, in fact, the very, very brightest member of a "bright" strain which had been developed over many generations, he was able to secure entrance to a graduate school.

One of the first things he learned (which he had, of course, known all along) was that there was a vast amount of experimental literature about the behavior of the white rat at a choice point. (To be sure, there were a few Skinnerians who occasionally had some unkind words to say about the T-maze as an instrument of experimentation, but in the main the maze was accepted as shedding light on the "dynamics" of choice behavior.) What surprised our white rat, Herman, however, was that very little knowledge was available about the behavior of the *psychologist* at a choice point. Except for a few major efforts, such as the Michigan study of clinical psychology students, there had not been much time available for the investigation of the investigative behavior of psychologists and of the choices involved therein. He did find that E. K. Strong had demonstrated quite a while back that professional choices were not likely to be wholly accidentally determined. And thinking about this it occurred to him (let us face it—our rat, Herman, had a taint of the "clinician" somewhere about him) that the psychoanalysts might be right, and that it might be true, as Roy Schafer had said, "Interests themselves must be regarded as . . . expressions . . . of major dynamic trends." So he was not in the least surprised when he found that Strong and Tucker had demonstrated that four rather distinctive profiles could be derived respectively for surgeons, internists, pathologists, and psychiatrists. He would have been surprised if it were *not* so, if these different specialists were *not* "different kinds of people."

Then, in diligent pursuit of his course assignments, Herman began to read the long "methodological" discussions which dotted the pages of the *Bulletin*, the *Review*, and, to a lesser extent, the pages of some of the other journals. In a short time he was able to distinguish between "theory" and "meta-theory," although it must be admitted that he occasionally confused these with "methodological considerations." One thing he found: psychological theorizing being what it was, it seemed terribly easy for some psychologists to yield to the temptation of telling *other* psychologists what, as theorists and/or methodologists and/or meta-theorists they *had* to do in order to remain members-in-good-standing of the "scientific" club. The rules of the game called "science"

as they pertained to the domain called "behavior" were sometimes laid out with enviable precision, neatness, and order. The more our Herman thought about this, the more puzzled he became; puzzled then worried, then haunted. He pursued an elusive idea and an elusive word—*choice*. It disrupted his chain of associations, became obsessively omnipresent in his thoughts, and punctuated his dreams in a shamelessly unsymbolic and undisguised form. It seemed to him that there were a number of instances in which important but implicit choices were being made; instances in which it was not quite explicit, always, *that* there were choices being made. For example, he happened one day upon two graduate doctoral students who were just completing experiments, one on secondary reinforcement, the other on changes in the "self-concept" with psychotherapy. The student with the secondary-reinforcement problem was taunting the other with his lack of "rigor." Either you were rigorous, said the former, or you were not. If you were, you could call yourself a "scientist"; if you were not, you couldn't.

That evening Herman worked far into the night, digging deep into the philosophy of science. He read, he thought; he thought, he read, and then he thought harder and harder. The more he read and the deeper he immersed himself in thought, the more perplexed he became. It was clear that rigor was a great idea, as it was clear, most psychologists would agree, that any and every area of psychology ought to be made as rigorous as possible, although writers on projective techniques occasionally contrived to give a contrary impression. But he wondered why, if rigor were the absolutely exclusive criterion, the student had not done an experiment in classical mechanics! What looked like the beautiful precision and control of secondary reinforcement (like "rigor," in short) began to look like sloppy lack of controls as compared with the precision and rigor of classical physics. This particular experiment, as he recalled, had been executed in the absence of any knowledge about the life history of the rat subjects before they had been purchased at the institute. And, being a white rat himself, he knew this might not be an irrelevant variable. It must be, then, (so our Herman ruminated) that there was an element of *choice* here; the psychologist took what rigor he could get and took it in something he happened to be *interested* in. To the psychologist's credit it had to be said that rigor was largely devised, evolved, invented, or discovered as a result of blood, sweat, tears, thought, and the ingenuity of his efforts. All in all, considering the psychology of the investigator, it seemed to Herman that this constituted an interesting bit of behavior at a choice point. It made him sad, though, to think of the poor thanks the psychologist sometimes got for his efforts at rigor, for he remembered the graduate student in physics who had asked scornfully: "Tell me, are there any *laws* in psychology?"

Then, too, the methodological papers puzzled him in another way. He

seemed to recall that somewhere MacFarlane had written that the centralist doctrine which was given substantial meaning by the work of Freud had somehow got distorted (in the projective techniques) into "Your unconscious is you." He wondered whether, comparably, the doctrine of operationism hadn't got distorted into "seeing is believing." Thus, in a discussion of intervening variables *versus* hypothetical constructs, he was informed that while one was permitted (if he *must*) to resort to hypothetical constructs, temporarily, he had nevertheless better cash these in at the earliest market opportunity for intervening variables. What looked like a methodological *must* to one psychologist seemed to Herman on closer examination, to be a doctrinal preference. If he were a clinician (which thank heavens, he was not!) he might even have said that this was an *interest* choice being smuggled in (quite unconsciously) as formal methodological doctrine. Such a methodological dictum, if adopted, would make it impossible to suggest that things might not, in fact, be what they seem, and that skimmed milk could conceivably masquerade as cream. It would, for example, make it impossible to say that observed behavior such as a persistent and hyperkinetic pursuit of women by a man could be a "defensive reaction-formation," a means of defending against "unconscious homoerotic impulses." He had read somewhere that Meehl had referred to this as sometimes giving the appearance of having a heads-I-win-tails-you-lose flavor. But here, too, there was a choice. Some psychologists might be made uncomfortable by the mere mention of such a possibility, and had every right to avoid it and to work in another theatre of operations. Those who elected to work this area would just have to be content with a little less rigor, less elegance, less precision, and perhaps a good deal more discouragement.

One October morn our Herman walked into his statistics course and sat down. The instructor was a well-known statistician with a widely-respected book in the field. He began the lecture (the instructor, that is) by announcing that he had spent the summer reading all the psychology journal articles he could (including the experimental journals) and these had depressed him. Of the whole lot, there were only two satisfactory papers; just two papers in which the researchers had been careful to see that all the assumptions underlying the statistical procedures used were actually met by the data. The statistician then proceeded to present to the class as an illustrative instance a study which *did* meet the requirements. It was a study in tossing pennies. Now Herman had no objection to tossing pennies, and he certainly felt that people who enjoyed tossing pennies ought to be permitted to do so. (He had himself once made a brief but disastrous visit to Las Vegas and the roulette tables.) But as a white rat he had (he felt) a certain natural interest in the *human organism*. He thought that he should be permitted to choose

between a precise estimate of something he was not the least interested in and a less precise estimate of something he passionately wanted to know.

Herman was made saddest of all by the constant guerilla warfare carried on by the "clinical" and "experimental" students. He thought the clinicians sometimes had a tendency to fuzziness about the "clinical method," "dynamics," and "clinical validation." He especially wondered what the latter might mean, and he counted seven different definitions of the word "dynamic." (On the other hand he had just read Gibson's seven different definitions of the word "stimulus.") But, thinking again in terms of choice and in the psychology of the investigator, it occurred to him that some of the more solid clinicians were making much the same *kind* of choice when they elected to research "countertransference," "identification," and "self-concept" in preference to secondary reinforce-

1.	MATHEMATICS	STATISTICS
2.	H-CONSTRUCT	I-VARIABLE
3.	ARISTOTLE	GALILEO
4.	S-O-R LAWS	S-R LAWS
5.	RORSCHACH	MMPI
6.	FACTOR ANALYSIS	PSYCHOANALYSIS
7.	PSYCHOPHYSICS	BEHAVIORISTICS
8.	PREDICTION	EXPLANATION
9.	S-R LAWS	R-R LAWS
10.	LOGIC	MEASUREMENT
11.	CONSTRUCT VALIDITY	CONCURRENT VALIDITY
12.	HOCKHEIMER	SEDLITZ

FIG. 1. Qualifying examination.

ment, pursuit rotor tasks, and reactive inhibition—the same kind of choice as were those psychologists who elected to research secondary reinforcement, pursuit rotor tasks, and reactive inhibition in preference to the capture probability of electrons, magnetic field, and hydrodynamics; all of which latter could, beyond all shadow of doubt, be stated with more exact and elegant mathematical precision and with greater generality than the "laws" of secondary reinforcement, pursuit rotor tasks, and reactive inhibition. At least it appeared to our rat that the same logic which would compel the psychologist to turn from the disturbing messiness and lack of controls in research on "countertransference," "identification," and the "self-concept" to the more rigorous controls of secondary reinforcement, pursuit rotor tasks, and reactive inhibition should compel the psychologist to turn from the messiness and lack of controls in research on secondary reinforcement, pursuit rotor tasks, and reactive inhibition to the more elegant, more general, more precise research on capture probability, magnetic fields, and hydrodynamics.

Troubled by this unprecedented and heretical view, Herman's advisor recommended that he (Herman) schedule his doctoral qualifying exam-

ination at once. Passively Herman acquiesced. The examination, reproduced in Figure 1, was given in three parts.

The instructions for each part were simple and straightforward, and were these.

Part I: "Select the member of each pair having the highest personal *interest* value"; Part II: "Select the member of each pair having the highest '*rigor*' value"; Part III: "Select the member of each pair having the highest *theory* value" (i.e., the one that is more theoretical than empirical).

Separate answer sheets were used for each part, and in no instance was Herman informed of the precise nature of the required tasks or permitted to see the instructions until he had completed the preceding part (e.g., he had to complete Part I before he could even be informed that there was to be a Part II, etc.). Herman's Interest, Rigor, and Theory scores are given in Figure 2.

Part I: Interest		Part II: Rigor		Part III: Theory	
A	B	A	B	A	B
1. (X)	( )	1. (X)	( )	1. (X)	( )
2. ( )	(X)	2. ( )	(X)	2. (X)	( )
3. ( )	(X)	3. ( )	(X)	3. ( )	(X)
4. (X)	( )	4. ( )	(X)	4. (X)	( )
5. ( )	(X)	5. ( )	(X)	5. (X)	( )
6. (X)	( )	6. (X)	( )	6. ( )	(X)
7. ( )	(X)	7. (X)	( )	7. ( )	(X)
8. (X)	( )	8. (X)	( )	8. ( )	(X)
9. (X)	( )	9. (X)	( )	9. (X)	( )
10. (X)	( )	10. (X)	( )	10. (X)	( )
11. ( )	(X)	11. (X)	( )	11. ( )	(X)
12. (X)	( )	12. (X)	( )	12. (X)	( )

FIG. 2. Herman's interest, rigor, and theory scores.

Herman's advisor, and indeed his entire department, were extremely delighted with the results. A comparison of Herman's interests with his notions of rigor revealed a congruency of 75.0%—he was, so they reasoned, a rigorous-rat. A comparison of his interests with his notions of theory revealed a congruency of 66.7%—Herman was a theoretically-oriented-rigorous-rat. Yet, upon *Herman's* closer inspection, such results were, for him, scarcely cause for rejoicing. What saddened him was the state of affairs represented by his low rigor-theory item overlap (41.7%). He wondered how a theoretically-oriented-rigorous-rat like himself could have actually obtained a higher rigor-empirical percentage (58.3%). Was he conflicted? It occurred to him that quite possibly he was—that, being infra-human, of course he was—"I am," he mused, "a theoretically-oriented-rigorous-conflicted-rat!"

In his second year of graduate study Herman became fascinated with the problems presented by learning theory. After surveying the various theories as presented in Hilgard, he tackled reinforcement theory more

systematically, studying Hull with great care. One night he fell asleep whilst ruminating over the problem of extinction and stimulus-response asynchronism. Suddenly, he sat bolt upright, waking from a dream in which he had been pursuing Hull's idea of developing a unit of habit strength, the "hab." Why not a unit of rigor—the "rig?" Well, why not? (The *definition* of rigor was, at least implicitly, very easy. It could be found in Mendelssohn's opening sentence in the paper "Probability Enters Physics": "Physics—or, if you like, experimental philosophy—is a philosophical method which differs from others in that it relies on rigorous quantitative relations between observations.")

In a burst of creative excitement and activity he burrowed into the works of Norbert Weiner and Anatol Rapaport: into cybernetics, into information theory, into James Miller's general systems theory. Armed with this constellation of information he then set to work building a machine, a rigometer, which like an ammeter or a voltmeter, would register values directly on a dial. In this manic burst of imaginative activity, Herman hardly ate or slept; it was too exciting. He knew that if the circuits could be made to reverberate properly, if enough circuits could be provided to obviate information overload, and if the programing mathematics could be mastered, the most complex theories and experimental designs could be fed into the machine and a precise measure of their rigor obtained in "rig" units.

At last the machine was ready. Would it work? With trembling paws and much trepidation Herman fed into the machine a number of theories: Newton's gravitational theory, Maxwell's and Boltzmann's equations expressing pressure and temperature of a gas as average energy of motion of gas molecules, Schrödinger's mathematical representation of de Broglie's theory of matter waves. The indicator swept over the face of the dial registering extraordinarily high values in "rigs." So far so good! Feeding in the various theories of learning, Herman saw the indicator move downward on the dial; lower and lower "rigs," as was to be expected. As he fed in the theories of transference and countertransference the dial plummeted ever lower. Herman was delighted. His rigometer was an indubitable success!

Nothing succeeds like success, and nothing breeds success more than the proliferation of previous success. Herman had run across some of Flesch's measures of readability and across some measures of interest. Why not, though, a more *precise* measure of interest, so that various problems could be assessed for their interest value? He decided to build another machine, an intometer, based upon certain statistical facts: The listed interests of the APA membership; the rated interest value of certain questions (e.g., How long does it take an ameba to locate food in a T-maze? How can we devise methods to prevent mutual annihilation by the hydrogen bomb? etc.); the nature and extent of subscriptions to pro-

fessional journals; and a number of other variables. Only after he had completed this piece of apparatus did it occur to Herman to investigate the relationship between the rigor value (in "rigs") and the interest value (in "ints") of certain questions. What he found disturbed him not a little, and is presented in Figure 3.

He found that if he plotted on the abscissa the interest values of some of the problems randomly sampled from the universe of problems, and along the ordinate the rigor values of the tightest experimental designs which could be devised to study those problems—he found that an

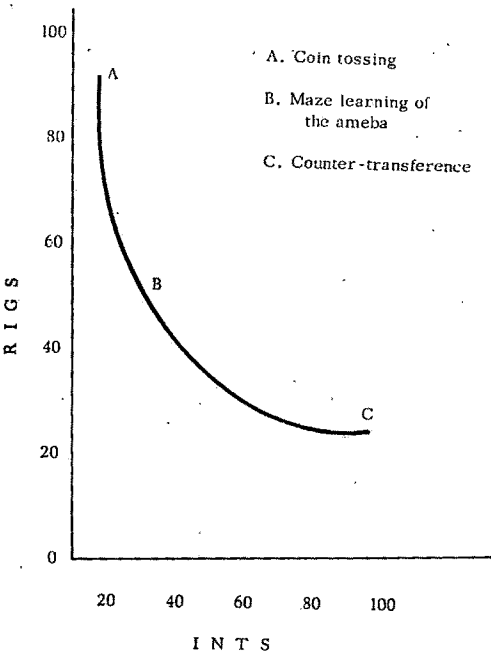


FIG. 3. Relationship between rigor values (RIGS) and interest values (INTS) for a random sample of problems.

inverse relationship turned up. One could say either that high rigor problems had low interest values or that high interest problems had low rigor values as measured by the instrumentation. With this Herman was not at all happy.

But degree-getting is not a process which waits on the personal happiness of the student, human or infra-human. The dissertation hurdle had now to be cleared. Herman selected as his problem "the prediction of daydreams in a gravitational field" and set up the problem in the following manner: He first surveyed a number of college samples to determine the incidence of certain daydreams types, and established

with astonishingly high reliability (with rank-order correlations of the magnitude of .95) the incidence of these daydreams types under twenty-five separate conditions. This was a purely empirical study, involving nothing more than frequency counts of daydreams. The dissertation problem proper was designed to answer the question: What kinds of theories could maximally predict these twenty-five daydream frequencies? To answer this question Herman selected five physicists, five chemists, five zoologists, five physiologists, five learning theorists and five clinical

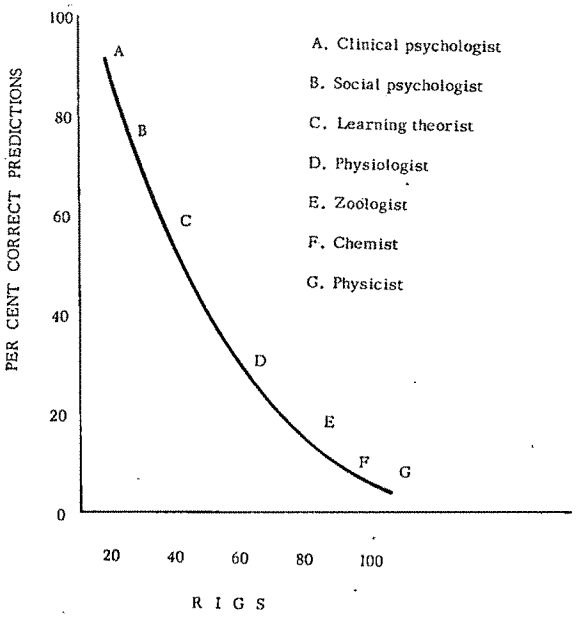


FIG. 4. Relationship between rigor values (RIGS) and per cent of correct predictions of daydreams for a selected number of theories (correct predictions for all 25 daydreams would yield 100%; for 24 daydreams 96%, etc.).

psychologists, and asked each of these to select any available theory in his chosen domain. These theories were then fed into the rigometer and their mean value in "rigs" thus determined. The next step was to require each selected theory to deduce logically (i.e., predict) the daydream outcomes, and the total number of "hits" (successful predictions) was computed for the theories in the chosen domains described above. The results of the experimental procedure are presented in Figure 4.

Herman's committee confronted these experimental results with dismayed incredulity, and were thrown into pandemonium. (Notable exceptions to this reaction were to be observed in the committee member representing the philosophy of science and the honorary committee member from physics, both of whom inspected the curve representing

the results with imperturbable equanimity.) Having examined the experimental results with minute and corrosive care and being unable to locate any errors in the design, procedure, and data analysis, the committee members concluded that Herman's thesis was not quite cricket. It was simply inadmissible, on logical-theoretical-methodological-meta-theoretical grounds, that any theory of lower rig value could

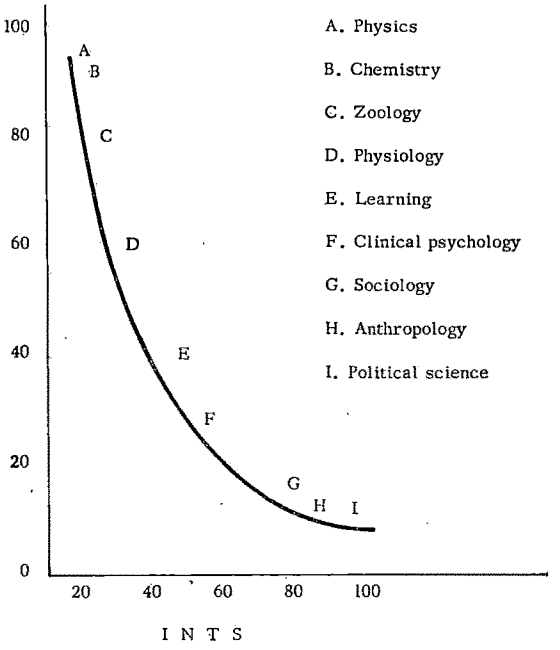


FIG. 5. Relationship between interest values (INTS) and rigor values (RIGS) as a function of departmental problems.

effect more successful predictions in *any* scientific domain. Surely, to take any other position was to throw a wrench into the machinery of science.

However heretical the doctrine presented by our theoretically-oriented rigorous-conflicted-rat, that doctrine, he maintained, had its validity credentials firmly anchored to the experimental results of his thesis. His committee remained unimpressed and unconvinced.

Herman then confronted them with yet another piece of experimental evidence. This was a little problem he had worked out just for the fun of it, at the time he had done his major survey of daydreams. At that time he had asked the chairman of each of the following departments in the university graduate school to suggest a problem of current interest in his

own domain. Indicated here are the departments and the problems set forth:

A. Physics (The capture probability of electrons)
B. Chemistry (The molecular structure of crystals)
C. Zoology (Tonic labrinthine reflexes of the crawdad)
D. Physiology (Muscle action potentials)
E. Learning (Gradient of secondary reinforcement)
F. Clinical psychology (Outcomes of psychotherapy)
G. Sociology (Delinquency prediction and control)
H. Anthropology (Cultural evolution of mankind)
I. Political science (Avoidance of mutual annihilation by the H-bomb)

The rig values attached to the methods of inquiry designed to investigate these questions are given in Figure 5.

Herman then challenged the committee to draw a line dividing the "scientific" from the "non-scientific" disciplines. (Naturally, to avoid contamination with respect to inter-rater agreement he politely requested them to do this independently.) The committee quite naturally refused to comply with such a highhanded request on the part of a graduate student, especially when he was infra-human. Herman then inquired whether the people at position A should expel from the scientific brotherhood (on the grounds of low rig value) all people at position E; or whether the people at position E should (on the same grounds) expel all people at position F; etc. The committee stared stonily and silently first at each other and then at Herman. *They* knew right from wrong, doctrinal sin from doctrinal truth. They politely but firmly suggested that such heretical doctrine (and such experimental results) could not be embraced under the rubric of "science." Equally firmly, they suggested Herman might be in the wrong school. Moreover, they left him no choice!

REVOLUTION FOR BIOLOGY*

By TALBOT H. WATERMAN

BIOLOGY is approaching an important crossroad. From one direction we traditional zoologists and botanists travel a well-worn path which steadily becomes less rewarding as well as more repetitious because we have largely failed to be both rigorous and intellectually creative. As a result our work has been branded by its paucity of quantitative data and lack of conceptual strength.

From another direction the new biologists, biophysicists, biostatisticians, molecular biologists, biomathematicians, and systems analysts approach along a different path with its origins in mathematics, physics, chemistry, and engineering which have themselves often been elegantly rigorous and conceptually powerful. But despite its surface glitter and areas of brilliant achievement, the work of this second group is often crippled by its ignorance or even scorn of the detailed facts about cells, organisms and populations as well as of their complex integrations in space and time. Both avenues for approaching the study of life may alone fail to reach their goal of broad scientific understanding.

Yet there are some signs that an intersection of these different approaches to biology need not inevitably be followed by further divergence. Indeed if the traditionalists could learn to become more mathematical and theoretical, and if the new school could become more biological, an effective collaboration with potentially great revolutionary consequences seems quite possible.

Naturally a multitude of approaches may be valid in biology, and scientists of many temperaments can be fruitfully engaged in this field. Yet the deliberate cultivation of adequate quantitative analytical techniques now available combined with the newer, powerful, synthetic, deductive methods modeled after those of the physical sciences seems particularly promising for the future. May not the dissatisfaction among the traditional biologists [19,43,47] and the lively ferment of the new biology[8] effect a breakthrough to a new level of understanding?

The Nature of Biology

While some of biology's shortcomings as a quantitative highly conceptual science are due to deficiencies of biologists and their training, others clearly depend on the nature of the subject. Living organisms are multicomponent open chain systems which combine a remarkable stability with an exquisite sensitivity to change [21]. Such systems, whose

* This article is based on a lecture which was presented to the Yale Chapter of Sigma Xi and which introduced a special course on Theoretical and Mathematical Biology given by visiting lecturers at Yale with the aid of National Science Foundation Grant 21114. It will be the first chapter in a book collecting most of the lectures in this course co-edited by Dr. Harold J. Morowitz and the author.

important characteristics arise from just this presence of many strongly interdependent variables, are difficult even to describe and require a high level of sophistication to analyze effectively. Without mathematics one can hardly even begin to think about entities with more than two or three variables. Yet the internal variables of organisms along with the relevant environment parameters number in the hundreds or thousands. To select which of these must be studied and to determine how few of them are adequate to explain the system's behavior make up the biologist's central task.

Thus the initial collection of data and their mere descriptive ordering are formidable tasks even when attention is focussed on some restricted aspect of the whole system. For this reason biology has frequently become bogged down in just collecting data and has ignored or even despised the need for doing more than this. Yet in any well-developed science primary facts, or sensory data, are the mere beginnings.

Once obtained, facts must serve as the basis for developing an interconnected field of statements, propositions, and constructs which may ultimately give rise to inductive generalizations, hypotheses, and laws. Such higher order inductions to be considered valuable must meet certain metaphysical requirements which are taken for granted, such as logical fertility, extensibility, sensibility, elegance, and causality[1] [36]. Furthermore these constructs to be of any use must permit deductive predictions and subsequent empirical verification. Hence a circularity is inherent in the methodology of science since one must proceed from data to construct or model and thence back to new data or from model to data and back to model again [3]. In a well-developed science a multiplicity of such intersecting circular pathways form a coherent system of consistent relations.

It should be noted that this essential circularity of science ultimately interconnects a black box and a white box, to use Wiener's terminology [61]. The black box is the system under study, and hence in Margenau's phrase a configuration in the data plane; the white box is the model, construct or law which explains the working of the black box. Science may start empirically with either qualitative or quantitative data relating the input of a black box to its output and proceed inductively to a corresponding white box. Or it may start theoretically with a construct and proceed deductively to the data plane in order to see what validity this white box may have.

In these terms biology, to become a mature science, must have white boxes to explain its facts. Each white box, if not achieved by intuition or creative insight, must be induced from data. While simple qualitative

[1] Such causality may seem to be deterministic, as in Newtonian mechanics, but both the nature of living systems and the means of obtaining data about them may indicate probabilistic stochastic causal relationships [e.g., refs. 5,6,37,50].

information may suffice, multivariate relational and metric facts are quite likely required and may in fact be the only basis for understanding. It is in this connection that the recent emergence of appropriate statistical and electronic computer techniques has so much inherent interest and promise for biology.

The goal of an exact science is to develop a strong system of basic constructs with such depth and breadth that particular parts of the whole field can be explained deductively as logical consequences of a small number of more fundamental principles. Thus the retinal absorption of photons by rhodopsin in the primary events of vision can be explained in the more inclusive terms of photochemistry in general.

In the biological sciences hypotheses and theories fall into two major categories. In one, as in the rhodopsin case, the data are explained in terms of underlying physical and chemical laws. Thus hemodynamics may be shown subject to Poiseuille's law, the diffusion of oxygen in tracheae to Fick's law, the activity of enzymes to the laws of catalysis, or the reactions of metabolism to the laws of irreversible thermodynamics and chemical kinetics. In the other major category the explanations provided are biological and hence couched in terms of the underlying principles of organic adaptation, development, heredity, or evolution [58].

Further development and emphasis on deductive explanations are crucial if biology is to progress from its present weak state as a primarily descriptive correlational subject to that of a rigorous deductive science capable of accounting for much of its content in terms of broad elegant generalizations.

Undoubtedly the physical and chemical components of organisms are the elements of biology, but the molecular and submolecular bits and pieces become biologically meaningful only when they are put back together again in the highly organized context of the living system [43]. A clear demonstration of this was given by Anfinsen's stimulating essay at explaining molecular biological data in terms of evolution [2]. Nevertheless biophysicists and molecular biologists often seem to believe that physical and chemical explanations of life are *the* final goal of biology. One may as well say that Fourier analysis as such can provide *the* explanation of a Bartok quartet! The ultimate scientific explanation of life must indeed be biological. Both historically and functionally live organisms share a number of basic organizational and behavioral properties not known in the nonliving world [42].

The special nature of biological systems centers around their immediate adaptedness and consequent persistence. Hence living systems as well as their environments must be so constituted and organized that life can survive and does in fact do so with a premium on a minimum over-all "cost" to the organism [21]. Life on earth has presumably

persisted from its original inception at least two or three billion years ago.

A very large part of biology can be explained by hypotheses derived from concepts of adaptedness or fitness which measure contributions to survival. Depending on the time scale considered, adaptedness relates to physiology and ecology for short intervals, to genetics and development for longer periods and to evolution and phylogeny for a far-extended temporal scale. This is not to say, of course, that all the underlying relevant processes are not occurring continuously, but merely that the time intervals in which the various series of events become significant, as well as the pathways through which they act, are characteristic of the different kinds of explanation associated with these areas of biology [58].

A Possible Solution

In view of the nature of life and the current strong dichotomy among practicing biologists [14,40], prospects for the early development of an exact science of biology may seem rather problematical. Yet a solution for these difficulties may well lie in the effective fusion of the two biologies characterized above and in the conscientious application of mathematical and theoretical methodology throughout the whole discipline. Not only should biophysics and biochemistry be strengthened biologically, but also systematics, zoogeography and the study of evolution should be quantified and more explicitly conceptualized. Embryology too would benefit from the same treatment.

Such reforms if effected would frankly acknowledge the real difficulties of biology, reduce the lack of balanced training and viewpoint shown by most biologists, as well as offer real promise of improving the field and raising it to a state worthy of its importance to man. To give substance to such hopeful generalities, some particular cases will be cited in illustration.

Descriptions. Although adequate description of the many components and interconnections of biological systems is a difficult task, such basic facts clearly must be available before generalizations or hypotheses can be induced and empirically tested [41]. Several aspects of the situation contribute to the difficulty. From a metric and technical point of view, the problem of measuring a large number of the living system's dimensions is a formidable one. From a relational point of view, too, the detection and evaluation of multiple interdependencies present considerable difficulties [45,46]. While the ultimate understanding of such systems may indeed be provided by the discovery of a few simple dimensions or relations, our faith in such a parsimonious outcome surely should not be sustained only by our inability to deal effectively with complex information.

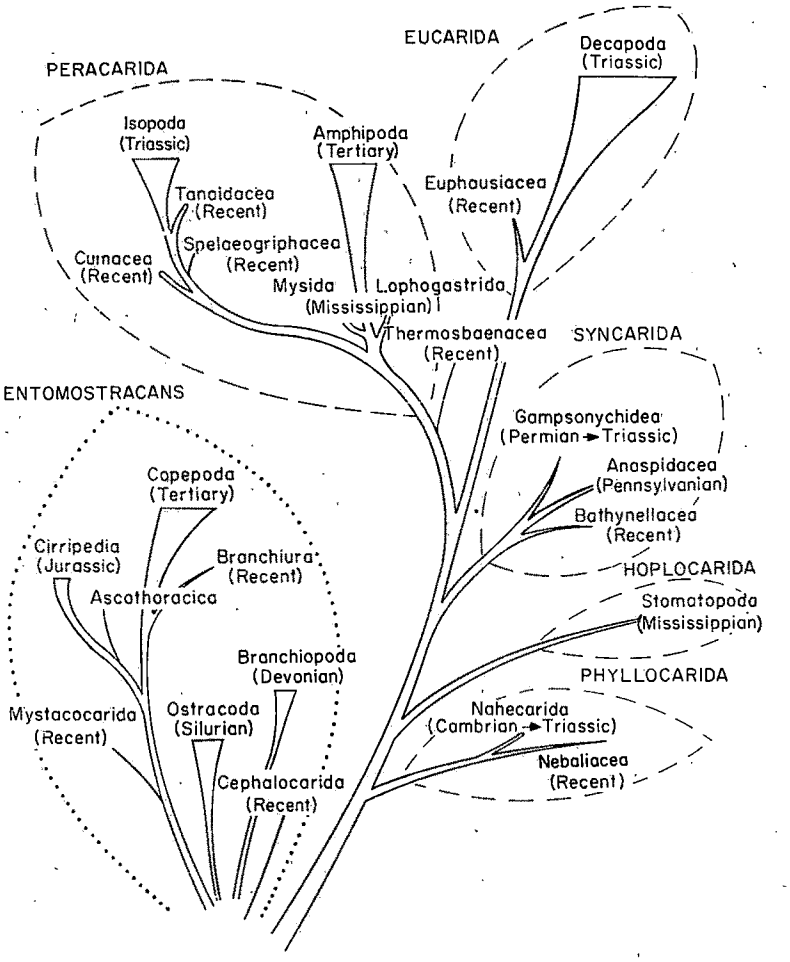


FIG. 1. Proposed phylogenetic tree for the Class Crustacea. In diagrams of this sort a large number of parameters are compressed into a two-dimensional representation. Thus distance along a branch in the direction from root to crown is related to both the evolutionary concept of transition from primitive to advanced groups and to the geological time vector. The branching pattern represents direct relationships hypothesized and the distance between two groups in the plane is inversely related to their degree of similarity and inferred relationship. In addition the approximate number of species in Recent Forms is indicated by the width of the branch they terminate [58].

One general deterrent to achieving adequate biological description lies in our own perceptual capabilities. Two-dimensional comparisons are easy on a sheet of paper, three-dimensional comparisons are quite possible but more difficult, while multidimensional representations are practically impossible on an intuitively comprehensible level.

Graphical representation. Graphs may be effectively utilized in solving

some of these problems. The basic situation is epitomized by the familiar attempt to represent phylogenies in a two- or three-dimensional illustration. Quite aside from their frequently non-quantitative, conjectural nature, family trees of groups of organisms have the difficulty that they must, by the pattern of branching at least, indicate the degrees

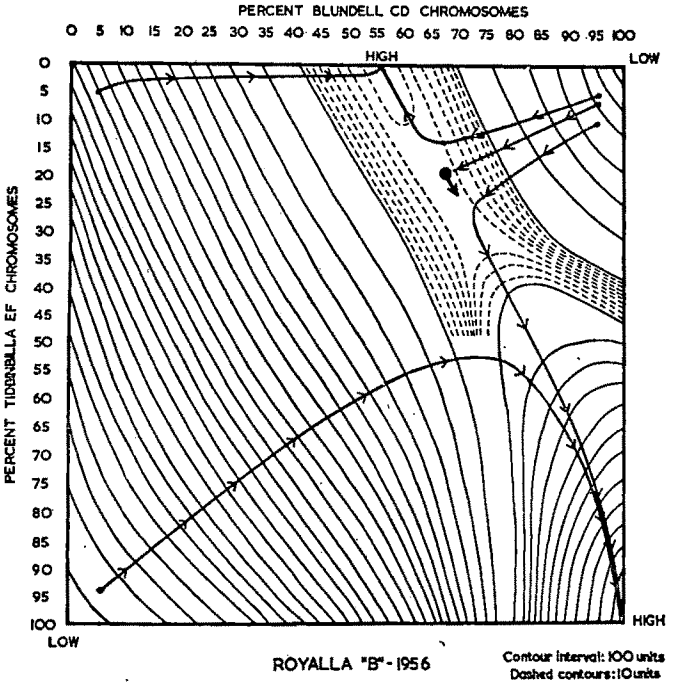


FIG. 2. Topographic chart depicting the relationship between two dimensions of genetic variation in a polymorphic population of the grasshopper *Moraba* and the calculated adaptiveness of any particular combination of chromosome inversions plotted as isoadaptive contours of a *z*-coordinate. It is postulated that genetic drift would tend to carry any given population over the resulting terrain toward peaks of maximal adaptedness as shown by the trajectories with arrows [34].

of similarity and difference between the subgroups involved [52]. Hence assigning to particular branches group names symbolic of a great number of metric and relational characters already includes a large degree of simplification. Such diagrams also frequently attempt to include a geological time dimension and perhaps some indication of the different numerical importance of the various taxa included (Fig. 1). Even when more quantitative data are explicitly utilized and more sophisticated multidimensional analysis is employed, the representation of the results remains imperfect [11,31,32,49,51].

If there are only three critical dimensions involved, quite adequate

representations can be made with topographic models in which elevated and depressed values of a z-coordinate are represented by isolines on a terrain. These may be simply three-dimensional graphs [e.g., in 35], or they may be more like landscape analogs over which components are postulated to move through passes, climb peaks or seek the lowest

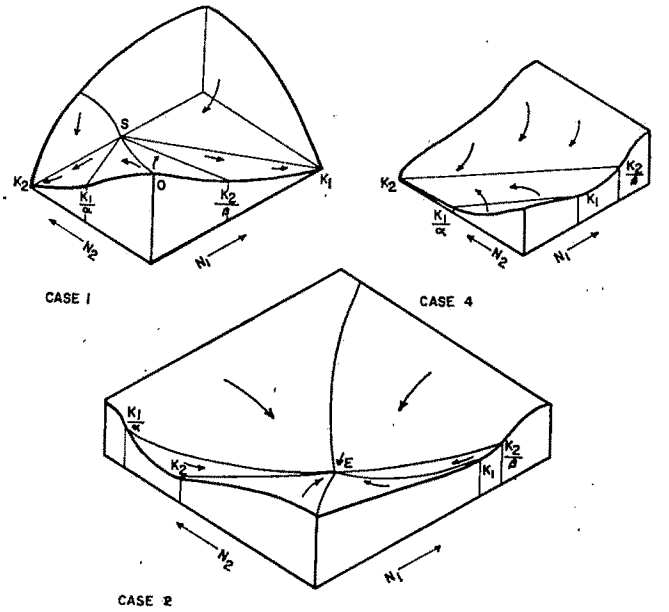


FIG. 3. Topographic model for interspecific competition in which the numbers (N_1, N_2) of two species are plotted against a z-axis whose elevation represents their effective interaction as determined by the respective saturation values (K_1, K_2) and competition coefficients (α,β). The cases plotted assume particular numerical relations between the saturation and competition coefficients such that (a) either species may survive depending on the initial N_1, N_2 (Case 1), (b) both species will survive (Case 2), and (c) only Species 2 will survive (Case 4). Note that in contrast to the adaptive model of Fig. 2 the populations in the present case are postulated to move downhill to the lowest points in the landscape, specific paths being determined by the starting positions as indicated by the arrows [48].

valleys. Such analogs have been used effectively by Eyring in developing the theory of reaction rates (Fig. 1.4 in [25]) and have several interesting biological applications.

For example, in dealing with genetic drift in populations Sewall Wright and others have visualized a landscape in which x and y represent dimensions of genotypic variation in a polymorphic population, and z graphs the adaptedness of any particular combination of these, peaks being highly adapted, valleys relatively maladapted genotypes

(Fig. 2). In such a terrain natural selection would force a population with a particular gene frequency not on a peak to move upward toward higher levels along paths like those in the figure [34].

An analogous biological application has been made in relation to interspecific competition where the numbers of individuals of two species competing in a restricted ecological space are represented by x and y, while z is a measure of their effective interaction determined by the saturation values for the two species and their competition coefficients (Fig. 3). In such a model any momentary competitive position, defined by the numerical ratio of the two organisms, would be forced by the ecological relationships assumed, to move downhill toward the sinks where competitive interaction is at a minimum [48].

Note that while the x-y coordinates of these analogs display relationships between directly measurable dimensions (genotypes and numbers of individuals), the z-axis and the proposed oriented movements of populations over the surfaces thus generated involve rather extensive use of constructs and hypotheses. This emphasizes the care required in deciding to what degree a graph or a mathematical expression is "just" a description of data and to what degree it is a model or an explanation in the sense defined above. Even the simplest cartesian graph ordinarily involves interpolative and extrapolative predictions which go beyond the actual observations.

But to return to the fundamental problem of describing multidimensional data, one important means of considerably exceeding the usual graphic limitations of 2–3 dimensions is the use of nomograms [33]. These alignment charts are in effect graphic tabulations which permit an unlimited number of variables to be plotted simultaneously on a plane surface. In such graphs a great many variables could become unwieldy, but as many as ten can represent highly informative descriptive correlations in which quantitative coordinate values may be obtained simply by placing a straight edge on the chart and reading off the resulting intercept value for any coordinate. Obviously, in such a graph, measurements of any two variables will determine all the rest by this means.

One of the most fruitful applications of this technique (which has been little used by biologists) was made by L. J. Henderson in his important but neglected book "Blood, a Study in General Physiology" [22]. To describe blood adequately as a physico-chemical system Henderson showed that at least 8–10 variables had to be taken into account. Obviously many more are pertinent and were in fact measured, but these can be rather simply derived from the particular 8–10 selected.

Thus the over-all functional relations of human blood, as studied by Henderson and his co-workers, are summarized in a single 10-coordinate nomogram for a particular individual man at rest (Fig. 4). In such a

multidimensional graph if a ruler is aligned with a specific measurement of blood pO_2 and with a second point for the concurrent pCO_2 measurement, all the correlated values for serum pH, volumes per cent oxyhemoglobin, cell pH and so on will lie directly on the same straight line. Two such intercepts of particular interest are the lines defining the equilibrium states for arterial and venous blood.

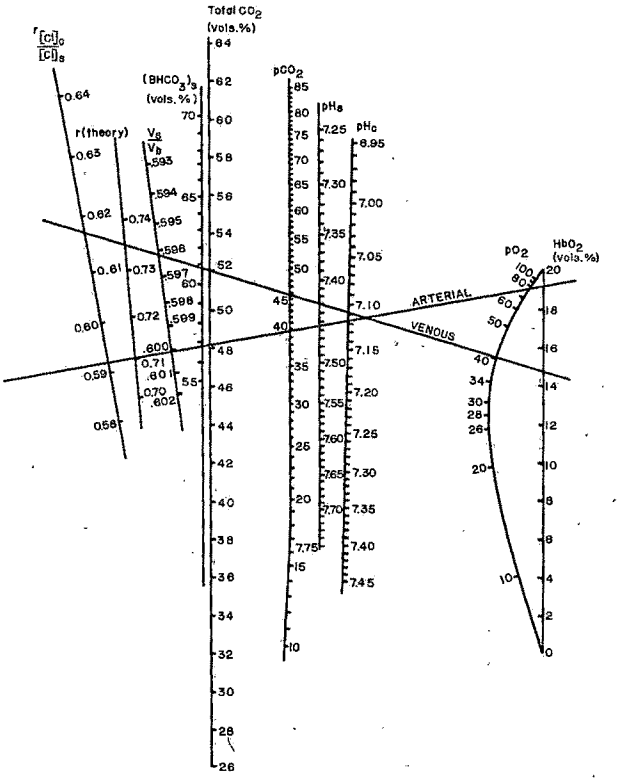


FIG. 4. Nomogram indicating the relationship between ten physical chemical parameters of the blood of a man at rest. A straight line laid across all of the coordinates will determine the simultaneous values of each of the ten variables. The normal limiting cases for the arterial and venous equilibria are shown [22].

Evidently graphing of this sort permits systems of moderate complexity to be well presented and compared. Thus nomograms of different physiological or pathological states may be quite distinct (Figs. 5 and 6), and the blood of various species yields characteristic configurations.

Henderson, himself, felt that such a nomogram was the Law of the Blood and hence, in the sense of providing intellectual satisfaction, explanatory. However, opinions would obviously differ here on the

extent to which such a representation significantly makes use of deductions from broad general principles. As pointed out above, the distinction between description and explanation must be made carefully and in certain areas at least is only relative. For example, none of the coordinates in the blood nomograms are as far abstracted from the measured variables as was the z-axis in the ecological and genetic topographic models cited above. In fact the nomograph coordinates as used by

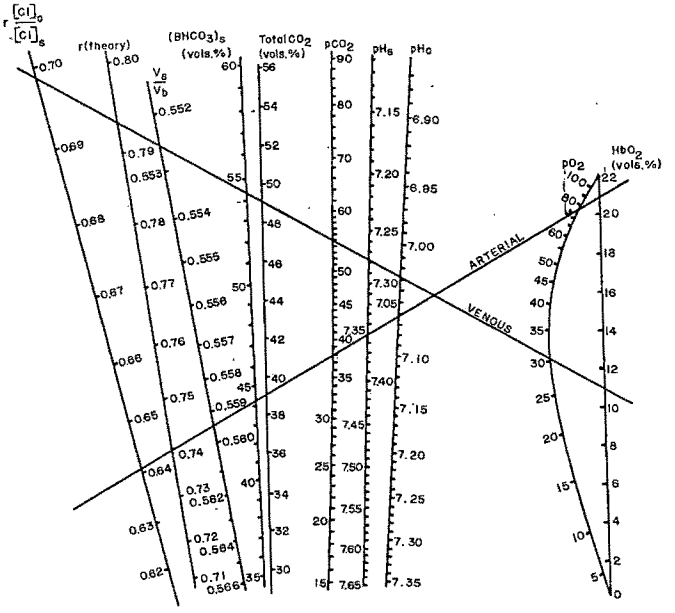


FIG. 5. Human blood nomogram like that of Fig. 4 except that here the subject's metabolic rate was augmented nine times by muscular work. Note that the relative positions and significant ranges of the ten coordinates have changed from the rest condition and that the differences between the arterial and venous equilibria have widened considerably [22].

Henderson appear to comprise direct descriptive representation rather than generalized constructs or explanatory mechanisms. If so, this representation is clearly a contribution to quantitative but not to theoretical biology.

So far the graphic methods mentioned have mainly been metric in nature; even though a strong degree of interdependence is characteristic of biological systems' components. Therefore, the importance of oriented graphs should not be overlooked since these can explicitly represent the relational features of a multidimensional organization [9,45]. A genetic chart showing genotypic and phenotypic ratios in successive generations is a simple case in point; a more complex example is the

flow chart for metabolic reactions in glycolysis (Fig. 7).

Here the interrelations between numerous reactants, products, enzymes and cofactors in the many phases of the total phenomenon cannot be presented more simply than in an elaborate diagram which is essentially a multidimensional oriented graph. A comparable situation holds for representations of pathways and functional interrelations in the nervous system of animals.

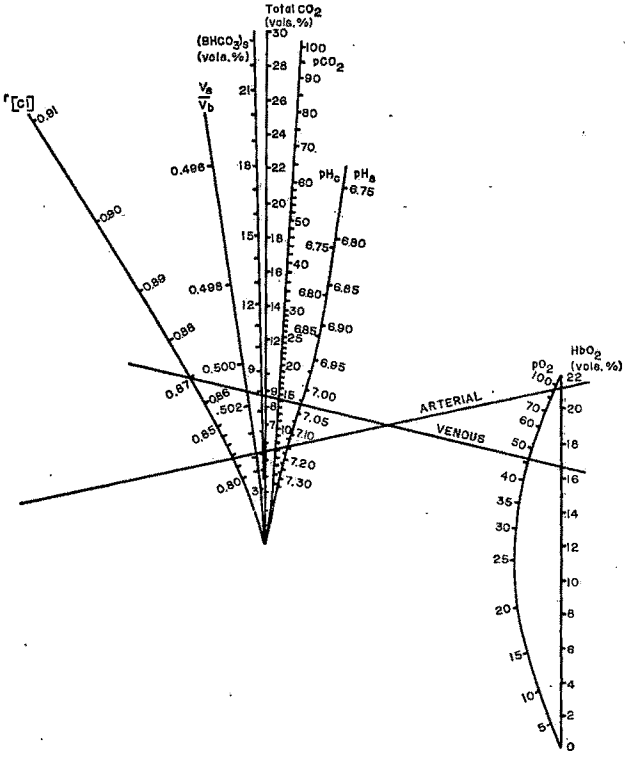


FIG. 6. Human blood nomogram like that of Fig. 4 except that the pathological condition in diabetic coma is depicted instead of a normal state. Marked differences are notable in the positions and scales of a number of the coordinates [22].

Since certain of the graphic methods discussed above (topographic models and flow charts) come close to being physical models or analogs [7] of biological systems, the latter methodological tools should be mentioned more directly. These white boxes, mentioned above, are of many sorts and their discussion is not simplified by the numerous distinct definitions given to the words "model" and "analog" themselves. Thus a physical model may be rather literal in imitating selected properties of the organism, like a mechanical model demonstrating how

an insect flaps its wings [44], or it may operate in quite another energy sphere as an electrical network modeling some hemodynamic properties of the circulatory system.

Note that as above, the distinction between a model's function in description and explanation will become blurred when the known general principles on which the model works are broad enough to include the

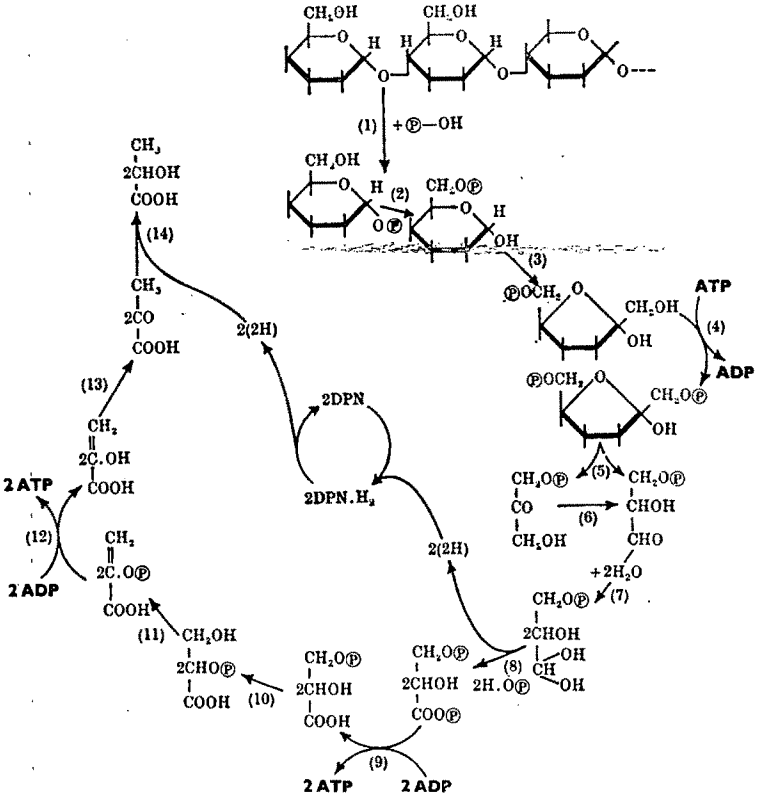


FIG. 7. Flow chart summarizing the metabolic reactions in glycolysis from the phosphorylation of glycogen (Reaction 1) to the formation of lactic acid from pyruvic (Reaction 14). In the process one high energy phosphate bond is used up (Reaction 4) and four are generated (Reactions 9 and 12) [4].

living system as a special case. Thus a mechanical or electrical analog may be used merely as a computer to interpolate or extrapolate responses which have not yet been measured in the organism, or it may go far beyond that to demonstrate in an explanatory way the principles on which a given living system works. In this connection recall that the broadest definitions of a model (the white box) would also include propositions or equations which deal with certain system parameters in symbolic rather than physical ways [26]. Both analog and digital

computers may be used to run computer "experiments" on such mathematical or logical models, as discussed below.

Equations. Mathematical ways of describing biological systems have often been utilized in cases where adequate quantitative data were available. In a number of interesting examples these take the form of exponential equations of the form $y = ax^b$. Such diverse biological functions as relative growth, relation between oxygen uptake, and body weight [10,58] and the effect of stimulus magnitude on the resultant sensations [56] (Fig. 8) are well described by equations of this form [15].

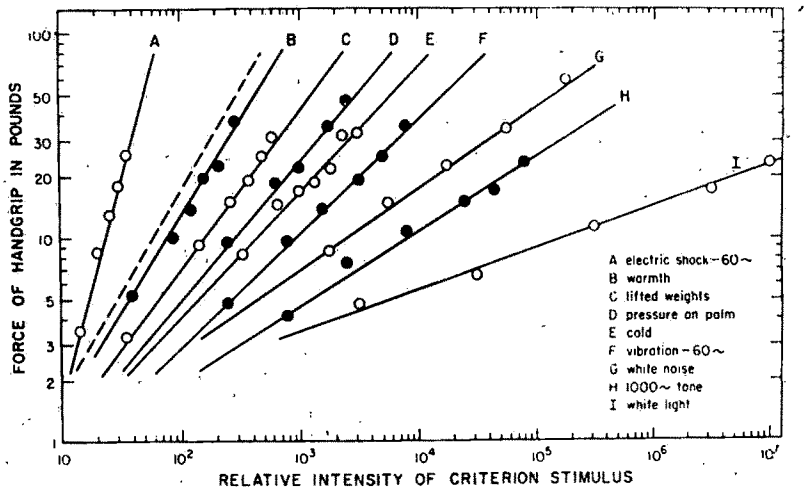


FIG. 8. Exponential relationship between stimulus intensity and psychophysical response confirmed for nine different kinds of sensory stimulation by matching their apparent intensity to force exerted on a hand grip. Points are averages for at least 10 observers. The slopes of these power functions show a remarkably good correlation with those predicted on the basis of magnitude estimation for the individual modalities [56].

These may be derived from data plots by curve fitting, in which case their variables and coefficients may be obtained by a complex series of inductive and deductive steps such that operational definitions and biological significance are available for the elements in the equation [20].

One general area where considerable success has accrued from such procedures is the field of animal locomotion. Here, following the pioneer contribution of Borelli [12], quantitative applications of mechanics, hydrodynamics, and aerodynamics have led to particularly interesting analyses of walking, swimming, and flying [18,44,59].

Following the lead of classical physics the most elegant way of describing a natural system is to formulate one or more differential equations which express the relations between the key variables. For example, the exponential equation for the relative growth curve cited above can

be deductively obtained by integrating the differential equation [10]:

$$\frac{1}{y}\frac{dy}{dt} = b\,\frac{1}{x}\frac{dx}{dt}$$

This is a generalized hypothesis for the growth rates of two dimensions (x, y) of an organism, based on the assumptions that the growth of one dimension, say y, is simply proportional to the amount of y already present and that the ratio between the specific growth rates for the two dimensions can be expressed by a coefficient b which is assumed constant within certain boundary limitations. On this basis the a of the exponential form of the relative growth equation is seen to be just an integration constant.

Differential equations have been similarly applied to membrane potentials and nerve conduction [23,45], population growth and competition [30,48], the metabolic application of reaction rate theory [13,25], and a variety of other biological systems [15]. When more than two or three simultaneous equations relate to a given case, the mathematics become difficult to deal with, but the use of computers has permitted quite complex systems to be handled quantitatively (e.g., the work of Chance and his colleagues discussed below).

Note that while ordinary first order linear differential equations with constant coefficients can be treated mathematically in a satisfactory way, higher order variables, variable coefficients and nonlinearity greatly increase their difficulty. Although natural systems often are nonlinear and involve time-dependent coefficients, their approximate treatment as linear systems has been highly fruitful [57]. In addition to such classical methods of the physical sciences it seems quite certain that the application and further pertinent development of set theory [17], dimensional analysis [53,54] and of multivariate analysis [1,29] will play important parts in the further development of mathematical biology.

Systems Analysis. In order that inductive generalizations as well as deductive predictions and their empirical verification can be generated in biology, information for a large number of components and their interrelationships must be available in the form of graphs, models or equations as suggested above. In addition, some over-all organizational scheme must be postulated to provide a framework within which all the numerous parts may be ordered. While it is true that such basic organization would be self-evident if all details were known, biological knowledge is far from that state. Hence hypotheses and generalizations about the organization of living things are an important part of methodology.

The traditional biologists have long been well aware of the importance of underlying organization and have in a number of instances greatly advanced our understanding by emphasizing its quantitative functional

significance. For example, William Harvey's demonstration of the circulation of blood, Claude Bernard's attention to stablization of the "milieu intérieur" and Walter Cannon's analysis of the homeostatic functions of the autonomic nervous system derived their great strength from what nowadays would be considered a kind of systems analysis. In the past twenty years the new biologists have been applying and developing more formally derived methods to achieve such understanding. Information theory, cybernetics, game theory and decision theory have been the main contributors to this movement. All of these have provided insight into possible organizational properties of living systems and have at the same time suggested ways of analyzing biological data which take account of such interrelations.

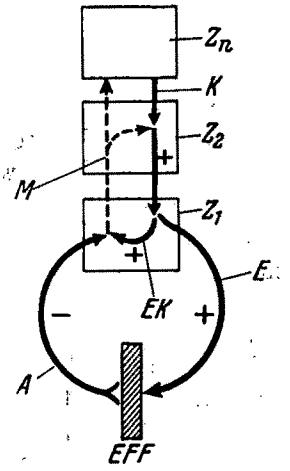


FIG. 9. The Reafferenzprinzip proposed to account for the usual absence of reaction by animals to apparent movements generated by their own voluntary displacement. In this model a higher nervous center (Z_n) sends out a command (K) relayed through two lower centers Z_2 and Z_1. In the latter this command is sent out as an efferent motor message (E), and a duplicate of the efferent signal (EK) is sent to another part of Z_1 where it is compared with the afferent sensory message (A) resulting from the movement evoked by (E). Any discrepancy between the sensed displacement and that expected on the basis of K is fed back (M) to Z_2 or Z_n where appropriate adjustments may be made. If EK and A correlate perfectly, there will be no response to the apparent movement [24].

Although this approach has been widely hailed and does in fact offer important potentialities, relatively few significant new applications have materialized. One interesting theoretical development from cybernetics was the Reafferenzprinzip of von Holst and Mittelstaedt [24] (Fig. 9). This postulates a general mechanism whereby the central nervous system can discriminate real from self-induced apparent changes in an animal's environment by comparing the actual sensory input at any given moment with that expected on the basis of the animal's own activity. In a few experimental situations applications of systems analysis to relatively simple reflex mechanisms have also been rewarding [e.g., 38,39,55] (Fig. 10), yet the effective exploitation of this general field lies largely in the future and undoubtedly depends on a real fusion of the traditional and the new biology.

Perhaps the most impressive applications of theoretical and mathematical techniques to living things have been cases where differential

equations were set up to describe the various rate processes in the system, where these have been programed into a computer which has carried out various experimental solutions of the simultaneous equations involved and where the predictions of such specific quantitative models were directly compared with the outcome of actual empirical tests. Three particularly interesting instances may be cited.

One example comes from the area of developmental biology where Weiss and Kavanau have proposed a specific mathematical model for

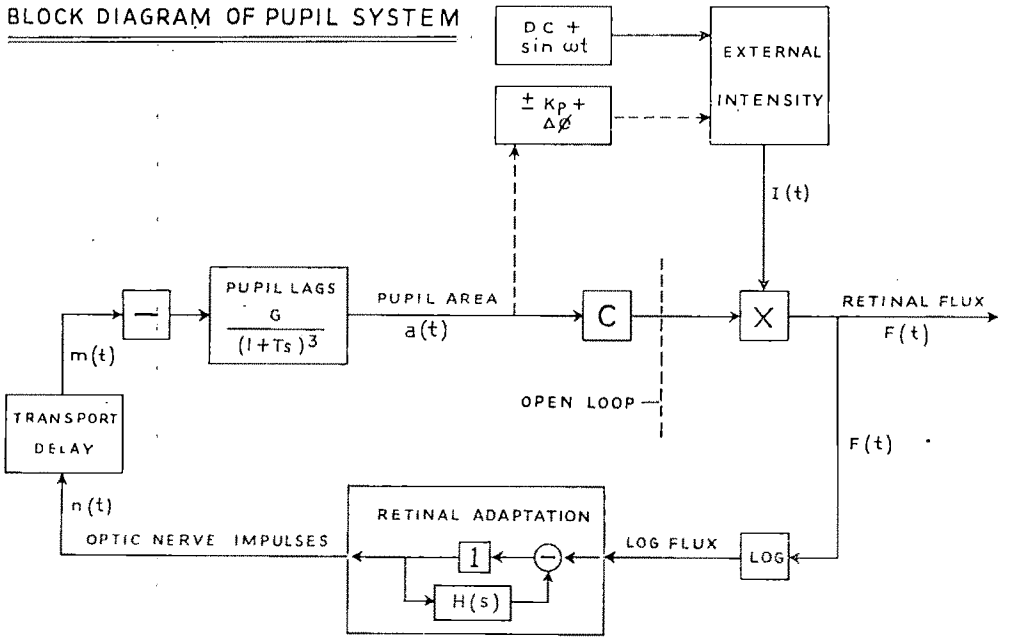


FIG. 10. Cybernetic model for system regulating pupillary aperture in the human eye. I, external intensity; F, retinal flux; H(s), the transfer function of the control system; DC + sin ωt is sinusoidal light source; ± Kp + Δ φ, electronic feedback control of light intensity; G, pupillary gain; T, time constant; C, point where pupil edge regulates amount of light reaching the retina [55].

growth and differentiation, the predictions from which have undergone some empirical verification [27,28,60]. The model combines three differential equations representing processes which include a closed loop permitting negative feedback ultimately to regulate development in the adult steady state. These equations define the rates of change in generative mass, in differentiated mass and in an inhibitory component which is postulated to account for the ultimate slowing and stabilization of growth and differentiation. While the equations for these processes are quite complex in the general case (equations (1, 2 and 3), Fig. 11), they can be considerably simplified if over-all growth is assumed to be es-

sentially complete and only regenerative growth in the adult organism is considered.

Based on available data appropriate values have been assumed for the various coefficients of these equations and a digital computer program carried out to explore the implications of the mathematical model. When these are compared with empirical data of experiments on compensatory growth, the systems analysis predictions show a number of interesting correlations and suggest certain specific new experiments to provide a more rigorous test of the underlying hypothesis.

A somewhat more complicated mathematical model has been proposed and quantitatively compared with experimental data relating to photosynthesis by the red alga *Porphyridium* [16]. Here a two-pigment,

$$dG = G\left[\left[1 - \frac{b(I/I_e)^x}{(V/V_e)^x}\right]\log 2 - k_2 - k_1\left[1 - \frac{I/I_e)^x}{(V/V_e)^x}\right]\right]dt,$$

$$dD = \left[k_2 G + k_1 G\left[1 - \frac{(I/I_e)^x}{(V/V_e)^x}\right] - k_3 D\right]dt,$$

$$dI = k_5 I_e\,[|(G/G_e);(D/D_e)|] - I/I_e]\,dt.$$

$$\text{restrictions:}\quad \left[1 - \frac{b(I/I_e)^x}{(V/V_e)^x}\right] \geq 0, \left[1 - \frac{(I/I_e)^x}{(V/V_e)^x}\right] \geq 0.$$

FIG. 11. System of differential equations hypothesized to model growth and growth control. The first equation relates to change in the generative mass G, the second to change in the differentiated mass D, and the third to change in an inhibitory factor I (antitemplate molecules). The k's are rate constants; b, ratio of equilibrium negative feedback to complete inhibition; subscript e, terminal value; x, number of antitemplate molecules required to inactivate one template; V, volume of dilution pool. At equilibrium (maturity) these equations simplify considerably so that computer solutions become feasible for regeneration [27].

photochemically-driven model was established with a set of five differential rate equations. The properties of this hypothesized kinetic system were then shown by means of an analog computer to account for a considerable number of experimental findings on the living system.

An even more complex and far reaching analysis of a basically similar sort is provided by the work of Britton Chance and his co-workers on metabolic regulation [13]. This analysis is particularly striking since it deals with one of the great generalizations of 20th century biology, namely that the main stream of energy flow in aerobic heterotrophic organisms occurs through the complex network of chemical reactions involved in glycolysis, the tricarboxylic acid cycle and the electron transfer system.

Thus the basic model used by Chance is conceived in terms of an oriented graph depicting the four primary systems and their interrelations in aerobic respiration (Fig. 12). Then a series of twenty-two differential equations, based on the mass law, are derived for a selected set of specific key reactions involved. These are programed into a digital computer in such a way that a solution of these simultaneous equations is graphed by the machine's output (Fig. 13). This arrangement permits experiments such as the addition of glucose or of an uncoupling reagent to be carried out on the computer model. Comparison of such tests with the outcome of empirical experiments where quantitative fluorimetric and spectrometric techniques of high resolution provide extensive data from living cells show that to an astonishing degree the specific behavior of the metabolizing protoplasm can be quantitatively accounted for in terms of the broad principles hypothesized in devising the system's organization and its detailed modeling.

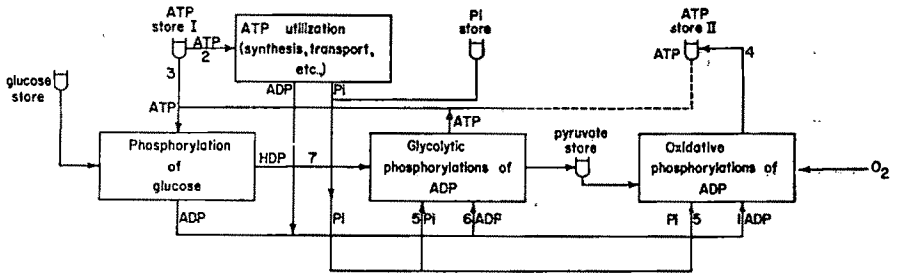


FIG. 12. Flow chart showing the major subsystems in aerobic respiration and their functional interrelations. With reference to metabolic regulation the relationships of the over-all system to stored ATP (cytoplasmic and mitochondrial), inorganic phosphate, glucose and pyruvate are of particular interest [13].

Conclusions

In the author's opinion, work of the sort illustrated by these three examples provides a cogent indication of the direction in which biology must move. Such multivariate quantitative and theoretical approaches need to be devised and applied not only in the study of photosynthesis, development and biochemistry, but in all other areas of biology including systematics, morphology, paleontology and biogeography. Although it has not been specifically mentioned, these developments must include, when appropriate, the explicit treatment of the statistical and stochastic aspects of living things [5,6,11,29,37,50].

Only in this way can the dangers of a continuing schizoid biology be avoided. Only in this way can biology outgrow the stigma of being a mere descriptive science and rise to the level of a rigorous intellectual discipline. To do this the methods not only of biology itself but also

those of mathematics, physics, chemistry and engineering are obviously required even though the examples cited above may only tentatively suggest the lines which will ultimately prove most fruitful.

On the other hand protoplasm, organisms and populations must be considered wholistically because their behavior cannot be adequately encompassed just by reduction to molecular or physical or chemical explanations. Hence biologists must be central participants in research

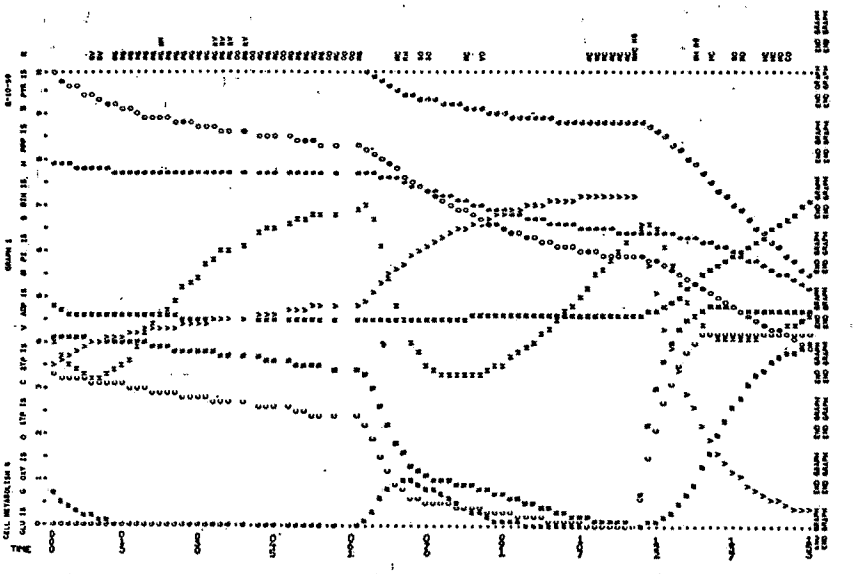


FIG. 13. Part of the computer output in the model experiment carried out by solving the 22 simultaneous differential equations defining aerobic respiration under various particular conditions. The abscissa represents time in seconds and the ordinate concentrations of the various components symbolized by the typed letters. From time 0-064 metabolism of endogenous substrate was occurring; from 064-119 the metabolic effect of added glucose was demonstrated; at 119 oxygen and glucose inhibition were relieved by addition of an uncoupling agent. O, oxygen; G, glucose; $, inorganic phosphate; H, intramitochondrial reduced DPN; R, pyruvate; %, enzyme intermediate concerned in ATP utilization; V, mitochondrial ATP; C, cytoplasmic ATP; #, ADP [13].

on living things. Yet many of them must learn to work and think effectively in the other relevant scientific disciplines as well as to collaborate with scientists better trained in such fields than they can realistically hope to be. No other means is available to obtain the kind of data and to carry out the kind of analysis required to advance the biological sciences effectively.

If indeed such a synthesis of the new and the old biologies can be effected, it may not be too sanguine to predict that the present nuclear age of science will be succeeded by a spectacularly revolutionary biological age in which the understanding of life, of growth, of evolution, and

of man and his behavior would far transcend any of the tentative beginnings we now know. However, we should not overlook the pitfalls and difficulties in such a revolutionary path. There is a challenging difference between the vague awareness of a problem and the energetic insight required to solve it. The forces of reaction are great; they are favored by inertia and the formidable difficulties of creative advance in any new or interdisciplinary area.

Nevertheless the rewards of such an effort are so attractive and the satisfaction to be derived from its success so great that it is surely worth a more serious try than has yet been made!

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DIRECTIVENESS IN BIOLOGY ON THE MOLECULAR LEVEL?

By PETER T. MORA

LIVING processes, in contrast to non-living, have a characteristic, all-pervasive property, which was described by words such as "directiveness" or "purposiveness." Ample evidence of "directiveness," mainly in animal biology, has been presented in a delightful book by E. S. Russell [1]. Even Oparin argues in his last book [2] that universal "purposiveness" of the organization of living beings "pervades the whole living world from the top to the bottom, right down to the most elementary forms of life," and it "is an objective and self-evident fact which cannot be ignored by any thoughtful student of nature." "Purposiveness" properly indicates that the process will end up by benefiting the organism, but unfortunately has connotations of consciousness of the goal in sight, which I do not wish to imply. Therefore, the word "directiveness" is a better choice, as it was also preferred by Russell. I do not wish, however, to imply that non-material factors are directing biologic activities. I intend to use the word "directiveness" exactly as Russell did, to refer to an inherent property of life.

It is quite obvious that individual animals and plants, even very small organisms, behave as though there were a directiveness in many of their activities, in maintaining and restoring their structure, in absorbing metabolites, in adjusting to their environment, in performing their predetermined life cycle including reproduction. This directiveness is obvious when living processes are observed as a whole, and under natural conditions where many complex processes operate simultaneously. However, when observation and scientific research are done on biologic problems on the molecular level and under simplified conditions, often in an isolated state, this directiveness is lost from sight.

The purpose of this essay is to be provocative: to bring to attention this fact, and to query what are the reasons for this, and to beg for new approaches. Of course, observations on the molecular level can be independently very useful and often lead to spectacular applications. But, the question I raise is whether it is possible to make observations and statements on the molecular level which pertain to this "irreducible" [1] characteristic of life—this directiveness of living activity.

One way to go about this is by considering biologic phenomena lower and lower in size and complexity, gradually approaching the molecular size. We might see then where the directiveness or, more properly, our ability to detect or investigate directiveness is lost from our sight.

Let us choose one aspect of directiveness for such investigation, the directed or oriented interactions in biology. I choose this aspect because

I have some familiarity with it, and to my knowledge this aspect of directiveness in living processes was approached best by molecular physicochemical research. I wish to emphasize, however, that oriented interactions represent only one and a rather limited manifestation of the directiveness in biology, which Russell so brilliantly surveyed.

As one lowers the scale and tries to find simpler and smaller biological systems and one approaches the macromolecular level, one can still find evidence of directiveness, for example, in the process of oriented interaction of a bacterial virus with the surface of the receptor cell. The bacteriophage T2 always attaches tail first to the cell wall of *Escherichia coli B*, and each single bacteriophage which attaches infects the bacterial cell with its DNA and effectively produces over a hundred bacteriophage progenies. This occurs in spite of the fact that the area of the cell surface which is suitable for attachment and infection by any particular type of bacteriophage is only a small percentage of the total cell surface [3]. Viruses, such as bacteriophage, do not have their own motility and they are brought by Brownian motion and movements of the fluid in which they are suspended in a random orientation to the proximity of the cell wall. However, when they are close enough to the receptor cell surface, they must turn over if they approach the cell surface with head first, because they attach exclusively by their tail.[1]

At first sight, it appears from certain experiments that the attachment is indeed 100 per cent effective once the viruses and their receptors are close enough, and only the diffusion rates of the components control the rate of attachment. For example, in an experiment Tolmach [4] placed *E. coli* bacterial cells in an aqueous suspension into alternating electrostatic field of varying frequency. The bacterial cells, which are large compared to the phage and have net negative charge, were pushed back and forth as the sign of the field changed, and swept out an approximately cylindrical fluid volume. Then, he suspended in this solution a known amount of phage, and, knowing the diffusion rate of the bacteriophage, he could estimate how much phage diffused back into the volume which was swept out by the bacterial cell. The loss of the bacteriophage from the whole suspension, and the frequency of the field where the added number of encounters were half of maximum, were commensurate with the assumption that every such forced collision resulted in irreversible absorption of the bacteriophage to the *E. Coli* cell wall. In other words, the reaction rate was diffusion-controlled. A somewhat similar experiment and the same conclusion, that the efficiency of reaction per encounter is close to unity, were presented by Valentine and Allison [5], using another virus and adsorption surface.

[1] This might be (as Dr. Roy Markham kindly brought to my attention) because of the apparently rapid rotation of the elongated phage particles around their heavy (DNA full) head.

Unfortunately, however, interpretation of these types of experiments is based on a theory by Smoluchovski [6], originally proposed for the coagulation of colloids, and these experiments cannot distinguish between a surface concentration of value zero (as is assumed) and a value of, say 1 per cent of the initial bulk concentration. This means, as Prof. A. G. Ogston kindly pointed out to me, that the efficiency of collision cannot be estimated within the range 1 to 10^{-n}, where n may be as large as 4.

Nevertheless, a large amount of empirical observation strongly suggests that specific structures participate in a very effective way in the interaction of the bacteriophage and the *E. coli* cell [7]. For example, as can be seen in electronmicrographs, slender protein fibers (1300 Å by 20 Å) come out from the tip of the tail of the phage, and these fibers are assumed to act as attachment organs, as tentacles which probably "feel out" the cell surface and orient and bring about the attachment of the tail tip with the complementary area on the bacterial cell surface so that the infection can occur. Then the complex and very purposeful process continues (the phage tail sheet protein contracts, acting as a muscle in driving the rigid tail core through the bacterial cell wall; then the DNA is injected, all within a couple of minutes) which eventually culminates in the formation of over a hundred virus progenies inside the bacterial cell in a matter of 20 minutes [8]. It is worth while to keep in mind that the size of a single bacteriophage is in the range of 2000 Å and the structures which are involved in the attachment process consist only of a limited number of macromolecules (the tail fiber and tail tip proteins and probably a small area on the cell surface). Thus, while there is no assurance that the efficiency of encounter is close to unity (because there is limitation imposed by theory) simply on experimental observations the overwhelming impression of a very efficient, directed interaction cannot be escaped.

Going further down in the scale, there is other evidence of a very efficient interaction in the bacteriophage field. The neutralization of the tail tip of a similar bacteriophage (T4) by its antibody seems to be a process where the proportion of effective collisions is in the order of unity [9]. Unfortunately, certain values which have to be assumed for some unmeasured parameters in this estimate might not have been approximated close enough to exclude a difference of, let us say, one order of magnitude. Still, within this limitation, fully effective collisions are indicated for the neutralization (inactivation) of the bacteriophage by the antibody molecule under favorable circumstances (low salt concentration).

In experiments with bacteriophage a single biologically active particle (virus) can still be observed, both by the electronmicroscope and, also, through its manifestation. The fundamental assay is the so-called plaque

assay, in which each plaque is produced originally by the interaction of one phage with one bacterium.

It is much more difficult, if not impossible, to assess the same problem of efficiency of collisions on the molecular level. First of all, one is restricted to the observation of the effect of a population of molecules which obviously makes it very difficult, if not impossible, to devise experiments to show whether there is an orientation or some other mutual directing influence between, for example, an enzyme molecule and its substrate, which will then cause a high frequency of effective collisions. Still, most investigators who have given thought to this problem assume that there is some orientation, and that the proportion of effective collision is very high. For example, there is substantial evidence for the presence of electrostatic attractive forces facilitating fruitful encounters of enzymes and their ionic substrates [10]. It is generally assumed that the interaction of enzyme and substrate (E + S) is not the rate determining step in enzyme reactions when the substrate concentration is sufficiently high.

We tried to devise an experimental approach to study the very first step of the enzyme substrate interaction. We thought that, if we included with the substrate an inhibitor which has a similar net charge to the substrate, and then added the enzyme which is oppositely charged to both the substrate and inhibitor, we might study the long-range electrostatic forces which are the most likely to cause an oriented interaction between the enzyme and the substrate molecules [11]. For example, we studied the inhibition of the ribonuclease (RNase) depolymerizing activity on ribonucleic acid (RNA) by using a stronger polymeric acid than RNA. RNase is positively charged at near neutral pH, while the two polymeric acids are negatively charged. We could conclude only, however, that non-specific electrostatic forces, and non-electrostatic specific forces both appear to act to about an equal extent between the RNase and the RNA under normal conditions (close to neutral pH and physiological salt concentration) and these two types of forces are in equilibrium; the equilibrium shifts with changes in salt concentration and in pH according to predictable changes in the mutual charge effects of the macromolecules on each other, and according to whether the electrostatic forces are less or more screened with decreasing or increasing salt concentration [12]. But such competition experiments only indirectly inform about the forces operating between the enzyme and the substrate, and do not say anything about orientation during the enzyme substrate interaction.

Actually, it is difficult to separate the E + S process and study it separately from the rest of the enzyme reaction. Generally, it is an end product or a change in the substrate which is observed experimentally by an enzyme assay. Nowadays numerous schemes and theories are

presented on various enzymes to break down the enzyme mechanism to many intermediate steps. Then it is rather arbitrary to divide kinetically the several steps which can be thought to occur, and we may choose any value, including unity for the efficiency of collision (that is the E + S process).

Still it is possible to make calculations using the over-all rate observed by an assay and taking certain molecular parameters (diffusion rates, diameters of both enzyme and substrate, etc.). We have undertaken [13] such calculations on about a dozen enzyme systems, using a modified theory [14,15] of Smoluchowski which does not assume zero surface concentration. The result of such calculation was that the probability of efficient collision α (the fraction of collisions which lead to reaction) is generally of the order of magnitude of $\alpha = 10^{-9}$ to 10^{-13}, with two exceptions, when $\alpha = 4 \times 10^{-5}$ and $\alpha = 5 \times 10^{-6}$. However, when calculations take into account the diffusion of the substrate into a small hemispherical reaction site on the enzyme having a reaction radius of 5 Å, and the effect of electrical charges is also included, the calculated minimum value of the second order rate constant (about 10^{10} M^{-1} sec^{-1}) is commensurate with a diffusion-controlled bimolecular reaction [16]. The estimated lower limits of the bimolecular rate constants may argue for the existence of several diffusion controlled enzyme substrate combinations, but only within an order of magnitude [17].

With all of these limitations and difficulties on the molecular level, partly theoretical, partly experimental, one becomes suspicious. Could it be that the conceptual and experimental restrictions in which we find ourselves when investigating phenomena on the molecular level, make molecular approach unsuitable to explain phenomena evident on the level where the individual biological entities can be observed, and for this reason the phenomenon of directiveness eludes us? Is it possible that our thinking in physical science is so much restricted to molecular and statistical approaches that we find ourselves unable to treat physico-chemically the actual difference between biological and non-biological phenomena, exactly because of this restriction? We have seen that, when observation of a single biological entity (such as a virus) is possible, it is still possible to accumulate empirical observation which shows a directiveness and an unusual efficiency of biological processes to an extent which never manifests itself in purely chemical processes. But, once the observation is on the molecular and statistical level we cannot *see* such efficiency.

Or is this the case because complex and directed or purposeful interactions between many factors, probably millions of molecules in the simplest of living cell are essential to living processes? Since the age of Galileo our experimental and theoretical approach to physical science is fundamentally analytical, which includes simplifications, omissions of

disturbing influences, to allow us to study only one thing at a time and, most important of all, we carefully avoid any consideration of purpose. Could it be then that such an approach is not sufficient to study or to explain the complex interrelationships so essential to life?[1]

Perhaps, if we physical scientistis working in biology do not fear the label of "teleologist," we may begin to think of what we can do to study by the methods of physics and chemistry the essential difference of biological phenomena from the non-biological ones. Then we might develop conceptual and experimental methods to utilize the observations of molecular science and connect the field of molecular biology more fruitfully with the biology of our obvious observation, so that investigations on the level of molecular biology may be utilized to approach problems which pertain to this all-pervasive and distinctive characteristic of life, designated by the word "directiveness." Personally, I apologize for not being able to present a constructive suggestion.

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[1] A similar feeling was expressed recently by Eugene P. Wigner in "The Logic of Personal Knowledge, Collection of Essays Presented to Michael Polanyi on his Seventieth Birthday," Routledge and Kegan Paul, London, 1961. Wigner calculated the probability for the existence of self-reproducing states by standard quantum mechanical theory, and found it to be zero. He thinks that in physical interpretation of biology we may overlook the influence of some kind of "biotonic" phenomena (for the meaning of this term and for related arguments see Walter M. Elsasser, The Physical Foundations of Biology, London, Pergamon Press, 1958, also in *Journal of Theoretical Biology, 1*, 18 (1961) not amenable to quantum mechanical treatment and regrets that discussion of the phenomenon of "consciousness" has become tabu in scientific discussions. "It seems more likely, however" (he states) "that living matter is actually influenced by what it clearly influences: consciousness. The description of this phenomenon clearly needs incorporation into our laws of nature of concepts which are foreign to the present laws of physics."

WAYS AND WAYWARDNESSES OF AMOEBA

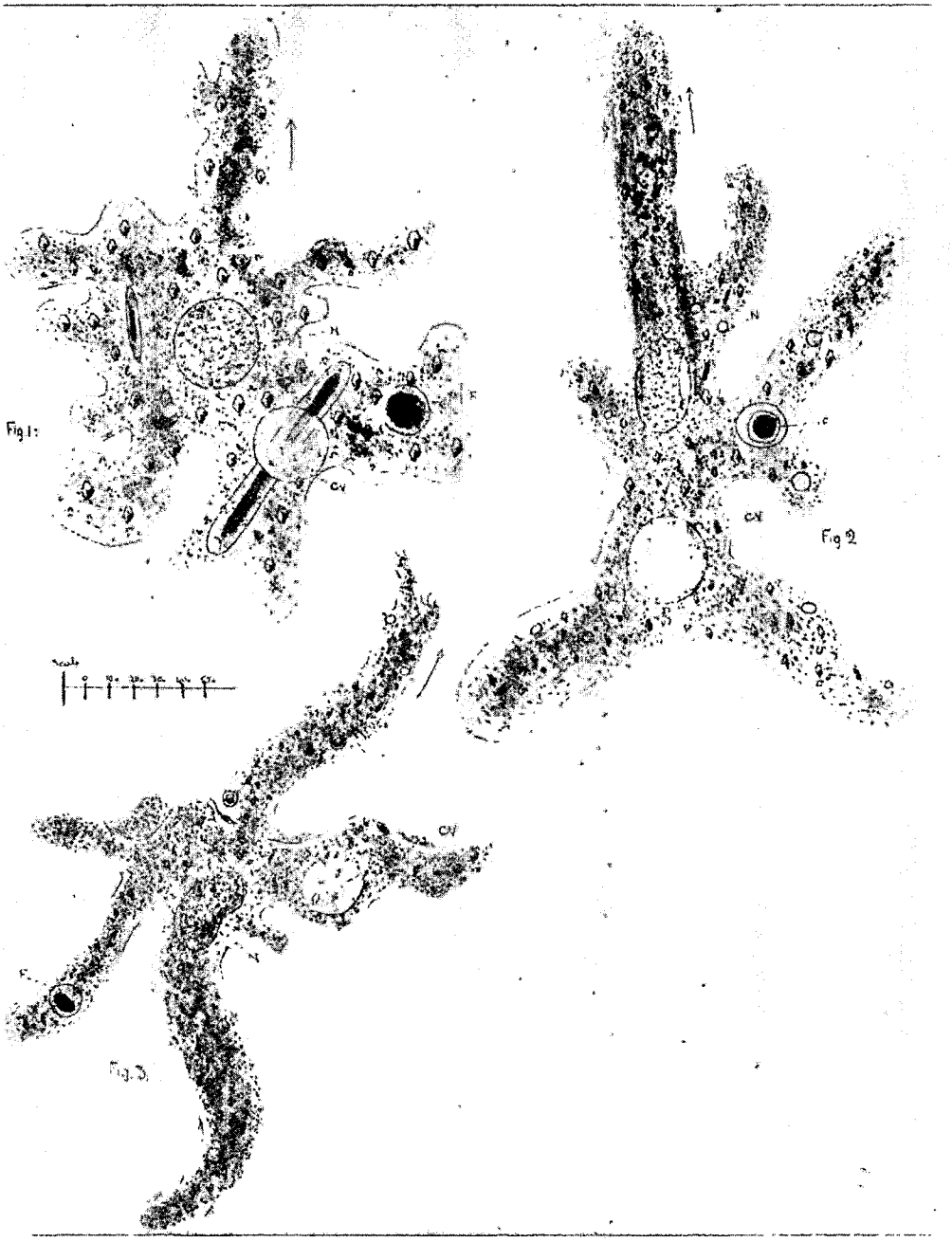
By MONICA TAYLOR

GLASGOW became the seat of a bishopric A.D. 540. Nine hundred and ten years later Bishop Turnbull obtained from Pope Nicholas V a Bull for the establishment of the University. Dwarfed as it is by geological time, historic time, nevertheless, has its lure. It seems fitting therefore that, in connexion with the 512 years' old University, an organism which appeared at the dawn of life should have received much investigation in its biological laboratories. It is not surprising that Amoeba, this aristocrat of aristocrats, which has held its own in the struggle for existence all down the aeons of time should have proved a wayward subject.

The first amoebae to appear out of the primordial slime must, of necessity, have been nonparasitic. The great bulk of scientific research on amoebae has, however, been made on parasitic forms. These are less elusive of approach and of course are of importance in medicine. To those investigators who have a love of all ancient things amoebae that today live freely in a liquid medium as they did in their primordial surroundings offer a more attractive if more arduous challenge to research.

Presumably these forbears of today's amoebae were all alike, i.e., of one species, and all able to subsist without other living organisms for diet, for these were as yet nonexistent. Even in those cases known today of successful axenic cultivation of amoebae, the addition of food organisms, e.g., bacteria to the axenic medium, causes, for example, *Entamoeba invadens* to increase two hundredfold. The gradual appearance and evolution of other microorganisms for amoeba food would hasten the increase in numbers in primitive amoebae and thus with a greater variety of diet and consequently of environment speciation would follow. [The question here arises what makes the axenic environment less attractive.]

In the course of ages, as new food organisms evolve and new environments multiply, it is easy to understand that in the ponds and streams of today are innumerable species of amoebae. One of these, as may surprise many readers of the AMERICAN SCIENTIST, is named *Amoeba hugonis*. It was discovered and described in 1952 and named to honour the Editor-in-Chief, Hugh Taylor. In a citation made on the occasion of the conferring of Honorary Degrees in the ancient University of Glasgow, Professor Alstead playfully referred to this amoeba in the presence of the said Editor-in-Chief. He happened to be the Guest of



LATE 1. FIG. 1. Amoeba dubia. FIG. 2. Young amoeba proteus. FIG. 3. Mature amoeba proteus. *CV*, contractile vacuole; *F*, food vacuole; and *N*, nucleus. (Drawings made by L. A. Carter, 1919.)

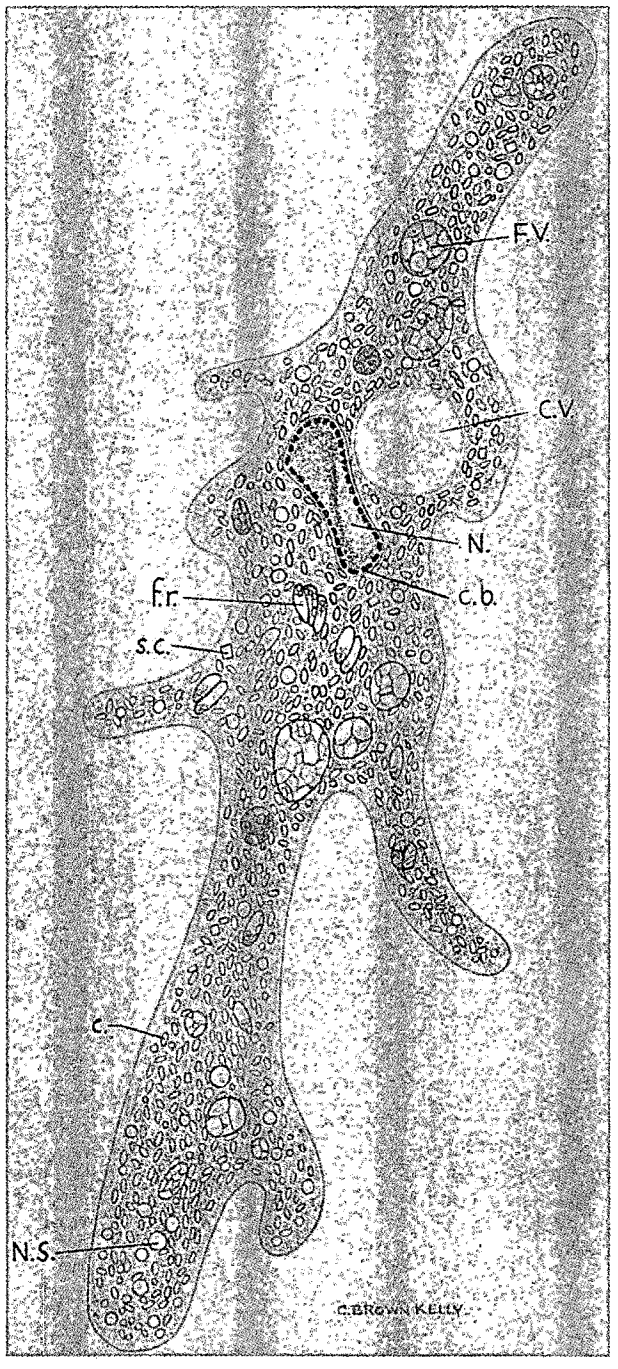


PLATE 2. Amoeba discoides. *C.V.*, contractile vacuole; *c.b.*, chromatin block; *c.*, crystal; *F.V.*, food vacuole; *F.r.*, food vacuole; *N*, nucleus; *N.S.*, nutritive sphere; and *S.C.*, cubic crystal.

Honour at the Commemoration Ceremony of 1953, the year of the Faraday Society Centenary of which he was President. It amused many of his distinguished friends that this Englishman, who had served both America and England so well, should have left behind him innumerable *Amoeba hugonis* in Scotland.

The antiquity of free-living amoebae which dwarfs historic time so effectively may account for some of the waywardnesses which confront their present-day investigators. Hence the research has attracted comparatively few adherents. It seems as though the effects of long ancestry cannot be shaken off; amoebae at all stages during the life cycle, except the young adult at the fission process stage [see later] manifest an extremely lethargic tendency.

However, it must be remembered that this narration is to be published in AMERICAN SCIENTIST, a journal used all over the world by people who want to be educated in, and to become informed about, the present-day status of a branch of science which attracts them, and which, once they are initiated, they can pursue further. So to the task!

With the exception of *A. hugonis* (104 × 52μ), this account concerns itself with amoebae that can be seen with the naked eye as a minute speck and which are used for the beginning studies of doctors, dentists, teachers of biology, agriculturalists, nurses, chiropodists, etc. And here we enter into the first of the thorny pathways which divide up the garden of free-living amoebae research—that of nomenclature.

Nomenclature

Obviously, workers in different parts of the world must be quite sure that they are referring to the same object. A self-evident necessity though this may seem, it does not always receive sufficient attention. Utter chaos reigned in amoebae scientific literature; many of the large amoebae had several names. In 1916, Schaeffer chose three commonly occurring large specimens, described them carefully, and gave them the names: *Amoeba dubia, A. proteus, A. discoides.* In the meantime, Sister Bernardine, Dr. Lucy Carter, who was working on the large Amoebae in the Zoology Department of Glasgow University, had discovered the fact that two different kinds of amoebae were being supplied to her by Mr. Bolton as *A. proteus.* She likewise described each in the *Proc. Royal Physical Society of Edinburgh.* Only at the end of World War I did Carter and Schaeffer find out that they were engaged on similar investigations, and exchanged papers and corresponded with each other.

As regards nomenclature, their results may be tabulated:

Carter's		Schaeffer's
A. proteus X	is	A. dubia*
A. proteus Y	is	A. proteus

* Prior to 1926 *A. dubia* is referred to as *A. proteus.*

Carter seems not to have encountered *A. discoides.*

In addition to Sister Bernardine's scholarly survey of Amoeba bibliography up to 1919, the whole story of the nomenclature of Amoeba since its discovery in 1755 is told by Schaeffer in his *Taxonomy of the Amoebas,* published by the Carnegie Institution of Washington in 1926. Of this great work it is sufficient for present purposes to say that Schaeffer gives the names *Polychaos dubia, Chaos diffluens,* and *Metachaos*

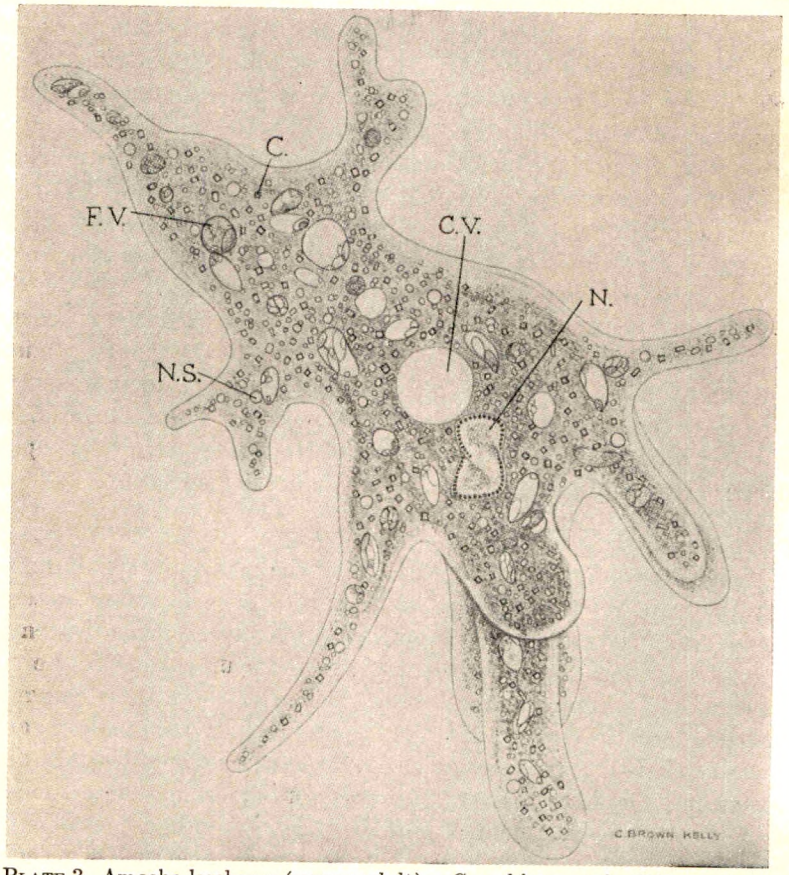


PLATE 3. Amoeba lescherae (young adult). *C.*, cubic crystal; *C.V.*, contractile vacuole; *F.V.*, food vacuole; *N.*, nucleus; and *N.S.*, nutritive spheres.

discoides to the three above-mentioned creatures, but the word Amoeba has become too firmly embedded in popular literature to be discarded. and Schaeffer's nomenclature has not been adopted, most workers still using the generic name amoeba.

One exception, however, is *Chaos chaos* the giant, many-nucleated Amoeba which some people identify with Wilson's *Pelomyxa carolinensis,* and which is becoming increasingly popular in England, though I am not aware that it has been discovered in British habitats. A false claim

for its occurrence was due to the fact that it was confused with the multinucleate *Amoeba nobilis*. The latter, however, is much smaller—in fact it is not much larger than *A. proteus*.

Amoeba dubia (*Polychaos dubia*) feeds largely on diatoms and is therefore different from *A. proteus* Y (*Chaos diffluens*) and *A. discoides* (*Metachaos discoides*) which do not eat diatoms. They consume ciliates, flagellates, rotifers, etc. Two other large amoebae since discovered and described have been named respectively *A. lescherae*, *A. kerrii*, the former belonging to Schaeffer's genus Chaos, the latter to Metachaos.

Jepps has made our narration an easier task by proposing the general name 'Proteus-group of Amoebae' for all those large types which feed on ciliates, etc., bear a general likeness to each other, and have a discoid nucleus.

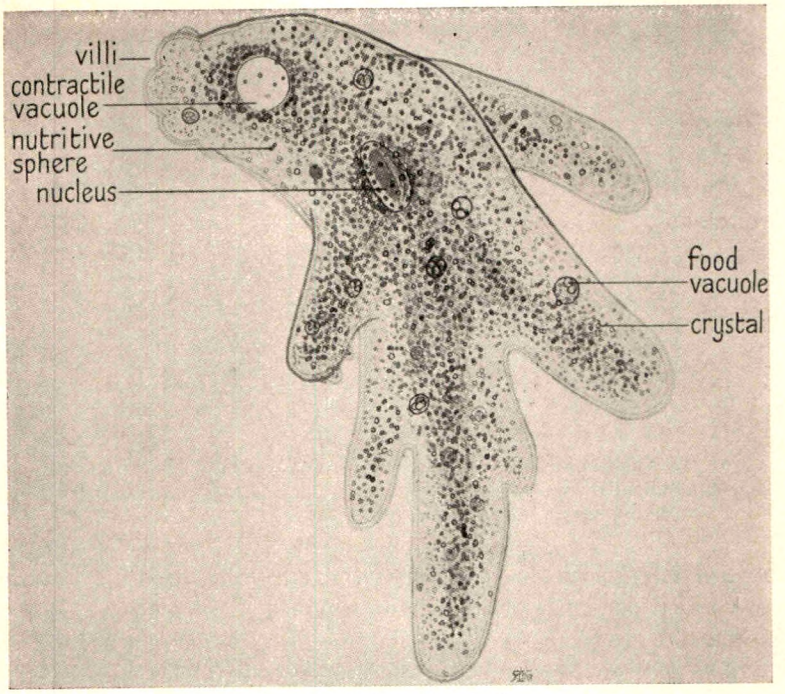


PLATE 4. Amoeba kerrii.

It seems advisable here to go into the story of the discovery of *A. lescherae* and of how it can be distinguished from *A. proteus*. It was in 1938 that one began to question whether or no all the amoebae that had been called *A. proteus* were really all alike, i.e., true *A. proteus*. After a prolonged investigation it was concluded that a new species could be sorted out. This was described and named *A. lescherae*. It is of importance to emphasise this fact, for, in spite of the availability of this knowledge, many workers are using *A. lescherae* under the

impression that they are dealing with *A. proteus*. The differences between the two may be usefully tabulated here:

A. proteus	*A. lescherae*
1. Creeps on a substratum when feeding.	1. Can feed when attached to floating vegetation.
2. An inhabitant of the surface-rich mud on the bottom of ponds and slow moving streams. Captured by skimming off this mud.	2. An inhabitant of fresh water vegetation, therefore captured by collecting weeds like elodea, water crowfoot.
3. Divides by mitosis when attached to a sub-stratum	3. Can divide by mitosis when floating on weed.
4. Has coffin-shaped crystals.	4. Has cube-shaped crystals.

A. lescherae is slightly smaller than *A. proteus*, grows more quickly, and has a tendency to form a few very large and most conspicuous cubic crystals* when its metabolism is not quite healthy, and, as will be

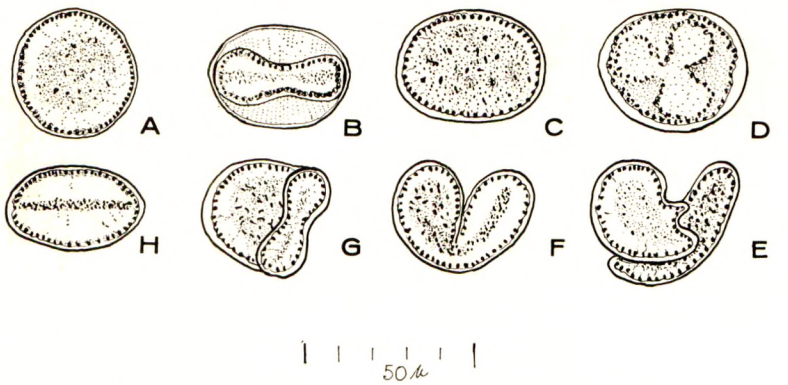


FIG. 1. Nucleus of the Proteus-group of Amoeba. Note the various shapes assumed by the nucleus as it grows older, and is rolled along by the streaming endoplasm.

explained later, is not so easily recovered from a dried-up culture. I have verified personally that Turtox supplies *A. proteus*.

As has already been stated, the diatom-eating *A. dubia* is not included in Jepps' "Proteus-group." In mitosis its metaphase, described by Carter in 1913, differs from those of the Proteus-group which were investigated and described at a much later date (1944–1947). It will be remembered that Schaeffer's distinction of *Chaos diffluens* from *Metachaos discoides* was based on his observations of their cytoplasmic behaviour, presence or absence of longitudinal folds in the ectoplasm. *A. lescherae* possesses these folds not shared by *A. discoides* nor by *A. kerrii*. It was a highlight when, years afterwards, it was discovered that

* A preliminary study of these crystals kindly undertaken by Professor Stoker and Dr. Wildy in the Institute of Virology, Glasgow, has shown that virus particles are not attached to them. The study, however, continues.

A. lescherae and *A. proteus* with longitudinal folds in the ectoplasm have, during mitosis, a spindle-shaped figure, while *A. discoides* and *A. kerrii* with no longitudinal folds have a barrel-shaped figure.

A Startling Ratio

Waywardness once more comes to one's mind when a most disconcerting contrast is noted in the 'Proteus-group of Amoebae.' Mitosis

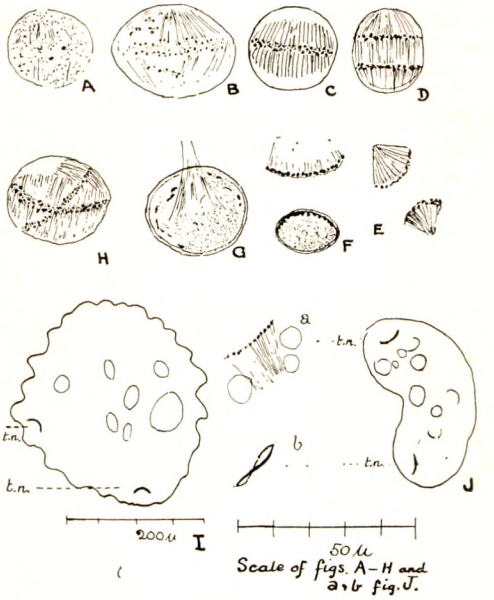


FIG. 2. Mitosis in amoeba lescherae. *A*, early prophase; *B*, early metaphase; *C*, metaphase; *D*, anaphase to early telophase; *E*, telophase; *F*, late telophase, one of the daughter products showing early signs of reconstruction of daughter nucleus; *G*, reconstruction of resting nucleus; spindle fibres still present; *H*, multiple division—two metaphase plates; *I*, outline of a stained amoeba showing position of the telephasic nuclei (*t.n.*), and *J*, ditto, *a* and *b* enlarged drawings of respective telophase nuclei.

of the nucleus is completed in from fifteen to twenty minutes in creatures whose life-history occupies from 4 to 6 months, roughly a ratio of 1 : 13104 in *A. proteus*. The answer to the question why involves a long research.

Commercial Supplies and Laboratory Cultures

A purely utilitarian reason prompted the earliest Amoeba researches in Glasgow University shortly after the turn of the century. It had been found difficult in the Continental and British Universities to supply the

undergraduates with living material. Commercial supplies were almost negligible. Laboratory attendants had too much other work to perform. Lecturers were very busy and inadequate in number and often unable to go out into the country to procure the living material.

Such, however, was the enthusiasm which heralded the introduction of practical laboratory work for the student in all science teaching (including pre-university level) that lecturers were quite willing to spend the hours following the dawn tramping the countryside for the collection of their biological specimens. The sources of amoeba, however, fluctuated: A journey to a pond once fruitful was liable to be a disap-

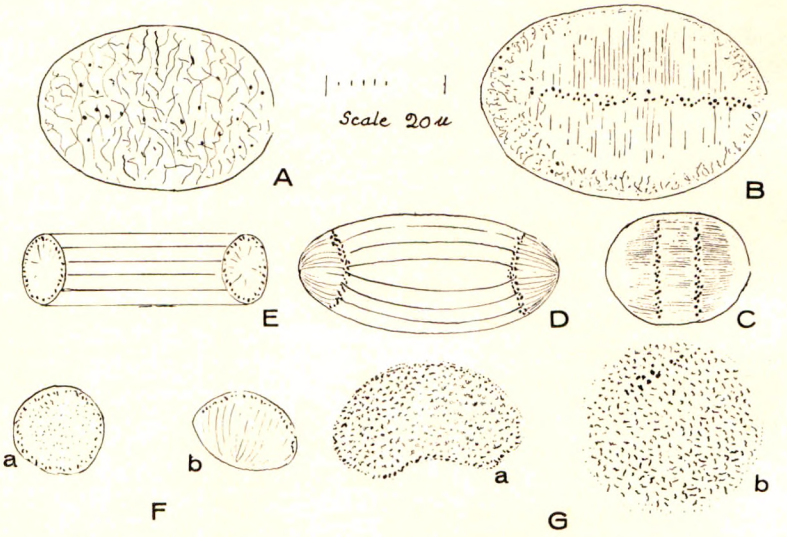


FIG. 3. Mitosis in *A. kerrii*. *A*, early prophase; *B*, metaphase; *C*, early anaphase; and *D*, early telophase (note barrel shaped spindle); *E*, late telophase; *F*, daughter nuclei; and *G*, reconstruction of the daughter nuclei.

pointment at another time. Research on ways and means of growing amoeba in the laboratory was an obvious necessity. Hope ran high when Drs. Allen and Nelson of Plymouth, in 1910, succeeded in growing salt water diatoms in Miguel's chemical solution. *A. dubia* fed on diatoms—the problem seemed easy of approach—cultivate freshwater diatoms and feed them to *A. dubia*. But, even now, in this year of 1962, a perfectly controlled method of growing freshwater diatoms in large quantities has not yet been worked out. So *Amoeba dubia* has never become a commonplace for any biological class.

I am unaware of the situation in U.S.A. at this time and of when dealers put Amoeba supplies on the market. Here in Britain it was a Birmingham naturalist, Mr. Bolton, who provided classes with small tubes of amoeba, and, as has already been narrated, his so-called *A.*

proteus consisted of *A. dubia* when he used one locality for his collecting, and *A. proteus* when he used another. He discontinued his services at the outbreak of the 1914 War.

It became incumbent upon some biologist to fill the breach left by Mr. Bolton's resignation. As a result of the War, demands for amoebae were augmented a hundredfold. The work was undertaken at Notre Dame, Dowanhill, and by using a technique similar to the one described later under a heading "A Dominant Species in its Ecological Niche," many universities and educational institutions of every category in Britain, and a few in France, were supplied from cultures grown in Notre Dame.* Incidentally too, it may be recorded here that specimens of *A. lescherae* may also have been disseminated, because, in those early days, it had not been recognised as distinct from, yet similar to, *A. proteus*.

During the course of this war work, and for many years after 1918, the demands for supplies being so great, numerous cultures had to be set up, not only because of the amount of material required but for another reason. The cultures underwent what were called, in the then prevailing ignorance, "Depression Periods." A culture luxuriant for amoebae was followed by a period during which there were none. After a varying period of time—4 to 6 months—the same culture would be flourishing again. There was a rhythm: adults—depression—adults. As time went on it became noticeable that, besides adults, could sometimes be seen small amoebae, just visible under a No. 3 objective (18 mm). It also became evident that, in a culture recovering from its depression, the amoebae were more attractive in appearance, spread out on a slide more easily, moved more quickly, in contrast to those amoebae taken from a culture due to become "depressed." In these, the cytoplasmic inclusions were bulky, the amoebae more lethargic in movement, and the living nucleus not so easily seen by the student using them. A consultation of the "Field Book" reveals the entry for the presence of the

* In addition to giving *Amoeba proteus* to our comparatively near neighbours we also got them established in Christchurch University College, New Zealand and later, in University College, Jamaica.

When the late Professor Percival, who had been accustomed to using our material, took the Chair of Zoology in Christchurch he wrote to ask if amoeba could be given to his friend, the Captain of a cold storage boat on its way out to New Zealand. A large "breffit" stocked with a good culture, under his care survived the long voyage, and the culture was confided to the Laboratory Technician, a Mr. Ernest Stocker, who maintained it efficiently and became a distributor to the surrounding colleges.

When an exhibit of Mitoses in *A. dubia* and the Proteus group of Amoeba was being shown to the Members of the Association of Experimental Biologists meeting in Glasgow 1946, one of the Visitors, a New Zealand Graduate, told us with great pride, of Mr. Stocker's success, and was quite thrilled to hear that the ancestors of his amoebae had come from Glasgow.

It was Dr. Jessie Rankin, who had spent a day in the Notre Dame Laboratory, and was Sir d'Arcy Thompson's Assistant, that asked us to give her amoeba supplies when she became a Lecturer in University College, Mona. Having made many attempts to send material by air mail we got them through successfully in a large glass bottle carried on a cargo boat.

small amoebae mentioned above. They heralded an onset of renewed luxuriance of young adults which, as has already been stated, are more suited for beginning students.

The Advent Of Tetrahymena

Mass cultivation of Tetrahymena and of other organisms palatable to adult amoebae of the "Proteus-group" has made possible the production of mass quantities of these creatures required by research workers in protein chemistry and by biology-teaching requirements. This luxuriance is due to the fact that, when adult, an amoeba divides into two, by fission every 24 hours at room temperature, the nucleus having already divided by mitosis. This stage in the life cycle is known technically as the Fission Stage. In its fission stage, therefore, the elusive amoeba has been completely conquered. It has become a most useful biological tool.

Just recently, a notable advance in the understanding of cell locomotion has been made by using *A. proteus*. Bell and Jeon have shown that heparin causes the formation of pseudopodia in Amoeba, a discovery that is of great importance.

Natural Habitat Versus Laboratory-Controlled Environment

The solving of the problem of mass production of adult amoebae in Britain during the 1914–1918 War and the more refined methods now in use owing to the production of the various food extracts now on the market, made all the more conspicuous the lack of knowledge of the private life of the large amoebae. So far there were two proved facts (1) the occurrence of depression periods, and (2) the presence of the small amoebae that heralded the recovery of a culture after its period of depression.

An attractive research on further events in its life-cycle as lived in its native haunts proved to be a lengthy one because so completely unexpected. Systematic study of natural sources of supply was most disappointing. As has so often been emphasised, amoebae were here today, gone tomorrow. What became of them?

A Wonderful Catch

A most rewarding catch when specimens were fixed, stained, and sectioned revealed the occurrence and the secret of spore* formation described in the paper: "*Amoeba proteus*, some new Observations on its Nucleus, Life History, and Culture." Unfortunately, no subsequent

* Throughout this article I have followed Dr. Margaret Jepps in applying the term "spore" to the small reproducing bodies out of which the amoebulea hatch. Other "resting" stages enclosed in an outer covering and spherical in shape are referred to as "cysts."

catch has ever been made of amoebae all at that precise stage. In most collections it is always possible to find individuals undergoing sporulation. For readers unacquainted with findings recorded in the above-mentioned paper, it may help to epitomise them here in order that the waywardness of amoebae in 1962 may be better appreciated.

In a mature amoeba small chromatin masses escape from the nucleus, whose nuclear membrane is "dissolved" in places to facilitate their

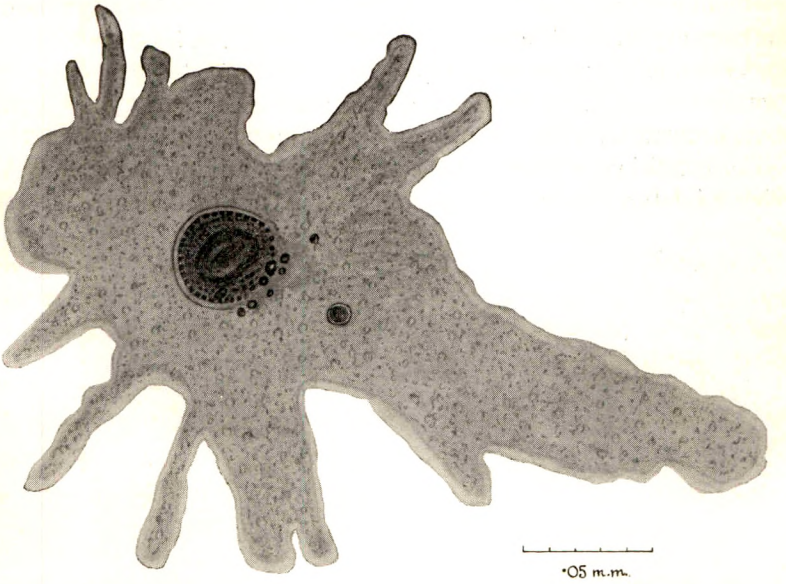


FIG. 4. Stained preparation of whole Amoeba drawn by Miss Margaret Curran, M.A., B.Sc. (from culture 3). A sub-culture (culture 10) having been secured, most of the material of culture 3 was fixed in warm corrosive acetic and used for the preparation of twenty-seven slides. On each slide there are several amoebae almost all of which are in the condition illustrated in the figure, i.e., in process of shedding "generative chromidia" from the nucleus into the cytoplasma. (*Q. M. J. S.*, 1924)

escape into the cytoplasm where, in association with the Nutritive Spheres (i.e., rounded masses of reserve food material enclosed in a membrane), spores are formed. The whole amoeba becomes packed with hundreds of these spores, each being surrounded with a membrane. Eventually, the spores escape into the fluid medium. All attempts to rear these spores to the adult stage in the then present state of knowledge failed.

We have already seen that modern mass production of adult amoebae by repeated fission was not revealing any information of what takes place between the hatching of the spores (10μ) and the attainment of the adult size (300 to 500μ). A technique for doing this had to be discovered.

A Dominant Species in its Ecological Niche

The hope of successfully establishing laboratory cultures simulating those in nature was raised by knowledge of the existence of habitats in which a single species, be it protozoan, coelenterate, etc., of a genus prevails to the exclusion of other members of the genus. An example of such an amoeba species occurring in a fish pond in Gartmore comes to mind where the beautiful camouflaged *A. radiosa* occurs in copious numbers as young, older and adult. No other amoeba has been captured, although a continuous search has been made. Decaying leaves, fish excreta, etc. supply the pabulum for the food chain from bacteria to fish. Into this chain *A. radiosa* hatches, is young, grows older, becomes adult, reproduces, in short, fits perfectly.

The conditions existing in the *A. radiosa* pond could be copied by using miniature "ponds" in the laboratory, thus facilitating the search

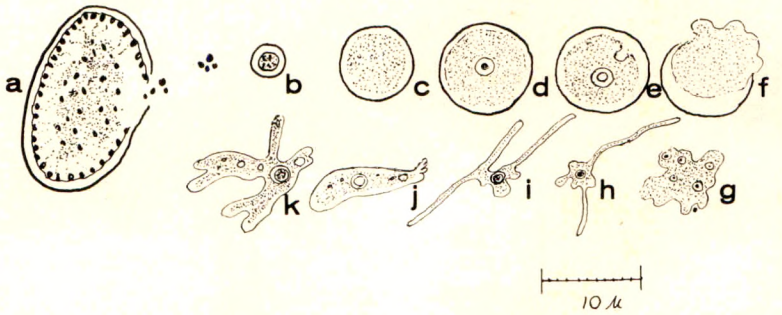


FIG. 5. Stages in the life cycle of a proteus group amoeba—chromidia escaping from nucleus, spore, differentiation of the nucleus in spore wall, excysting amoebulae, growth of amoebulae. (N.B. These drawings were made *before* discovery of the repeated encystment of the developing young.) *Cf.* Fig. 7.

into developmental stages. So, into Petri dishes were distributed sterile pond water, or rain water with amoebae and a supply of the organisms on which they feed along with a pabulum, e.g., hay water, wheat grains.

One result of using this technique has already been quoted: the discovery of the rhythmic cycle which marks the appearance and disappearance of adults.

That amoebae undergo a sporulation process had been discovered in the fortunate Milngavie catch of 1924. To amplify this knowledge, preparations for an intensive study of what was taking place during the "depression" period were made. Numerous Petri dish (4 in. diameter) "miniature ponds," prepared in a time sequence, of *A. lescherae* and *A. kerrii* were set up and subjected to outside temperature. Experience in outdoor collecting had made evident the fact that in late summer and early autumn adult amoebae were most abundant. Systematic

searching of these cultures revealed a whole sequence of sizes of these microscopic creatures; spores were recognised and seen to hatch into tiny amoebulae. These stages were measured and drawn to scale and so the spore was linked up with the adult. In the meantime, Sister Carmela had measured and sketched developmental stages in *A. discoides.*

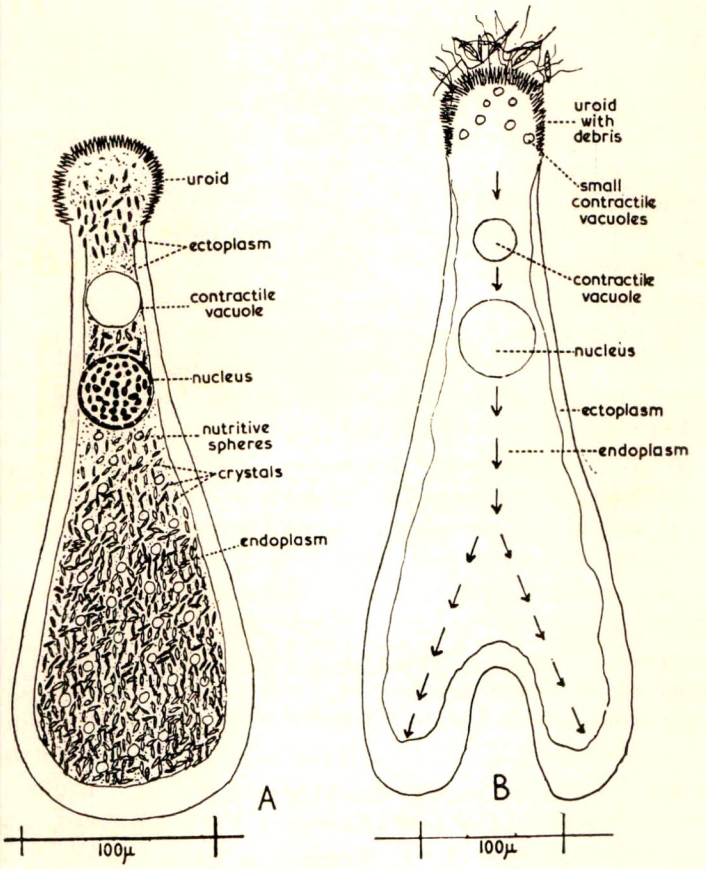


FIG. 6. *A*, a living *A. taylorae*, showing the uroid, contractile vacuole, nucleus, a great number of small crystals in the endoplasm, and a considerable number of nutritive spheres. *B*, outline of *A. taylorae*, to show debris attached to the uroid, the bifurcation of the anterior pseudopodium, and the direction (shown by arrows) of the cytoplasmic stream.

Before estimating the value of these findings a digression must be pardoned. It concerns the great contrast that exists between the appearance of the developmental stages of *A. lescherae* and *A. proteus* thus adding one more reason for the species validity of *A. lescherae.* In the latter, the "babies" are much more fragile looking, their pseu-

dopodea long and slender, and they have a tendency to float. The adult *A. lescherae*, when creeping, often becomes Y-shaped. This same tendency is foreshadowed in the immature individual.

A sad waste of good cultures may also be recorded here. In the early days of our inexperience, many good cultures of *A. proteus* were thrown away, because they contained these fragile developmental stages, now definitely known as *A. lescherae!* We were throwing away *A. lescherae* cultures!

Satisfying as they were from one point of view, these results so laboriously collected told us nothing of the great contrast in time taken to grow up and rapidity of fission, once the fission stage had been reached. There was a mystery. In the light of present knowledge, these drawings are seen to lack an essential. The discovery of this omission must now be narrated—it is connected with the apparently slow growth of the developmental stages.

Before presenting the recent work on the "Proteus group" mention must be made of *A. taylorae* a large free-living amoeba (420–500μ × 70 to 140μ) discovered and studied and named by Sister Carmela. Although, because of the absence of longitudinal folds in its ectoplasm she placed it in Schaeffer's genus Metachaos, its nucleus is very unlike that of the "Proteus group." Its diameter is 30μ; it is biconvex in shape and therefore circular when viewed from above and elliptical in side view. It has no Karyosome, the chromatin masses are large and conspicuous and scattered evenly through the nuclear substance. It is very full of needle-shaped crystals and therefore appears opaque in reflected, and dark in transmitted, light. Its movements are very characteristic by means of one large pseudopodium which when the creature alters its direction bends at right angles.

Bright orange food vacuoles appear in it occasionally, such as are found plentifully in *A. hugonis*. It can now be definitely stated that these are derived from ingested euglena. It will be remembered that Professor Pringsheim concluded these bright orange masses of pigment similar to the eye spot, which sometimes occur in *E. gracilis*, augured an unhealthy condition of the euglena. That fact does not prevent them from being very palatable to amoebae generally and their presence in the cytoplasm makes a gay picture for the student investigating them under the microscope. The golden colour is due to carotene.

A. taylorae has not yet been brought into cultivation in the laboratory and so the nuclear divisions and developmental stages of this most arresting amoeba await investigation. That is the reason why rather more details of structure have been given of this amoeba, unknown in the commercial world. They may attract a future investigator. It is to be regretted that the full life story of *Amoeba villosa* which has a similar habitat to *A. taylorae* and which was the first large amoeba to be de-

scribed fully by early naturalists who found it in pools where now stand the Medical Research Laboratories of Hampstead Heath, is not yet known.

Recovery of Adult Amoebae from Dried Cultures

The accidental drying up of a routine *A. proteus* culture was the means of a great advance on the road to unravelling the problems just stated. Instead of discarding the brownish dust-like powder left by evaporation in the container, her scientific curiosity caused Galbraith to ask "What will happen if I add water to this desiccated mass?" Her patience was rewarded by the reappearance first of "babies" and after some months of adults. She repeated the experiment with success and proved conclusively that *A. proteus* in many cases could be recovered from a dried-up culture.

Repeated efforts to induce *A. lescherae* to do the same nearly always failed. Very few positive results followed the trial. It will be remembered that this difference in behaviour was used as a criterion for separating out *A. lescherae* as a distinct species.

The importance of this discovery was enormous, for it provided a technique for securing developmental stages without the trouble of laboriously searching many cultures for any requisite stage. Briefly: If a small quantity of this powder be wetted and examined either in a Petri dish under a water immersion lens or, in the usual way, on a slide under a cover slip, there are seen, after a varying time—never longer than a week—amoebulae creeping about and feeding, but, wayward as ever, these "babies" very soon encyst. But more astonishing was the fact that these excysting amoebae varied very much in size. Obviously they were not issuing from spores. It was tantalising that all, great and small, encysted after a feed and stubbornly remained in that condition. What means could be taken to make them excyst and get on with their development, as they did in those successfully recovered *A. proteus* cultures described above?

Developmental Stages of Amoeba Obtained by Changing The pH of Their Culture Fluid

Another step forward in deciphering the cause of the lethargic conduct of developing amoebae was made, when a Petri dish (4 in. diameter) full of adult amoebae was subjected to a slight change of pH. The adults were all killed off! After a varying period of 2 to 8 days, however, an examination of the Petri dish proved a most rewarding experience! Developmental stages of all ages were present, as well as many unhatched spores. But once again, the unpredictable happened: They all went into cysts. Further examination revealed an alternation of encystment followed by excystment. On rare occasions, a population of adults

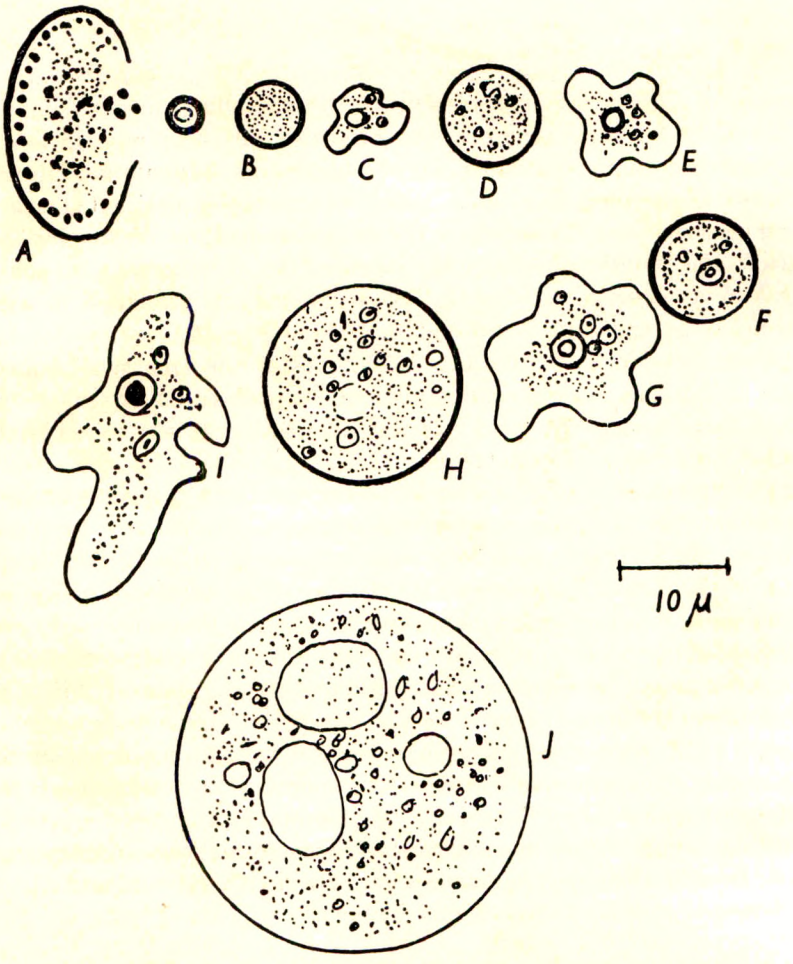


FIG. 7. Outline drawings of events in juvenile stages of proteus group of amoebae, drawn to scale. *A*, nucleus of adult amoeba—nuclear membrane absorbed where chromidia (= chromatin blocks) are escaping into the cytoplasm of a mature amoeba; *B*, a differentiated "spore"; *C*, amoebula hatched out of spore *B*; *D*, encysted amoebula —note minute food vacuoles; *E*, amoebula hatched out of Cyst I; *F*, encysted amoebula Cyst II; *G*, excystation, feeding and growing amoebula; *H*, encysted amoebula Cyst III; *I*, excysted amoebula; and *J*, large cyst. The amoeba which hatches out is adult in size, but very transparent because the nutritive spheres and crystals are so small. The nucleus is discoid.

resulted at the end of six months. On one occasion, an encysted juvenile hatched out again exactly two years later to the day, began to feed, and once more encysted. A fruitful field of research in these days of space travel would be to test the longevity of these encysted juvenile stages, both dried and in sealed vials.

Complexity of Amoeba Cultures

A perusal of what has been narrated above will furnish an explanation of why a dried-up culture of Amoeba yielded all stages of growth when fluid was supplied to it. Once more it must be emphasised that adult amoebae of the "Proteus group" do not encyst. An enumeration of what was actually present in the dried mass is: (1) spores (2) encysted stages of juveniles of all ages. Before the culture was dried, it would also have young and mature adults killed off by desiccation. It is important here to recall the fact that the adult "Proteus type" Amoeba does not encyst, but right up to this adult stage encystment occurs.

This knowledge gave an explanation of a most spectacular event recorded in its field book of one of the routine cultures. The reading was "No adults." A week later the entry was "A rich culture of young adults." The advanced stages of the developing amoebae had, for some reason, been all encysted, and then excysted as young adults shortly before the last examination recorded. A catch of Amoeba from an outside natural source would have the same complexities as those described above, so the young collector of Amoeba material is advised not to throw away his collection until he has given it a six months trial.

A prolonged study of the results of the investigations described above seems to warrant the conclusion not that juveniles grow slowly, but that they spend the greater part of their time in the quiet retirement of their cyst walls.

What evolutionary process has brought about this characteristic, so obviously a protective device against changes of environment?

Discussion

Adult amoebae in Nature, as well as in laboratory cultures, are rarely all of precisely the same age. If a pure-line culture be set up (i.e., a clone be initiated) some individuals, after fission has well begun, seem to lose step for no obvious reason, and so heterogeneity in age sets in. The optimum pH for adults is 6.6. Developing stages like a more acid medium. In view of this, it is not surprising to learn that the "Proteus-group" of amoebae have never been collected on a Keuper marl drift even though dozens of collectors working in widely separated locations during the course of ten years have searched systematically.*

It would seem then, that pH of the environment controls their occurrence. In Nature, a violent though temporary change in pH could

* I am grateful to Mr. R.F. Crowther for supplying me with this information.

conceivably occur. If the adults were killed, the developing stages would be saved by encysting for the duration of the danger time.

Variations in the population of the food organisms of amoeba are bound to occur as pabulum supplies fluctuate. Under starvation conditions the ability of the developing stages to encyst would clearly be advantageous. The effects of starvation on adult amoebae are well known. They can linger on indefinitely but eventually die. They do not encyst. One factor about which comparatively little is known is the life-cycle of the food organisms themselves. When the respective life-cycles of host and prey do not synchronise, developing amoebae carry over the resulting starvation period as cysts. Thus, encystment of host gives the prey a chance, so to speak, of "recovering."

It would be interesting to contrast the relative dangers of change of environment and food supplies obtaining in a lonely moorland with habitats nearer to urban districts. Temperature, too, must play a role in encystment. Still, when all these factors have been considered it is difficult to see why, in laboratory cultures, this tendency to long periods of encystment in the developing stages still persists and remains one more challenge of this wayward protozoan.

However, a hopeful research on this problem has been begun. A Petri dish full of encysted developmental stages was prepared. To this culture bacteria, which initiate the decomposition of wheat, were added. Without exception, the amoebae excysted and started feeding. Can encystment be warded off by systematic addition of bacteria? Further research is indicated.

A Factor to be Considered in the Identification of Amoeba

In the light of what has just been discussed, a reader of AMERICAN SCIENTIST may well ask "How did the collector of the amoeba which was named *A. hugonis* know it was not a development stage of the 'Proteus-group'?." A Glaswegian would very soon have found the answer: it would not tolerate the addition of Glasgow tap water* when being examined microscopically under a cover slip. Glasgow tap water comes from the lovely Loch Katrine of the Trossachs, made famous by Sir Walter Scott. It is a moorland loch and therefore distinctly non-alkaline. It can be used with impunity for all stages of the "Proteus group."

To continue—this first decision's accuracy may, however, be checked by a breeding test. Supply a specimen of *A. hugonis* with food in a suitable environment. If it proceeds to produce a clone, as it did, then

* Glasgow tap water is well known on the Continent for its palatability in diluting whiskey. Large carboys of it are exported and distributed into small bottles sold together with the whiskey. Manchester tap water is very similar to that of Glasgow and has long been "exported" to Cambridge for culture work.

it is already an adult capable of undergoing fission. No developmental stage can produce a clone. There were of course other criteria contributing to the conclusion that the new Amoeba had species value.

Sex in Amoeba

In no phenomenon as that of sex has the "Proteus-group" aroused more diversity of opinion. In my long years of acquaintance with and research upon this elusive group have I ever discovered a sexual cycle. All the elegant work done by Dobell, Bishop and McConnachie in this country on Entamoebae goes to show the absence of a sexual cycle in these parasites.

Droop startled the world of amoeba research when he proved conclusively by his ingenious and beautiful indirect method the existence of a sexual cycle in a diphasic amoeba. This group is, however, more nearly related to the Mycetozoa where the sexual cycle has long been known.

Some thinkers give to flagellates a priority over amoebae in that they were the first protozoa to appear at the beginning of life. Are the diphasic amoebae with their flagellate forms more primitive? Are the large "Proteus-type" amoebae degenerate? Is there something analogous to sex in the spore formation mentioned earlier in this article: the nutritive sphere corresponding to the food-carrying function of the macro-gamete, the chromatin material to the micro-gamete, in the production of a zygote? Will the waywardness of Amoeba ever be entirely conquered?

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PINEAL ORGANS: PHOTORECEPTION SECRETION, AND DEVELOPMENT

By DOUGLAS E. KELLY

THE brains of animals contain many cellular aggregations or organs for which functions have never been described. For many of these components, functional obscurity can be attributed to minute size, remote location, transient existence, ill-defined anatomical relations to other brain regions, or only recent discovery. Pineal organs, however, defy all these premises, having been meticulously studied at all anatomical levels over a period of more than six centuries. To date, every effort to arrive at some final physiological characterization has resulted in a greater or lesser degree of frustration. It is not surprising that numerous workers have abandoned the search, many of them conveniently labeling the pineal organs as evolutionary vestiges of doubtful functional significance.

Since this essay does not report any new or singularly unique hypothesis, the reader might question the rationale of restating and re-examining the problem. My motive is simple. Pineal organs, while still insufficiently understood, have been the object of recent studies which strongly imply that they do function significantly and in a variety of ways among vertebrate animals. The time seems proper to replace with a more modern viewpoint the pessimistic, brief, and often confused surveys adorning the pages of many current texts.

Excellent reviews [1,2,3,4] are available which adequately summarize the long history of experimental probing and speculation on the origin and function of pineal systems. So at the risk of historical negligence, I should like to devote this article primarily to several aspects which have dominated the more recent scene.

The notoriety surrounding pineal organs is in no small measure due to two historical circumstances: that Descartes conferred upon these knobs of tissue the role of "seat of the soul"; and that the eyelike appearance of pineal derivatives in the median dorsal skin of the heads of some reptilian species has long suggested a photoreceptor function. This latter fact forms the crux of the present discussion. I leave the former to philosophers.

We can begin by briefly relating some basic anatomical, embryological, and evolutionary considerations.

It is essential to recognize any pineal organ as but one type of elaboration of the dorsal diencephalic brain roof. Here it is at least topographically related to several other structures whose roles are likewise unknown. Conceivably, functional relations may exist as well. All vertebrates, for example, possess some type of subcommissural organ lying just ventral

and posterior to the pineal organ. Here are found elongated secretory cells which elaborate a copious mucoid material. In all but the highest vertebrates most of this material is released into the cerebrospinal fluid where it is rapidly condensed into the long flowing fiber of Reissner and, as such, travels the length of the spinal canal [5]. The function of the material remains largely a mystery, but in many vertebrates positional intermixture of pineal and subcommissural cells can be detected, and neural connections between the two organs also appear likely.

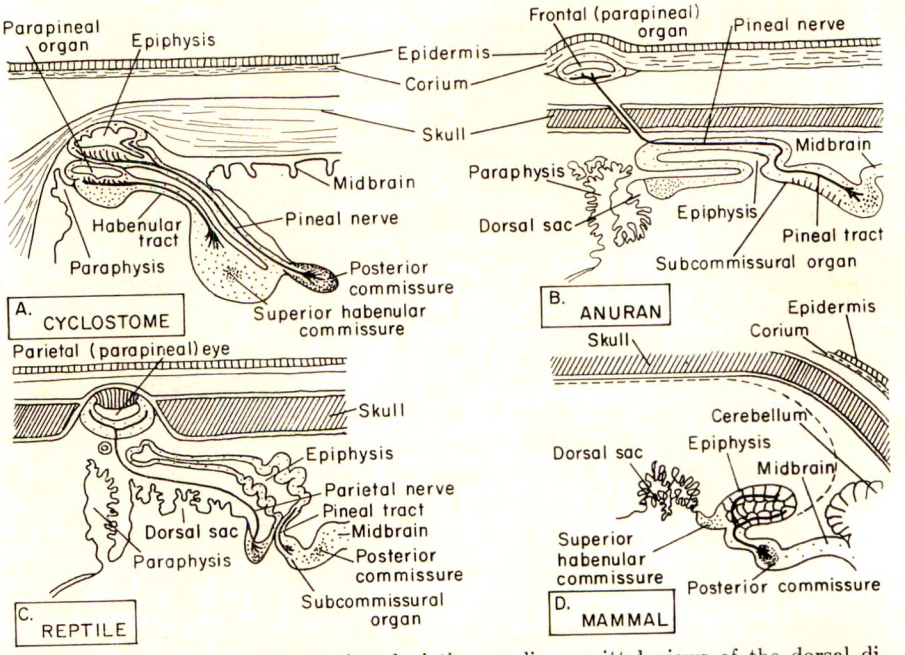


FIG. 1. Schematic representation depicting median sagittal views of the dorsal diencephalic region in several vertebrates. Components of the pineal system and related organs are shown. Revised from Studničĥka [7].

Preferably one speaks in terms of "*pineal systems*" rather than *a* pineal organ since in both phylogenetic and embryological senses, pineal organs appear to be double structures. It is well established that two primordia constitute the embryonic beginnings of the pineal mass [6], each derived from the lateral edge of the anterior embryonic neural plate. Curling of the plate and formation of the neural tube causes the two primordia to be repositioned side-by-side and finally to fuse into a single dorsal pineal presumptive area. Then a second doubling may occur. This results in the more obvious phylogenetically double pineal systems achieved in adult brains of the majority of lower vertebrates (Fig. 1) [7]. In these cases one component is usually saclike, lying atop the diencephalic roof with its cavity nearly always connected to the

brain ventricle via a narrow orifice. This portion is most accurately termed the pineal organ proper, or epiphysis cerebri. More superficial in location is the parapineal (or parietal) component which seems to originate as an outpouching from the pineal organ (e.g., frogs), or as a separate diverticulum from the brain roof (as in some lizards). In any event, the parapineal component, when present, is most often stationed just beneath the skin of the head, and in many lizards may be highly differentiated into a small eyelike configuration (the parietal, parapineal, or third "eye"), complete with lens and retina [8,9]. A less specialized version lacking the lens is common to frogs and is termed the frontal organ (or Stirnorgan). About the most primitive existing pineal system is the arrangement of the two components found in lampreys. Here they are nearly identical, have separate stalks, and are positioned in tandem with the pineal organ (occasionally referred to as a parietal eye) being the more superficial [10]. None of the above components should be confused with the paraphysis, a possible secretory outpouching of the brain roof lying anterior to the pineal system and highly developed in some vertebrates (e.g., amphibians).

At this point, the reader may well suspect, as have many others, that the adult pineal and parapineal organs could simply represent direct outgrowth of the paired embryonic primordia; a shift from bilateral to tandem positioning occurring during development. In some forms the innervation pattern of the two components suggests that they may have developed in this way, but unfortunately direct embryological evidence is not yet conclusive. On the other hand, since fossil skulls from several primitive fishes display appropriate bilaterally placed paired indentations, it does seem likely that paired adult pineal organs may once have existed side-by-side rather than in tandem [11].

This is not to say that some type of dual system has persisted in adult vertebrate brains in general. It hasn't. Virtually all mammals, birds, most fishes, and the tailed amphibians display but a single pineal organ, or epiphysis. At the same time one finds many variations in these single organs, and at least some birds and tailed amphibians do show indications of a parapineal lobule at the anterior margin of the epiphysis. The organ as a whole is very small and rudimentary in procavians, elephants, opossums, and several whales. It is possibly lacking in dugongs and certainly missing in all crocodiles, alligators, armadillos, sloths, and anteaters examined [12,13].

Good reasons exist to suspect that a spectrum of cytological specializations runs through the total array of gross morphological types we have come to know. Toward one end of the spectrum we can now recognize pineal organs characterized by cells of photoreceptive configuration. They are characteristic of the lower vertebrates (primitive vertebrates, fish, reptiles, and amphibians) and are altogether unlike the more

closely packed "parenchymal" mass of tissue that has evolved at the other end of the spectrum as the single organ of mammals, and probably birds as well.

Without exploring for the moment aspects of the situation which might link these two cytological types, it is useful to emphasize the distinctive properties which characterize each. I shall refer to these types as *photoreceptor* and *parenchymal* respectively, realizing that we have yet to demonstrate unequivocal photoreception on the part of the former, and that the term *parenchymal* says little, if anything, other than to denote an historical addiction to its use.

The Photoreceptive Pineal Organ

The concept of pineal photoreception, at first a matter of speculation, was placed on a firm descriptive basis by anatomists of the late 1800's, and particularly by meticulous descriptions [e.g., 14] of the pineal apparatus in the primitive lizard-like Tuatara (*Sphenodon*). Here the parapineal component displays an obvious similarity to the arrangement of the typical vertebrate eye. It was ultimately learned that a number of the lizards (see review of Stebbins and Eakin [15]) and at least one fish [16] also possess highly evolved parietal (or parapineal) third eyes. Moreover, investigators have long been aware that many pineal systems which display few gross eyelike properties are often accompanied by a striking transparency of the scales, skin, or cranial elements overlying them [15,17,18]. Young African clawed frogs, for example, maintain a pigment-free transparent fleck above the developing frontal organ [19], and the salmon possesses a veritable "pineal window" [20] of transparent tissues which could serve to admit light to the epiphysis located rather deeply in the head.

What wasn't fully realized during the early years is that cells found lining the lumina of pineal components in a wide variety of lower vertebrates possess cytological similarity to cells having proven visual capabilities. The possibility of photoreception is, therefore, not limited to those pineal derivatives that grossly resemble a vertebrate eye. Indeed, we now know that the deeper-lying epiphysis as well as the parapineal components in amphibian and lizard species have photoreceptive elements during part or all of the life cycle.

At least three pineal cell types, photoreceptors (or sensory cells), supportive cells, and ganglion cells, can be described in amphibians, fish, reptiles, and more primitive vertebrates. Of these, the only cells for which we presently have any degree of understanding are those which we cautiously call photoreceptors.

In the knowledge of our most recent evidence concerning the photoreceptors, one becomes increasingly impressed with studies reported by Nils Holmgren [21,22] in 1918. Although others before him realized

that the cavities of pineal organs often contain protrusions from the cellular walls of the organ, Holmgren provided exquisite illustrations of these, the outer segments of "pineal sensory cells." He could discern that, in the frog and the dogfish, the sensory cells each possess a basal nucleus and a more apical dense region of cytoplasm (termed the ellipsoid) which culminates in the protruding outer segment. Accompanying these cytological descriptions were careful reconstructions of the nerve supply to the pineal system and its involvement with ganglion cells. Holmgren speculated that neurones supplying the main pineal tract are axons of the ganglion cells and he implicated dendrites of the same cells in some sort of synaptic relationship with the pineal sensory cells.

These were exquisite descriptions indeed in view of the times and techniques available, and only recently have they become fully appreciated. Particularly is this true in regard to one difficult and often misunderstood facet of Holmgren's work. Holmgren had the acuity to note and describe a high degree of variability in the configuration of outer segments—ranging from thin, striated fingers extended into the pineal lumen, to types with knoblike endbulbs, or balloon-shaped sacs. Still other sensory cells displayed no visible outer segments or only very small bulges. While admitting the possibility that the variations were due to artifacts derived from killing and preservation, Holmgren postulated that they might well represent stages in a "regeneration cycle" involving the production, casting off, and rebuilding of outer segment components. Subsequent workers have sometimes taken this to mean secretion *per se*, and hence Holmgren has occasionally been misinterpreted as an advocate of a gross secretory function for lower vertebrate pineal systems.

Ultimately this work and its interpretations not only fed fuel to the controversy over pineal secretion (which, as we shall see presently, was spurred by investigation of mammalian pineal systems where the "*parenchymal*" configuration is in distinct contrast to that of the lower vertebrates), but it also cautioned later workers that many of the delicate structures observed in these systems could in reality represent artifacts.

The caution has persisted to the present time when it has been possible to explore the problem by electron microscopy. With the improvement in resolution and preservation provided by this technique we are beginning to separate fact from artifact, and it may turn out that Holmgren's idea of a regeneration cycle is still valid.

The first clear electron microscopic images of photoreceptive pineal tissue were obtained independently by Steyn [23,24] and Eakin and Westfall [25,26], both studying the parietal (parapineal) eye in lizards. Eakin [27,28] and Eakin and Westfall [29] soon extended the investigation

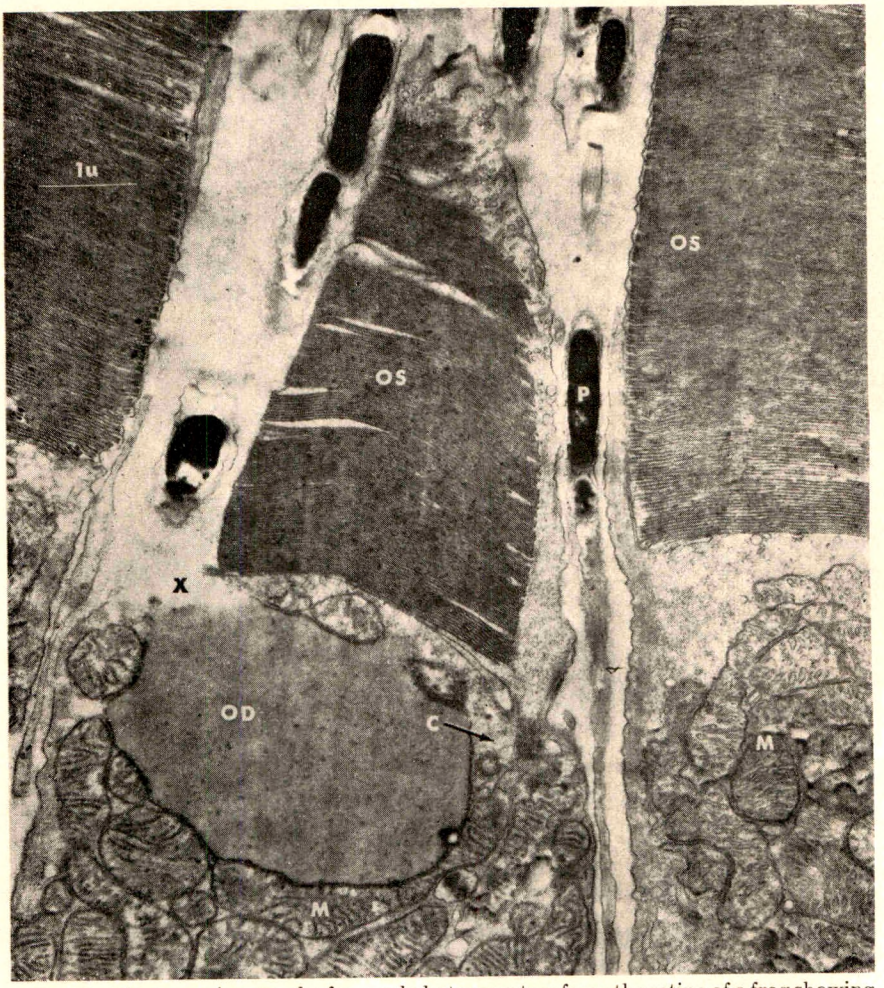


FIG. 2. Electron micrograph of several photoreceptors from the retina of a frog showing inner and outer segment regions. Cone cell (center) displays lamellated membranous character of the outer segment (OS), and closely packed mitochondria (M) around an oil droplet (OD) within the inner segment. Note centrioles (C) and ciliary apparatus located at the narrow junction between the two segments. Similar membranous components are found in rods shown to either side of the cone. Processes containing melanin granules (P) extend between the outer segments from the pigmented epithelial cells of the retina (out of view). × denotes an area of preparation damage. Buffered osmium fixation and alkaline lead stain. 13,800×.

to the frontal organ of the tadpole and most recently pineal organs of lampreys. Both authors have since examined the fine structure of the lizard epiphyses [28,30]. We [31] have also obtained electron micrographs of the adult epiphysis and frontal organ in the frog, *Rana pipiens*. One thing seems quite clear from these studies. Parietal eyes, frontal organs, and epiphyses in these forms all possess a large number of cells



FIG. 3. Electron micrograph showing a pineal photoreceptor extending into the lumen (L) of the epiphysis of a frog. The outer segment (OS) is composed largely of lamellated membrane sacs similar to those seen in retinal photoreceptors. Numerous mitochondria (M) occupy the ellipsoid (which corresponds to the retinal inner segment) and ciliary components (C) are also found in the narrow junction. Buffered osmium fixation and alkaline lead stain. 13,800×.

which closely resemble in their fine and gross structure the known photoreceptive rods and cones of lateral eye retinas. These are the protruding pineal sensory cells which Holmgren so meticulously described.

Figures 2, 3, and 4 (from the frog) illustrate the similarity between visual and pineal photoreceptors. In both cases the dense ellipsoidal mass, apical to the nucleus, is packed with mitochondria; suggesting the capacity for a high level of oxidative metabolism. The outer segment in both photoreceptors appears to be a ciliary derivative, possessing

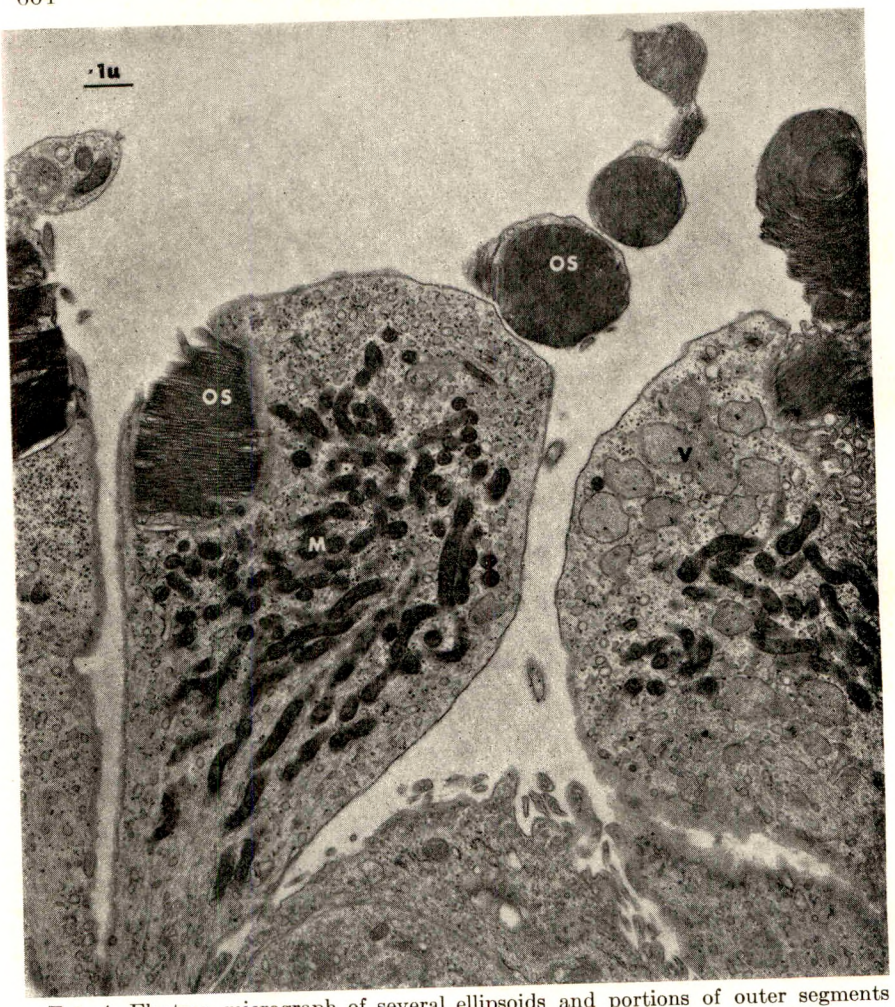


FIG. 4. Electron micrograph of several ellipsoids and portions of outer segments from the frog epiphysis. Note dense population of mitochondria (M) and frequent large, moderately dense granular vesicles (V) which characterize ellipsoidal regions. Buffered osmium fixation and alkaline lead stain. 7060×.

centrioles at its junction with the ellipsoid (inner segment) and a typical array of nine ciliary fibrils coursing from the centrioles distally within the protrusion. In both systems the cell membrane has been elaborately folded in upon itself to form an intricate array of stacked lamellae or sacs along the length of the outer segment. It appears that, in addition to the lamellar pattern of folding, each infolding or sac may be lobed or may have secondary, lateral outpouchings which appear as vesicles or tubules in some planes of section. There are differences to be found in the images of pineal photoreceptors obtained to date in comparison to the numerous studies on vertebrate retinas [e.g., 32,33,34,35,36,37,38,39],

but the basic similarities and their photoreceptive implications seem clear.

An adequate histochemical or biochemical analysis of pineal photoreceptors has not yet been obtained, but the brief reports that are available [e.g., 40,41] suggest that both similarities and differences from visual cell histochemistry probably exist. The matter needs further effort if a complete photoreceptive picture is to be obtained.

The current case for pineal photoreception in these forms does not rest only on electron-microscopic evidence. It has long been observed [e.g., 42,17,43 and earlier workers] that aquatic vertebrates (particularly those with exposed or translucent pineal regions) display altered behavior or pigment patterns in response to light when the pineal apparatus is removed or shielded. Similarly, pinealactomy, parietal eye removal, or shielding in lizards leads to behavioral changes related to thermoregulation [44,15,45], an effect which may involve function of the thyroid [46,47,48]. A similar, though less clear, pattern has been reported for young tadpoles [49], while the Tuatara has not yet been observed to respond in the same fashion [50].

If photoreception is being achieved by pineal cells, cytological evidence of this activity might be detected. In point of fact, it has been reported [51] that pineal cell nuclei in the lamprey change position in response to light and dark. Similarly, in the lizard parietal eye [41] glycogen mobilization within the lens and lumen, and a lumenward shifting of pigment-containing cells has been observed during light adaptation. Such changes have not, however, been seen in young newt epiphyses after treatment with light, dark, or other physical or chemical factors [52]. Few pigment cells and no lens are present in this system though, so the difference is understandable.

Within the past year two physiological investigations have been reported which further substantiate the photoreceptive case built to this point on behavioral and morphological grounds. A pattern of electrical activity detected by Miller and Wolbarsht [53] in the retina of the chameleon parietal eye corresponds to onset, prolongation, and cessation of illumination of that organ. Similarly, Dodt and Heerd [54] recorded impulses from a cut nerve tract leading from the frog frontal organ after exposure to long wavelength light or to darkness. White light or shorter wavelength treatment inhibited electrical activity. An additional inhibitory response to light of all wavelengths was detected in some of the animals tested. Taken together, the evidence implicates the frontal organ as a wavelength discriminator.

While examining the photoreceptive fine structure of amphibian pineal cells, we have become impressed with the array of variations which exist in the size and morphology of the outer segments (see Figs. 4 and 5). Some outer segments are small, seemingly immature or degenerate, with

little membranous organization, while others are greatly expanded. In addition to typical rod- or conelike patterns, some outer segment membranes appear as tubular arrays, tight concentric layers or large, empty, myeloid-like whorls of lamellated membranes. Interesting,

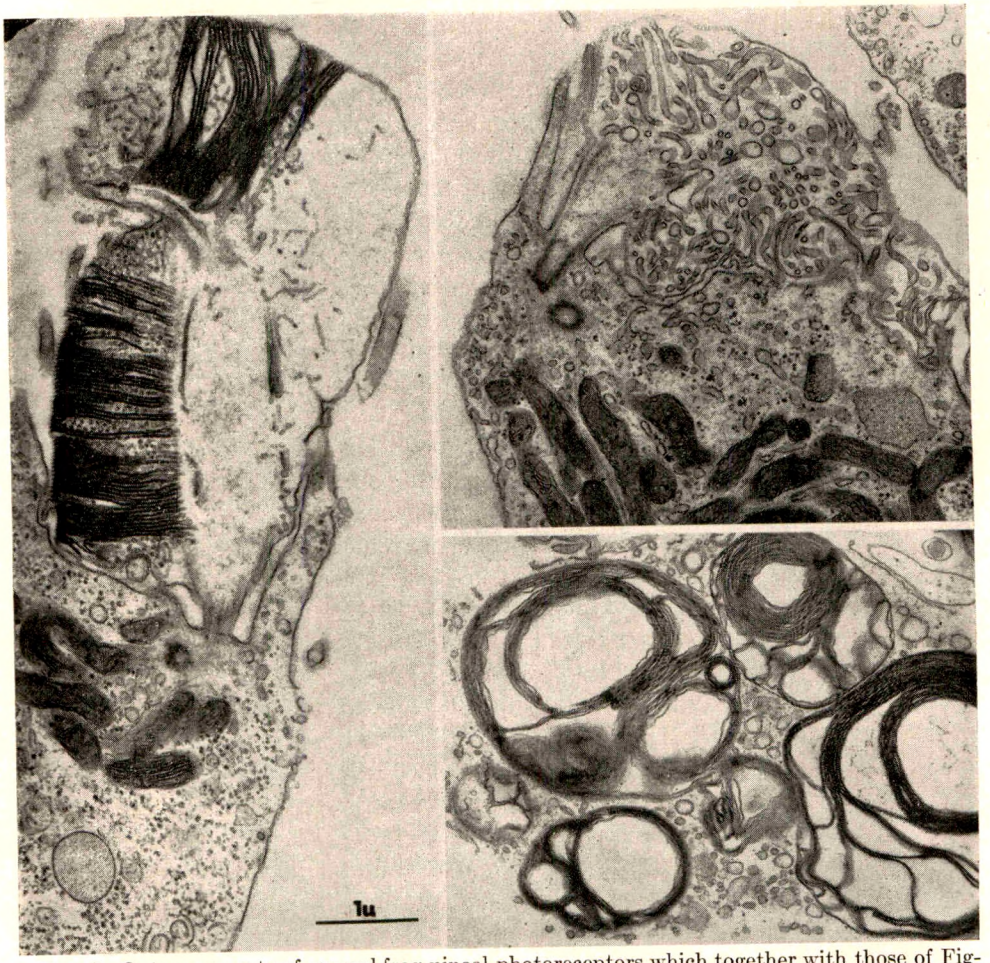


FIG. 5. Outer segments of several frog pineal photoreceptors which together with those of Figures 3 and 4 illustrate the range of variation observed in the membranous components of fixed specimens. Assuming that these variations are not preservation artifact, they may represent stages in a regeneration cycle. The possibility is discussed in the text. Buffered osmium fixation and alkaline lead stain. 13,800×.

and perhaps significant, is the fact that dystrophic degeneration of retinal photoreceptors produces patterns of variation [35] not unlike those observed in normal adult amphibian pineal organs. Since it is known that similar variations (especially the myeloid whorls) can be duplicated by post-mortem degeneration, it is not yet completely possible to elim-

inate killing and preservation procedures as the cause of these outer segment patterns. However, since similar variations have been noted by other electron microscopists, and since they have appeared with a variety of preservation techniques, one cannot help but reflect upon the variations Holmgren described and his explanation of them. It may well turn out that there is more than one morphological type of pineal photoreceptor. There may be, as Holmgren surmised, degeneration and a cyclic renewal or further elaboration of membranous components occurring in adult pineal outer segments beyond what is observed in normal adult visual rods or cones.

The ultrastructural evidence produced to date does not indicate much in the way of secretory activity—at least not in the usual sense of neuro-, exo-, or endocrine secretion. While the older literature contains many allegations of pineal secretory activity in these forms, only a few offer very direct evidence of elaboration and/or release of secretory substances [see, e.g., 55,56]. To discount the possibility of an elaboration and release of metabolically active materials would be premature, but it does seem likely that if secretion is occurring it is on a subtle basis, perhaps subsidiary to photoreceptive processes. We are overwhelmed with the evidence supporting photoreception, and beg for direct evidence of secretion. If these pineal systems are indeed sensory in their primary function, and if by this function they affect other organs which may or may not be secretory, it is of utmost importance that care be exercised not to interpret indirect secretory evidence emanating in reality from the affected organs as evidence of pineal secretion. In fact, some possible affected secretory organs (particularly the subcommissural organ) lie in such close proximity to the pineal region [40,57,58,59] as to make their functional distinction in pinealectomy experiments or extract analyses virtually impossible.

It is risky to draw general conclusions before many more species are examined with modern methods. Nevertheless, it does seem reasonable at this time to predict that we will eventually come to consider photoreceptors as the basic functional unit of lower vertebrate pineal systems. Other pineal activities than photoreception, such as secretion or cerebrospinal fluid maintenance [60] may also be involved in the total function, but more substantial evidence is needed. It may well turn out that in order to understand the basic photoreceptive function in all its ramifications, it will be necessary to consider pineal organs as but one component of a photoreceptive-secretory diencephalic roof complex [57,58,59]. We shall return to this point later.

The Parenchymal Pineal Organ

For the purposes of this discussion I will not dwell at length on the pineal systems of the higher vertebrates (birds and mammals).

Since the mammalian organ has occupied the bulk of attention classically, the reviews cited previously adequately cover the efforts and controversies until the past few years. Some pertinent recent developments will be reviewed, however, as well as the general cytology of these organs for comparison with those just discussed.

Pineal organs of birds and mammals are single—the epiphysis seemingly the only manifestation of a pineal system (although the swan and perhaps other species give some indication of a parapineal lobule [12]). Rather than the hollow saclike components seen in the pineal systems of lower vertebrates, most birds and mammals possess a more or less solid pineal mass with little, if any, vestige of a lumen remaining in the adult. The human epiphysis cerebri is a somewhat larger than pea-sized knob of tissue wedged deeply between the enlarged cerebral hemispheres. Its central location atop the diencephalic roof accounts for the philosophical importance attached to it by Galen, Descartes, and others in earlier days.

If the photoreceptor cell is accepted as the prime functional unit in lower vertebrate pineal systems, the unique, so-called "parenchymal cell" probably fills the designation in mammals. Within the mass of the epiphysis, parenchymal cells (sometimes called pineocytes) are arranged in circular clusters or lobules. Each parenchymal cell possesses numerous processes trailing off from the main cell body, permeating the pineal stroma, and often terminating in close proximity to the small blood vessels of the organ. Most other cells of the system fall into the category of glial types, the astrocytic variety being the most common in the human. Less easily defined are a number of other cells including subcommissural cells [61], and even occasional striated muscle fibers [62,63] that have been described in human, bovine, and rodent preparations. In no case have cells other than the parenchymal type been the main distinctive constituent. Photoreceptors have not, as yet, been observed in avian or mammalian pineal systems; the possibility that they may yet be detected should be kept in mind.

Innervation of the parenchymal pineal organ is also in contrast to photoreceptive systems. Distinct tracts originating from neighboring brain components are scanty, but nerve fibers have been found originating in various sites of the brain and coursing by a number of routes into the pineal tissue. In the rat, nerve fibers traced into the epiphysis from the habenular and posterior commissures degenerate when cut [64]. But another study [63] has shown that many of the fibers from the diencephalic roof actually course into, loop, and course out of the organ via its stalklike attachment. The prime neural components are efferent autonomic fibers originating from the superior cervical ganglion and invading the pineal tissue via two tracts coursing through the tentorium cerebelli. Very recently, tritiated norepinephrine has been reported to

become localized in axons terminating on the pineal parenchymal cells [65], and the predominance of autonomic fibers has also been demonstrated in the monkey [66]. Whether neurosecretory tracts enter or leave any mammalian pineal organs is still an open question [see 61,67].

The human pineal body often contains calcareous granules (corpora arcuacea), particularly in the more glial regions. These have commonly been attributed to regressive changes in the organ which usually, but not always, commence sometime after puberty. In spite of these changes, many workers have demonstrated that the epiphysis cerebri remains as a distinct entity throughout life. Other adult mammals may [68,69] or may not [70] manifest more distinct regressive changes as detected both morphologically and physiologically.

In general, it has become increasingly difficult to consider mammalian pineal organs as "useless" vestiges in view of considerable evidence of substantial physiological activity. Among other reported properties there is high phosphate turnover [71], high amino acid formation [69], a high level of serotonin [72], high amino peptidase [73] and succinic dehydrogenase activity [74,75], and a rapid absorption of certain dyes [76].

Now this picture, particularly the human or mammalian one, represents a system that is far more suggestive at the outset of secretion than pineal systems in which photoreceptors dominate the scene. In addition, the literature is crowded with descriptions of substances, globules, or histochemical reactions interpreted as having secretory significance.

Perhaps the largest secretory chapter in all the pineal story arose when tumors near or within the epiphysis were observed coincident with precocious gonadal enlargement and puberty in boys. The stage was thus set for some fifty years of pinealectomy and pineal extract administration aimed at elucidating a pineal-gonad interplay. Nearly the gamut of experimental birds and mammals was utilized, so that by now the great majority of pineal literature on birds and mammals involves argument for and against pineal influence on the gonads and other endocrine organs.

And an argument it has been. By sheer weight of manuscript paper, the evidence discounting an antigonadotropic function of the epiphysis is probably nearly equal to that favoring it. A central problem seems to have been elimination of the possibility that pinealectomy or tumors in the region might indirectly affect other brain regions such as the hypothalamus-pituitary system which possess known direct control over gonadal function. Kitay and Altschule [4] reviewed the problem in 1954 and emphasized the futility of making a sound conclusion on the basis of evidence then available. A few reports to the contrary, it still seems quite doubtful that such a function exists in the lower vertebrates (which is not surprising in view of our previous discussion).

Recently several workers have focussed attention on pineal relations

in rodent reproductive cycles, and at present there is fairly solid evidence in this regard. Particularly well documented are cytological changes (cell volume and granule content) in the pineal organ associated with varied day-length in the mouse, *Peromyscus leucopis* [61], and the demonstration of a histochemically detectable secretory cycle in rat pineal parenchymal cells [77]. Likewise, it has been adequately shown that rat and hamster pineal organs undergo a marked reduction in weight, lipid content, and levels of serotonin and related amines under conditions of continuous light [e.g., 78,79,80,81], a response which is obtained equally well after hypophysectomy, gonadectomy, thiouracil feeding [82], or adrenalectomy [82,79]. Since section of the optic tracts does alter these responses, it seems that mediation is accomplished by the central nervous system rather than hormones of the glands tested [79,81].

Now, continuous light also brings female rats into a condition of prolonged estrus with a concomitant increase in ovarian weight, and this latter condition is reportedly duplicated by pinealectomy and prevented by pineal extract treatment [80]. Pineal extract administration also appears to re-establish estrous cycles in cases of light-induced prolonged estrus [83,84], produces anestrus in postreproductive-aged rats where spontaneous persistent estrus exists [85], and is also reported to have a differential effect on fat deposit in male and female rats [86,87].

More substantial evidence is needed, but if, as these observations imply, the rodent pineal organ is engaged in translating photic stimuli from the eyes into control of reproductive cycles, it is logical to look toward a pineal antagonism to pituitary gonadotropic function for mediation of this control. There are a number of recent reports indicating greatly altered hypothalamic neurosecretion (as indicated by histochemical detection) after pinealectomy [88] or treatment with pineal extracts [89,90,91], and others [e.g., 92] which indicate pineal changes after hypophysectomy. However, an accurate evaluation of the significance of these observations (and many before then) remains difficult. An unequivocal link or antagonism between pineal organs and known pituitary gonadotropic functions has apparently not been established although suggestive evidence continues to be reported [e.g., 93].

Many other connotations might also result from an interplay of pineal and pituitary activities or pineal influence of other endocrine organs. Recently, osmoregulatory functions have been mentioned prominently. Farrell [94] has described a temporary decline in aldosterone output from canine adrenal glands following pinealectomy; a decline which is corrected by pineal extract treatment. Perhaps related to this in the overall picture of salt-water balance are observations of increased diuresis [90] and accumulation of neurosecretory material in the pars nervosa of the pituitary following administration of pineal extracts [89,90,91]. Subsequent evidence for and against antidiuretic or aldosteronotropic

functions is problematical [13,92,95,96,97,98,99,100,101,102,103,104,105,106]. Nevertheless, Farrell's [94] interpretation of the situation, that the pineal organ acts as a storage area for a hormone, adrenal glomerulotropin, produced somewhere in the diencephalic roof, has remained a prevailing hypothesis. The further possibility that a pineal glomerulotropin inhibitor is also produced might help explain the diversity of experimental evidence.

At this point it is interesting and perhaps significant that similar aldosteronotropic, antidiuretic and osmoregulatory functions have been proposed [107,108,109,110] or considered [59] for the equally enigmatic subcommissural organ (which was briefly mentioned at the start of this essay).

The subcommissural organ, lying subjacent to the pineal system in the diencephalic roof, secretes its mucoprotein product into the brain ventricle as the fiber of Reissner in those vertebrates that possess photoreceptive pineal organs [5]. A neural tract, not believed neurosecretory, often connects pineal and subcommissural regions in these forms. Oksche [57,58,59] has postulated that this tract serves as a pathway by which the subcommissural organ is controlled by photoreceptive capacities of the pineal complex. But mammalian evolution appears to have been accompanied by involution of an increasing number of the subcommissural cells to form follicles which release their material in an endocrine fashion [5,111]. In man and a few other mammals little, if any, ventricular release occurs, and subcommissural follicles are commonly found in or near the pineal organ [1,100,101]. The fact that subcommissural cells have also been described in the rodent pineal [61,107] and are occasionally found as follicles along frog epiphyses [40], lends further weight to the supposition that later evolution of pineal organs has been accompanied by concomitant intermixture with subcommissural components. A functional merger of the two systems may also be involved (see Oksche [59] for a detailed discussion of this point). It is not presently possible, therefore, to distinguish whether the hypothesized antidiuretic or osmoregulatory activity is attributable to pineal parenchymal cells, subcommissural cells, or a synergism between the two [100,101].

As mentioned previously, connections may also exist between the pineal organ and other brain roof components such as the habenular nuclei. These latter may [112,113,114] or may not [115] be active in inhibiting thyrotropic functions of the pars distalis of the pituitary. Miline and his co-workers have presented information on such pineal-habenular and pituitary relationships and proposed a role in general adaptation to stress [116].

Nearly forgotten in the array of possibilities is the importance that parenchymal pineal organs might assume during immature and embryonic ages. It has been known for some time that newly hatched chicks

(12 days) display an epiphysis that becomes temporarily invaded by massive lymphoid accumulations, but the significance of this phenomenon remains a mystery [117]. Even before hatching age, evidence has been advanced that pineal tissue grown *in vitro* has already undertaken activity antagonistic to pituitary function [118]. In the rat pineal organ an adult level of serotonin and succinic dehydrogenase activity is reached within two weeks after birth; during older age, it drops off [74,81]. But perhaps even more striking are the recent qualitative observations by Owman [119] suggesting a greatly elevated secretory activity in the pineal organs of fetal rats in comparison to adult stages. Our understanding of these early stages is fragmentary, and it is too early to predict where these observations might fit into the over-all picture.

As complex as this situation seems, it reflects but a portion of the physiological considerations given to the question of secretion by parenchymal pineal organs. Unifying principles by which many of these considerations might be brought together do not, as yet, appear clearly defined. The primary point to be gained is a realization of the concentration of effort to establish a role of *secretion* in so far as parenchymal pineal organs are concerned. This wealth of secretory information is as striking as the dearth of such information in the lower vertebrate systems where photoreceptive data predominate.

These prevailing secretory concepts have not been deviated by the recent introduction of electron microscopical study. The first published electron microscopical examination of mammalian pineal tissue (from the rat) emphasized the variety of vesicles found within the parenchymal cells and frequent nerve endings located in the vicinity of fenestrated capillaries [120]. Other investigators described nuclear components [121] and suggested secretory and neural implications for various mitochondrial configurations and other cytoplasmic organelles [122,123]. Accounts of the cow pineal organ [124,125] have focussed attention on the wealth of fine cytoplasmic tubules, smooth endoplasmic reticulum, Golgi complex, lipid inclusions, and densely filled vesicles of parenchymal cells. Processes of these parenchymal cells were seen to terminate on a thin basement membrane associated with perivascular spaces. These terminations are dilated and possess many mitochondria. They also contain membranous invaginations interpreted as pinocytotic, therefore suggesting cellular uptake of material from the blood stream. At the same time, the other granular and oval vesicular structures noted within the parenchymal cytoplasm are more suggestive of secretory activity.

By contrast, similar cells in the rat pineal are reported to have club-shaped endings extending into the perivascular space. The endings display closely packed vesicles, some of which contain osmium dense granules and numerous vacuolized and normal mitochondria [126]. Preliminary tests [127] have shown that the osmiophilic granules disappear

after reserpine treatment, and that this disappearance and subsequent reappearance parallels that of brain serotonin and other non-adrenaline contents after similar treatment. These data have, therefore, been interpreted as suggesting the storage of a pineal hormone, probably one or several of the biogenic amines. However, in view of more recent evidence, a further confirmation appears necessary before this role can be accurately assigned to the parenchymal cells. Quite similar areas containing equally dense granular vesicles have been identified as adrenergic nerve endings since the dense granular vesicles are reportedly the site of tritiated norepinephrine uptake [65]. More information is needed to establish whether both axonal and parenchymal endings are separately identifiable and to distinguish their respective chemical constituents. In any event, both approaches strengthen rather than weaken the basic secretory concept.

The case for elaboration and release of a substance or substances remains, therefore, problematical, but perhaps more sound as extensions of classical avenues of investigation merge with newer methods. As observers to this progress, we can distinguish more clearly the contrast between parenchymal and photoreceptive pineal systems thus far examined.

Pineal Organs And Pigment Behavior

In 1915 McCord and Allen [128] performed a simple experiment which gave a startling result. It consisted in essence of feeding frog tadpoles minced mammalian pineal organ. The tadpoles responded with distinct but temporary blanching of pigment cells in the skin—the melanocytes being the most responsive pigment cell type. The same effect has subsequently been obtained by many workers using amphibian larvae and pineal tissue from several different mammals either as food or as a source of pineal extract. The extract was administered by injection or by addition to the aquatic environment.

I have deferred discussion of the interpretation of these experiments until a clear distinction between parenchymal and photoreceptive systems has been portrayed. It is essential that we view both components involved here—mammalian pineal extracts and amphibian pigment cells—in the knowledge of the differences which exist in higher and lower vertebrate pineal systems. Without this, misinterpretation of data is possible.

To be sure, a melanocyte-affecting substance does exist in mammalian pineal organs; it has been isolated and characterized by Lerner and co-workers (see [129,130] and earlier publications). The substance, N-acetyl-5-methoxy-tryptamine, or melatonin, produces the blanching effect when applied in minute amounts to amphibian skin by causing a clustering of black melanin granules around the nuclei of melanocytes.

In this respect, it is opposite to melanocyte-stimulating hormone (MSH, known to be released from the pars intermedia of lower vertebrate pituitaries) which causes skin darkening by scattering of the melanin granules throughout the cytoplasm of melanocytes. Melatonin can be obtained in lesser amounts from other nervous tissue, and is probably derived by N-acetylation and O-methylation of serotonin [130,131].

Isolation and bioassay of melatonin is not the only means of detection since Prop and Ariëns-Kappers [132] have made positive identification in rat pineal tissue by the use of paper chromatography. Mention has already been made of the presence of serotonin and the electron microscopical evidence for the release of biogenic amines (see pp. 602 and 605).

The story would seem a neat one were it not for an intriguing paradox. Mammalian melatonin acts on amphibian melanocytes, but no direct evidence yet exists that amphibian pineal organs produce or contain melatonin. On the other hand, no evidence has demonstrated that mammalian pineal melatonin affects mammalian melanocytes; its role in man is unknown [130].

Now many amphibians and other lower vertebrates respond to light or dark, or to the texture of their background by blanching or darkening of the melanocytes. It is quite common for blanching to occur in the dark but some species respond oppositely and some variably. Bagnara [133] has studied the African clawed frog, *Xenopus laevis*, and found that cautery in the pineal region of tadpoles prevents the usual dark-induced blanching from occurring. He has, therefore, postulated that the pineal system is normally activated in the dark and produces melatonin which, in turn, blanches the melanocytes. If this hypothesis can be substantiated, it could account for innumerable observations of pigment behavior to light and dark made over the last fifty years. Presumably melatonin would be antagonistic to the action of MSH whose release is known to be stimulated by light reaching the lateral eyes.

Bagnara's contention is not without support. A similar, though less apparent, reduced blanching has been cautiously reported [27] in blinded tadpoles of the tree frog, *Hyla regilla*, after removal of the frontal organ. These animals were compared with blinded controls. Similarly, blanched melanophores have been noted in the region of pineal transplants in the heads of host tadpoles [134]. Recently we [52] have studied some cases of similar blanching around pineal transplants into tailfins of newt larvae (*Taricha torosa*). In our experiments, the blanching was of short duration (4–5 days in spite of continued development of the pineal graft), was of variable intensity (sometimes absent), and also occurred in some cases when brain tissue other than pineal was transplanted.

Brick [135] pinealectomized salamander larvae (*Ambystoma opacum*) and he also observed a resulting loss of dark-induced blanching. His experiments were accompanied by pituitary removal in some cases, and the

results were interpreted as indicating a pineal action exerted on the pituitary and its melanocyte stimulating activities. Pinealectomy tests in our own laboratory [136] using larvae of *Ambystoma opacum* and *Taricha torosa* are in agreement with all those cited above with the unexplained exception that loss of dark-induced blanching appears to diminish in a short time (after 5–10 days, similar in time to the loss of blanching around pineal transplants), and larvae return to normal pigment behavior in 30 days despite pineal loss. We have, therefore, treated the results with skepticism until the possibility of other factors, such as operative shock, can be definitely eliminated and the return to normal pigment behavior explained.

For the moment at least, it seems desirable, on several grounds, to reserve judgment on the possibility of melatonin release by amphibian pineal cells.

In the first place, as we have seen, there is little morphological evidence from electron micrographs of amphibian pineal cells to date, to indicate a secretory role. Admittedly, however, since melatonin is known to act in quite small concentrations, it is possible that our methods have yet to distinguish an actual melatonin secretory site.

Secondly, a number of studies involving pinealectomy in amphibians have been carried out in recent years in which no distinct change in pigment behavior was detected (e.g., [49,137]). This could be due to species variation resulting in the lack of a long-term, distinct response.

Thirdly, if melatonin *is* released in amphibians, we need precise evidence that it comes from the pineal organ, and not some surrounding brain region. The fact that mammalian pineal organs possess relatively higher concentrations of melatonin than other tissues does not mean *a priori* that amphibian pineal systems do also, or that the pineal organ is an exclusive site of melatonin synthesis. Again, it is well to remember that known secretory structures such as the subcommissural organ lie in close proximity to pineal organs, and that in mammals and at least one frog species, subcommissural cells and follicles are commonly intermixed with the pineal organ itself. Experiments to date do not adequately distinguish between these components or other nearby regions.

Finally, there is a distinct possibility that rather than releasing melatonin, the pineal organ may exercise some mode of control over another secretory or neural component which in turn affects pigment cells (perhaps among other things). That the subcommissural organ is implicated in such a scheme is strongly suggested from the recent, thorough analysis of the system by Oksche [59]. The possibility of hypothalamic involvement is supported by Brick's [135] experiments and other observations that administration of pineal extract to hypophysectomized frogs does not cause blanching of the skin as it does in normal animals (e.g., [138]), and that pinealectomy in the turtle appears to promote

detectable changes in hypothalamic neurosecretion [88].

Pending new evidence to satisfy these areas of doubt, one might summarize by stating that evidence is at hand which suggests that in lower vertebrates the postulated photoreceptor function may be related to the action of other neural and endocrine components. Behavioral, metabolic, and other effects such as pigment control may thereby be related, directly or indirectly, to pineal function. The possibility of pineal elaboration of a melanocyte-affecting hormone in the lower vertebrates awaits more substantial evidence.

My point here, and indeed a main point of this essay, is an appeal that we not be misled by the temptation of overgeneralizing between photoreceptive and parenchymal pineal systems. Our evidence has led us in the direction of accurately distinguishing the two types, and the older literature discloses many confusions in the rush to bind too closely functional qualities to both types at once.

Pineal Organs As Developing Systems

This essay began with a speculation that a spectrum of pineal configuration and functional potentialities exists in nature. Thus far a case has been presented to establish the two poles of the spectrum—photoreceptive and parenchymal systems. It seems plausible that the less highly evolved pineal systems are predominantly photoreceptors and serve to translate photic stimuli into physiological control of some type. Though evidence is scanty, the translation may involve a secondary secretory function of the pineal complex itself or the action of neighboring organs. It is not unlikely that further evolution of pineal systems, perhaps in conjunction with more highly evolved cerebral and visual capabilities, has led to pineal organs which incorporate and emphasize the secondary secretory role, and which rely upon optic pathways for photic input.

If this proposal is sound, then the adult systems examined to date display little evidence of the evolutionary transition, and it becomes important to find approaches which will elucidate more clearly the middle of the spectrum. We should like to know, for example, if pineal photoreceptors and parenchymal cells are homologues; or if the transition has involved the degeneration of one cell type and the emergence of a new one.

I have hinted previously that much might be gained by inquiry into the embryonic beginnings of pineal systems. This might now be said not only from the standpoint of attacking the factors which initiate, contribute to, or control the development of smaller brain derivatives, but also as a means to gain insight into possible evolutionary relationships of the pineal cell types we have discussed. I believe there is at least one and perhaps several developmental sequences now known which might

ultimately help clarify the middle of the spectrum and serve to link photoreceptive and parenchymal types.

It is likely that all pineal systems begin visible embryonic emergence similarly—as a single or perhaps double (lizards) outpocketing of the thin diencephalic brain roof. The unspecialized cells (ependymal cells) which contribute to the outpouching(s) proliferate rapidly to form an early, hollow vesicle atop the brain roof. From this vesicle a secondary evagination may occur at the anterior end to form the parapineal organ. The origin of parietal eyes in reptiles, however, is still not known with certainty.

At this time in development, the essential differences between parenchymal and photoreceptive types begin to unfold. In birds and mammals many secondary outpocketings form around the wall of the vesicle. These in turn subdivide, and eventually all the diverticulae become separated from each other as tiny rosettes or follicles of ependymally derived cells. Many of the cells to each follicle differentiate into pineal parenchymal cells while others apparently develop into astrocytes and other less easily identifiable types. Other cells such as microglia and subcommissural cells may secondarily invade the interstices between the follicles.

Photoreceptive systems remain less bulky and more delicate. The epiphysis is usually a single hollow sac with few or many blind diverticulae opening into it. Often, but not necessarily always (e.g., some lizards), an open connection with the brain ventricle is retained. As the sac enlarges, the cells of its walls differentiate either into the intricate photoreceptors or into supportive cells. The latter may be pigmented cells surrounding the photoreceptors, or glial types which may send processes across the pineal wall to end on or near blood vessels. The origin of the ganglion cells in the system is unknown. Parapineal systems (parietal eyes, frontal organs) possess the same basic developmental pattern. Frontal organs which develop in most species of frogs come to possess a thickened floor, rich in photoreceptors and ganglion cells, while the roof remains thinner, but composed of the basic cell types. In the lizard parietal eye, the floor acquires many pigmented cells and comes to resemble an inverted retina. Furthermore, an excellent lens is formed in the dorsal surface, perhaps from materials extruded by immature pigment cells of the retina [139].

These, then, are the general features of developing photoreceptive and parenchymal types. To the extent of a common ependymal origin, it does appear that a similar derivation might be ascribed to photoreceptors, parenchymal cells, and at least some of the supportive or glial cells that accompany them. The critical question of whether or not a parenchymal cell is most closely related to photoreceptors or to some supportive type is by no means settled.

Pineal development in the newt, *Taricha torosa*, seems unique in that it appears to combine some properties of both photoreceptive and parenchymal sequences, and I should like to dwell briefly upon this system and what experimental work has been done on it [52] in the hope that similar approaches might be utilized for other vertebrates.

The newt pineal organ is single and, at first glance, simple or even degenerate. An evagination giving rise to the organ begins by elongation of a small group of ependymal cells in the center of the diencephalic roof.

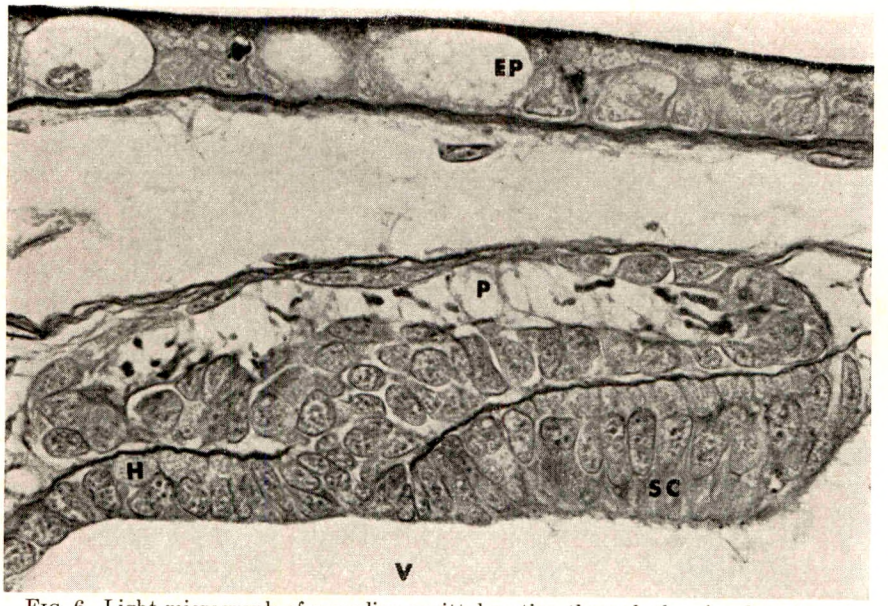


FIG. 6. Light micrograph of a median sagittal section through the pineal region of a young newt larva. Single pineal organ with a wide lumen (P) can be seen dorsal to habenular commissure (H) and sub-commissural organ (SC). Note that outer segments of photoreceptor cells protruding into the lumen are particularly well developed at the peripheral angles of the organ. A slight initial flattening of the organ by the developing cranial roof can be seen. EP, epidermis of the head; V, third ventricle of the brain. Aldehyde-fuchsin preparation. 423×.

These create a bulge, while other cells surrounding the bulge divide and proliferate. Combined action of bulging and proliferation directs the outward evagination of the new pineal bud. The embryo is now approaching the final stages of development before hatching as a larva.

The ring of proliferating cells remains quite active. As the pineal bud enlarges, this proliferation zone comes to occupy the neck-orifice region connecting the pineal vesicle with the brain, and it continues to supply new cells. An almost identical evagination sequence and source of cellular supply is apparent in regenerating pineal organs after embryonic pinealectomy [140]. As new cells emerge from the proliferation zone and

migrate peripherally through the floor and walls of the enlarging vesicle, they appear to differentiate into photoreceptors and supportive cells. Because of this pattern of growth, the most highly differentiated photoreceptors are found in the extreme peripheral angles of the vesicle. Between these and the orifice are seen progressively less well-developed photoreceptor cells. The roof cells remain nearly cuboidal or flattened with only occasional photoreceptive appearance.

This is the "set of equipment" possessed by the newly hatched newt

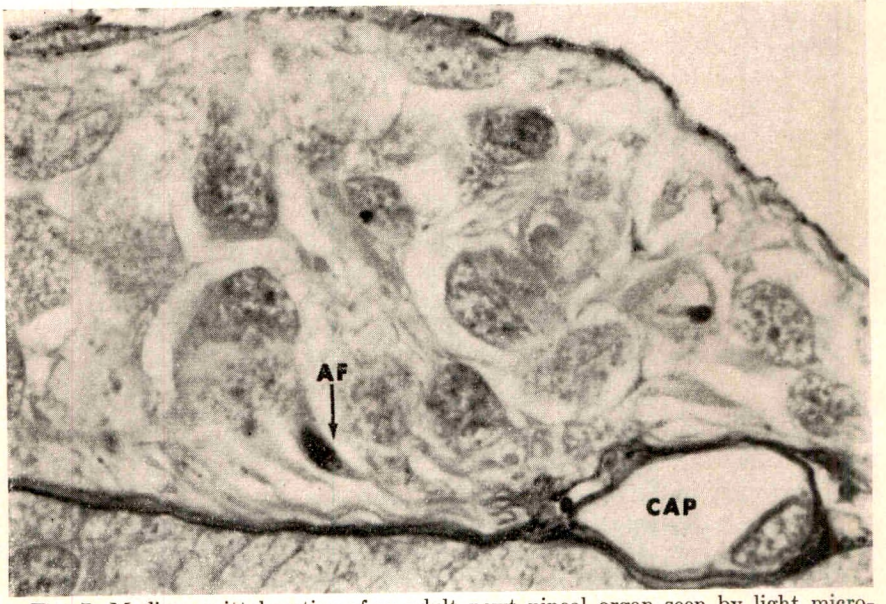


FIG. 7. Median sagittal section of an adult newt pineal organ seen by light microscopy. Note the totally different, predominantly glial cytology as compared to the photoreceptive configuration seen during young larval stages (Fig. 6) Aldehyde-fuchsin positive droplets (AF) are visible in one of the processes of glialike cells converging toward a capillary (CAP). 1080×.

larva, and it enlarges into a typical photoreceptive, but single, pineal organ in approximately the next ten days (Fig. 6).

Then a new developmental sequence begins. It seems to be initiated by pressure of the developing cranium over the pineal vesicle, and it results in a progressive flattening of the vesicle until, in two or three months as the larva approaches metamorphosis, the lumen of the pineal organ is almost totally obliterated. By now very few photoreceptors can be found, although some may be seen protruding into small, round, disconnected lumen remnants. The whole organ has taken on a totally different character, for the predominant cell type seems to be a supportive or glial type of cell. Occasionally, we have been able to detect

what appear to be accumulations of droplets (Fig. 7) within the processes of these cells near their point of contact with blood vessels.

We cannot yet say that the final adult cytology is in any way akin to a true parenchymal cytology; it may represent, in reality, a degeneration. However, the question arises: can this rather dramatic shift in cytology denote a basic change in function of the organ from that of photoreception toward one emphasizing secretion? This possibility was suggested some years ago on the basis of similar shifts of cytology of frontal organs in some frog and toad species [57]. The change seems to accompany metamorphosis of aquatic amphibian larvae which subsequently inhabit dryer terrestrial environments. Indeed, the newt we have worked with is one of the more "dry-living" species as an adult. The way seems clear for attempts to correlate water-salt balance mechanisms and other physiological and endocrinological requirements of these animals and of other amphibians.

Discovery of the cytological changes which occur during larval stages of the newt has prompted us to undertake other studies on the factors that might control the initial photoreceptive developmental sequence. As a start, we have watched the development of newt pineal buds transplanted into sites devoid of the physical influences encountered in the cranial region which appear to initiate the cytological shift discussed above. Embryonic pineal organs transplanted into the anterior eye chamber or dorsal tailfin of older host larvae occupy fluid or spongy surroundings which theoretically should not impose physical limitations on a developing pineal vesicle.

Under these conditions, as long as one is careful to include some of the proliferation zone with the transplant, the pineal buds develop into well-formed vesicles. If the proliferation zone is lacking or minimal, smaller than normal vesicles will be obtained. Thus far, transplant vesicles of various sizes have been reared for up to 36 days. During this time, they remain unflattened, hollow, and come to possess typical photoreceptive qualities. Had they been left in the normal cranial site, flattening would have intruded upon this photoreceptive character before this time. We are now anxious to extend these experiments toward longer term cultivation *in vivo* and *in vitro* in order to see if the "pineal metamorphosis" toward the more secretory appearance noted in normal larval development ensues in older explanted or transplanted vesicles deprived of the initial flattening influences.

The transplant vesicles studied to date were obtained just at the time of pineal bud emergence, but long before differentiation of photoreceptors was detectable. The fact that transplant cells do undergo quite normal photoreceptive differentiation indicates that the basic direction of differentiation has been set previously, and remains unaltered in the eye chamber or tailfin. We have been interested to see that even very tiny

implants will persist in this course of development. Vesicles with fewer than 50 cells, for example, will differentiate quite normally in the tailfin.

Just when this relatively unalterable predilection toward photoreceptive cellular differentiation is established in the embryo is not precisely known. A number of previous workers [6,141,142,143] have obtained vesicle differentiation from pineal primordia taken from much younger embryonic stages and transplanted or grown *in vitro* for short durations. It appears from these studies that pineal determination is set during neurula stages when the presumptive brain and spinal cord are still a wide, flat plate of cells lying over the archenteron of the embryo. Presumably the archenteric roof induces the overlying cells in two pineal parts of the plate to begin synthesizing materials which will ultimately characterize many of these cells and their derivatives as pineal photoreceptors.

The process which then takes place must either be a long one, or else it lies dormant for a time, for there is a considerable delay between inductive onset of pineal determination and the emergence of a pineal bud. Subsequently, it is still some time, as we have seen, until actual photoreceptors can be recognized. During this interval, the embryonic central nervous system is being formed as the neural plate curls into a tube thus fusing the two presumptive pineal areas; the major regions of the brain also become distinct, and the eye rudiments are pinched off. There is no information about what is taking place within the presumptive pineal cells during all these events. Perhaps newly acquired techniques of electron microscopy and cytochemistry can lead us to some understanding.

Once the pineal bud has made its appearance and activity in the proliferation zone increases the size of the vesicle, there is evidence suggesting that the amount of growth and differentiation may be controlled by surrounding tissues. If, after 30 days, tailfin transplants which have included the pineal rudiment plus a large amount of diencephalic roof tissue are compared with others composed of similar rudiments, but with less diencephalic roof, we have often seen that the smaller transplants develop a larger differentiated pineal mass. These are composed of multiple pineal vesicles or lobes which exceed the mass of normal pineal organs of similar age. Larger amounts of diencephalic tissue, on the other hand, lead to formation of a more normal-sized pineal mass and a single vesicle.

We cannot yet say with assurance what type of control is occurring in this system or if it is very precise. It seems natural to postulate for the moment that some type of inhibition is exercised by the non-pineal neural tissues involved in the transplant area. This seems more plausible since only single, normal-sized vesicles are obtained from similar grafts into the anterior eye chamber regardless of the amount of accompanying tissues. Here the surroundings, the eye itself, constitute a massive, and

perhaps controlling diencephalic derivative.

More precise characterization of the influences of accompanying tissues will be the aim of further investigation. At this juncture, it seems we are beginning to glimpse the experimental possibilities of the amphibian pineal system as a developing small brain derivative.

* * *

What has been termed "*the* pineal problem" still remains open—and open from many angles. The problem is now mature enough that it can be viewed from the facets it embraces: from gross anatomy to ultrastructure; from broad endocrinological implications to the discrete chemical detection of cellular processes. The levels of inquiry which cut across the problem range from the evolutionary implications of pineal organs and the cellular homologies which they may possess, to the problem of function in the various vertebrates, and on to the embryological questions manifest in the acquisition of intricate membrane systems and chemical properties which we term differentiation.

The breadth of the pineal problem has produced a chaotic literature in previous years largely from the tendency to jumble the facets of the problem, from a readiness to attribute to some pineal systems qualities demonstrated in completely different pineal systems, and from the simple fact that not enough pineal systems had been described or subjected to experimentation.

I have written this review in the optimism that the era of chaos is about over. We now have the insight to pinpoint more specific facets and the technical means to approach several of them. What seems most necessary is to recognize that *if the facets are organized and defined accurately* we actually have available a good measure of information with which to characterize pineal systems and to pursue their mysteries further.

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PERSPECTIVES

By CHARLES COULSTON GILLISPIE

"TELL ME," said a physicist, "what are the exciting things happening in the history of science?" Thinking on the matter, one ventures to predict that an answer soon may be the contemporary history of physics itself drawing on the material currently being mobilized in two related projects, that on American physics housed in its newly dedicated Niels Bohr Library of the American Institute of Physics and that on quantum mechanics under the auspices of the American Philosophical Society. A retrospective symposium of physicists on the discovery of the neutron enthralled the International Congress of the History of Science held late this past summer in Ithaca and Philadelphia. But all that is still a prospect, one depending for realization on the development of a new species of scholar who should fortify with a professional command of physics the historian's mode of narrative, telling what the technical content of an event was and telling, too, the more delicate, the more elusive, the more enduring story of how one event led to another and thus spun out history in its one dimension of sequential time. When that does come about, it will be in continuation of a literature already to be regarded as classical in the historiography of science, and one is even safer in predicting that in that literature no writings worthier of emulation will be found than those of E. J. Dijksterhuis, to whom at the banquet which closed the recent Congress the History of Science Society awarded its highest honor, the George Sarton Medal.

The immediate occasion of this Perspectives is the availability of Dijksterhuis's most comprehensive book, *The Mechanization of the World Picture*, first published in Amsterdam in 1950 and now most skillfully translated into English by C. Dikshoorn and published by Oxford University Press [1]. Among those (very few they are) who may properly be called great historians of science, Dijksterhuis has so far been perhaps the least known in the English-reading world. It is difficult to understand why this should be so. He is Professor of the History of Science in the University of Utrecht and has published much in his own country and his own language. His is the admirable tradition of Dutch science and scholarship, however, and his work seems equally at ease in French, in German, and in English. Indeed, he has brought unobtrusively into print within the space of a few years what only a lifetime of the finest scholarship could yield in modest and in full knowledge of a great subject. In 1951, Dijksterhuis followed the first edition of his *Mechanization* with a documented set of studies on Cartesianism in his native country, Descartes having returned the hospitality of Holland by the philosophy

which served in effect as the nursery of its science. In 1955 appeared the first volume of an edition of *The Principal Works of Simon Stevin*, to whom Dijksterhuis has devoted one of his deepest and most considered studies. In the same year appeared *The First Book of Euclidis Elementa* edited with a glossary. In his renderings of the mathematics of antiquity, Dijksterhuis follows a policy of his own devising. The Greeks used no symbols in writing mathematics. Their geometry is in words, and modern translations, notably those of T. L. Heath, devoted admirer though he was of the corpus of ancient mathematics, convey rather the results than the style and quality of the reasonings. Dijksterhuis does not go to the other extreme of literal translation, which would be of an intolerable verbosity, and he has perfected instead what may be called a free rendering. He treats his sources as one might in translating poetry. Through his own command both of language and of the author's intent, he conveys the latter in the former, and thus by art admits the reader more closely into the thinking of Euclid or of Archimedes than other scholars have succeeded in doing. On Archimedes Dijksterhuis published two volumes in 1956, one an edition of the *Arenarius* with glossary, the other a small but general study of inestimable value in expounding what scholarship does in fact know of the greatest scientific mind in all antiquity.

This constitutes a notable body of work, and it presents the reviewer with a peculiar though not unwelcome problem. Everything in it seems right. Everything in it seems necessary. Dijksterhuis offers no handle for argument as between the scholar and the one who would discuss what he tells us. And perhaps this is why he is not yet so famous in the subject as have been more programmatic or more eristic scholars—his work makes no noise. He rides no hobby. He does not cast about in his knowledge of the past of science for the bits that seem to foretell and hence to justify what he wants it to be or do or mean today. He has no formula for the history of science. He simply tells us how it came about. Gently the opening pages of *The Mechanization of the World Picture* deny the reader who is too eager to come to grips with the subject either definition or judgment. It is clear enough what the mechanistic conception has done. It has brought forth physical science: "experiment as the source of knowledge, mathematical formulation as the descriptive medium, mathematical deduction as the guiding principle in the search for new phenomena to be verified by experimentation" [3]. It has rendered rational and even possible the technology by which the modern world has necessarily to live. It has penetrated and polarized philosophy. "Owing to all these factors the mechanization of physical science has become much more than an internal question of method in natural science; it is a matter that affects the history of culture as a whole, and on this account it deserves the attention of students outside the scientific world" [4].

Less evident than what mechanization did, is what we are to think of it

and what it was. Reluctantly but unavoidably Dijksterhuis introduces the term "mechanicism" in order to distinguish the movement as a whole from the science of mechanics. Its champions within science and apologists without have advocated mechanicism on pragmatic grounds as the mode of handling experience which admits of clarification and verification, which replaces errors and figments with reliable and reproducible results. Its critics, unable to deny the evidence of its gathering success, find this but a brutal justification for what they take as the encroaching sands of a philosophical desert, a wasteland of material plenty and spiritual discontents. Not that Dijksterhuis thinks choice or judgment unimportant, but we cannot well proceed to make them until we know how mechanicism in fact evolved and what the reasons were for its hold upon the thinking of scientists, until we appreciate why it was that Huygens (to take a distinguished example from among many) laid down as a policy that we must necessarily adopt the mechanical mode of reasoning or else renounce the hope of comprehending anything in physics.

Ultimately the reasons for appreciating mechanicism have to be historical, for the subject itself has changed in scope and content. What do we mean by mechanistic? There is no easy answer: "In one sense the whole of this book is an attempt to discover to what extent it is possible to speak of a mechanistic world picture" [5]. Do we mean that conception which would consider the universe as a machine, such a notion as would end with making mind itself an implement? Or do we mean that nature may best be described in its operations by means of formulations drawn by extension or analogy from the science of motion? This is quite a different matter. The one implies a science of machines and the other of matter in motion. The one usually pre-supposes the notion of a designer-creator of the world, and the other that reality is bits and pieces in random whirl. How, then, did a word drawn from the former come to designate the latter? And if (however inconsistently) we do mean by mechanics primarily the science of motion, what kind of kinematics was at the heart of mechanization? That of the classical world, most notably of Aristotle, which is inconsistent with the later world picture of corpuscular mechanics? That called confusingly enough "classical" by physicists who mean by it Newtonian rather than antique (though as we shall see Newton had not reached it)? Or finally do we mean by mechanistic the concepts of quantum physics? These are not questions to be answered *a priori*, and though Dijksterhuis puts them searchingly, he attempts no complete resolution. His account is of the genesis of classical science, and that culminates in the work of Isaac Newton.

The scope of the book thus delimited, let it be said at once that this is no narrow monograph. In a quite extraordinary way the treatment combines comprehensiveness with discrimination in the selection of material.

Dijksterhuis is not one who would make all science emergent from some egg in the Archimedean lever fertilized by the Democritan atom. His method is to survey in turn each of the ancient schools and each successive period and distinguish therein the elements which in historic fact and regardless of initial philosophical incompatibilities did come together in the evolution of mechanicism. He addresses himself, moreover, to a general (though certainly an intelligent) public of whom he requires no prior knowledge of the subject. Here again, however, he makes the best of both worlds, never compromising the scruples of the specialist in the interest of a specious clarity. His is a true clarity in which there is no oversimplification and which bespeaks a serene and comprehensive mastery. The reader is bound to feel secure in his sense of deep reserves of scholarship behind every statement and every judgment. One's only complaint must be of the author's disclaimer of intent to write a book from which professional historians of science too will learn. They can and do. There is scarcely any topic touched, on which some observation will not strike them, occasionally by its novelty, more often by its economy, justice, and accuracy. And so widespreading are the roots of this subject, so central the stem, that the book may be recommended without qualification to those unfamiliar with the literature of the history of science, both as the best treatment extant of physicomathematical science from the beginnings through Newton, and with no less confidence as an exemplar of what the historical approach at its most conscientious may accomplish in illuminating the scientific enterprise in the context of general culture.

No summary would be useful of a book which is itself a summary, indeed a *Summa*, and an idea of the contents will be better conveyed by an account of the anatomy of the work, for its structure offers several features which, if not exactly novel—in what sense can sound history offer novelty?—nevertheless throw the subject into a distinctive relief. The historian orders his information in the first instance by appropriate choice of periods, and Dijksterhuis disposes his material into four—Antiquity, which he carries through Patristic times; the Middle Ages, for our purposes the twelfth through the fourteenth centuries (he appreciates but does not develop the Arabic transmission); that which he calls a prelude to classical science and which essentially consists in the Renaissance continuing into the sixteenth century; and finally the rapid inauguration in the seventeenth century of classical physics itself. It has not been usual in the historiography of science to bring forward the Renaissance phase as a period in its own right. Thus to promote it appears a most valuable device for it eliminates the false and sometimes angrily disputed question whether the Renaissance as a movement of culture was favorable or inimical to the scientific enterprise. Dijksterhuis never enters as a partisan into such disputes, nor does he take issue in point of

method with the concept of historiography whence they arise, by which a unique course led with right and wrong turnings from the Greeks to Newton and on to the present. This is not his practice. He treats things in succession and in their own context and not only relative to some datum, the seventeenth century or the twentieth, taken as a goal. Rather than judge the men of the Renaissance as if their job had been to convey fourteenth-century mechanics into seventeenth-century minds, Dijksterhuis arranges his exposition so as to make it appear that they contemplated nature in their own way and with their own interests. Their science was different from that of the scholastics and also from that of Galileo and Newton. It was in historical fact a prelude, not to be assessed either as a regression or advance.

The historian must identify themes as well as periods, the strong threads that run longitudinally along the historical fabric, and Dijksterhuis distinguishes three central topics which appear in evolving content and proportions of varying importance in the successive historical environments. He tells first of the main schools of philosophy in each period with respect to their contributions to science. In the ancient world the Aristotelian philosophy outweighed all the others. It would be difficult to better in brief or even in extended compass the account which Dijksterhuis gives of its salient features, especially of the relation of its metaphysics to its physics and cosmology. He has no illusions about its inhospitality to mathematical physics, yet one feels that his respect goes out to Aristotle for the fullness and fidelity of his constructions, and that he admires these qualities more profoundly than flashes of brilliance in less careful but seemingly more auspicious thinkers: "... a fuller acquaintance with Aristotle's scientific works leads to a much greater appreciation. If his works are read with an unbiased mind, it is obvious that here is a student of nature who possesses an extensive knowledge of physical phenomena, is vividly interested in explaining them, and bent on giving such an explanation on purely physical grounds. A comparison with Plato shows that the tendency—known from the *Timaeus*—of constructing an imaginary nature by reasoning from preconceived principles and forcing reality more or less to adapt itself to this construction had now been replaced by a purely empirical attitude, based on the recognition that true knowledge of nature can only be gained from carefully collected observational data" [6].

In the medieval period Thomism merging into nominalist scholasticism formed the expectations held of science; and in the Renaissance, humanism set the style, primarily under the inspiration of Plato. Dijksterhuis's discussion of the influence of humanism on science is a small masterpiece of nuance. Humanism was, he observes, as verbal and as respectful of antiquity as ever Aristotelianism had become. At most we are to regard it as a restriction upon literal adherence to Aristotle.

Yet neither may one overlook its service in the discovery and purification of ancient scientific texts, or in its occasionally mystical summons to mathematicization in a Pythagorean vein. In the final period, that of the seventeenth century, philosophies seem attenuated in importance by comparison to the youthful strength, the growing bodies of the sciences themselves. There was coming into its own what Dijksterhuis in a slightly earlier connection calls the doctrine of a double truth, by which "an idea may appear useful in practical scientific work and may be applied quite thoughtlessly, although it may not satisfy a man's critical and philosophical faculties. On the other hand, conceptions which are acceptable to the latter may fail to have any influence on positive science" [7].

Secondly within each period, Dijksterhuis turns to the development of those parts of what we would regard as positive science that are relevant to the theme of mechanization. The account of ancient astronomy explains the principle of the Ptolemaic computational devices, and displays the issue as between them and the spherical models imagined for physical reality. In the Middle Ages, kinematics and formalization of motion as a quality of variable intensity occupy pride of place. Nothing in the whole historiography of science has been more revolutionary in this century than the rehabilitation of the claims to scientific importance of fourteenth-century kinematics and philosophy of science. Dijksterhuis does full justice to the magnificent researches of Pierre Duhem which wrought (and sometimes overwrought) this change and to the meticulous studies of Anneliese Maier, who has corrected some of Duhem's enthusiasms and expounded the natural philosophy of the later scholastics in relation to their own outlook and metaphysical commitments rather than in the light of later mechanics. Writing in 1950, Dijksterhuis could not reflect the further studies of A. C. Crombie, notably on Grosseteste and his methodology of experimental science, and of Marshall Clagett, who has summarized much recent work in *The Science of Mechanics in the Middle Ages*, a book which is the indispensable starting point for anyone who would make an acquaintance with medieval achievements in physics [8]. The section on the seventeenth century, finally, occupies itself equally with cosmology and mechanics. Dijksterhuis puts very openly the dilemma which the historian must confront in organizing his material. Shall he follow straight through the lines of development of each of the sciences? If he does, the connections are slighted and so too are the scientific personalities of the great men. But if he follows a biographical approach, he will cut up the story of each of the sciences and lose its internal logic of development. Convergence of all the lines in Newton gives Dijksterhuis a goal, toward which he works by choosing with great discernment those parts of the work of the great figures—Copernicus, Kepler, Stevin, Galileo, Pascal, Descartes, Boyle—which reveal their qualities in the parallel lines of astronomy, cosmology,

statics, hydrostatics, mechanics, and the structure of matter.

The third major theme in this, the vertical aspect of Dijksterhuis' organization, opens to his treatment prospects comparable to those deriving from his horizontal particularization of Renaissance science as preliminary and distinct. He tells in each of the four periods of the prevailing theories of matter and its structure and discusses it as a subject equal in dignity and distinction to astronomy and the mathematicizable parts of physics, i.e., optics, statics, and ultimately kinematics. This permits him to work into the evolving fabric of mechanistic science the antecedents of those subjects which certainly have come to belong in it since the seventeenth century, that is chemistry and the physics of matter and force as well as of motion and the stars. Dijksterhuis treats alchemy and hermetic thought as considerately as his other topics. Alchemists there were, we are assured, who according to their lights truly sought to conform their thinking to what they learned in their experiments. Philosophers too in the Middle Ages concerned themselves with the problem of mixture, whether in a true *mixtum* the elements are still present, and if so then how this is to be understood.

By no means did that question disappear in modern chemistry, and the logicians of the fourteenth century worked out its alternatives with scrupulous subtlety, holding on the whole by the Aristotelian doctrine of natural *minima* (not to be confused with Democritan atoms since they undergo a qualitative change in mixture). Below that level bodies could not be divided without ceasing to exist, though whether qualities too had minimal intensities—motion, for example—was a vexatious puzzle. In all this pre-chemistry, however, Dijksterhuis attaches greater significance on the whole to "the voices of those who not only reflected upon the nature of a chemical compound, but also handled substances in practice and actually produced compounds or decomposed them into their components . . . . We shall therefore have to turn to the alchemists, not because they were the only people who were in a position to handle substances, but because the altogether practically minded technical workers, who also did so by profession, did not as a rule commit their findings to writing. The fact that the aim of the alchemist was to make gold is of no account in the present context. Indeed, alchemy can only be distinguished from chemistry by the objective which it set itself, and where this is irrelevant there is no longer any reason to maintain the difference of name" [9]. As to combination, moreover, the working chemists seem in practice not to have taken the Aristotelian view of combination as mixture occurring with transformation of the elements: they simply supposed themselves to be making some substances out of others and decomposing compounds into their original constituents. There, moreover, where Dijksterhuis' two unexpected themes cross, in the discussion of theories of matter in the Renaissance, he is at his most

felicitous with what others have found to be either an embarrassment or a cause to espouse. The juncture enables him briefly but naturally to place Paracelsus in the history of science, not as some Faustian *magus* posturing and declaiming (though no doubt he did), but as the transformer of chemistry who in making it the science of the "principles" of mercury, salt, and sulfur instead of the four ancient elements of merely philosophical fame thus brought it to closer grips with actual materials, and who, further, in turning the subject to medical account rather than to making gold enhanced its dignity and started its popularity as the science specially apt for giving access to the real workings of nature in body and in depth. In this wise all the hermetic tradition, the correspondences of macrocosm and microcosm, the sense of cosmic sympathy, the parallelism of stars and metals and their empire over fortune, the trinities on every hand—all that becomes background for the history of science and not (as this reviewer confesses he has always regarded it) simply as magic or superstition to be exorcised by reason and experience.

One must not leave the impression of excessive schematization, however, admirably adapted though the scheme is to the material, for it is not there that the excellence of the book resides, but rather in the reconciliation of breadth of theme with economy of statement, of urbanity in judgment with fullness in criticism, that most rare and welcome genre of criticism which values without blaming. On reading Dijksterhuis the historian of science will again and again enjoy the satisfaction of hearing struck the note one knows is right and could not have played oneself. Consider, for example, the observation on the blending of what might otherwise seem the antitheses of rationalism and mysticism in the Pythagoreans and by extension in Platonism and much highly abstract science since, how the ideal of pure knowledge can become also an escape from the imperfections of a spotted world, and how this tradition, albeit a stimulus to mathematicization, might also disparage the empirical study of nature, particularly in times when religion should happen to inculcate contempt for the material world. Nor will Dijksterhuis adopt drastic alternatives and categorically rule Aristotelianism out of any relation to the genesis of mathematical physics. He agrees, and there can be no dispute, that Aristotle generally stands for the a-mathematical aspects of natural science, but he also observes that the very structure of his axiomatization and demonstration is derived from mathematics, and that Euclidean geometry, Archimedean statics, and much of the best of seventeenth-century physics were written in the form of just such a demonstrative science.

Dijksterhuis emphasizes a general criticism of Greek science which, though one has not found it given such importance elsewhere, again arrests attention by its justice. This is a practice of valuing by which the philosopher or the educator is forever posing choices between polar

alternatives on grounds of relative nobility, perfection, or excellence—mind over matter, the finite over the infinite, the square over the rectangle, the male over the female, circular motion over rectilinear, the celestial spheres over the sublunary. Carried over into the Renaissance, this became the convention for the philosophic or aesthetic dialogue—painting over sculpture, poetry over prose. Does this habit of false and unreal choice persist in our own world, even if mainly in the realm of intellectual snobberies, as perhaps the least altered by the centuries of our legacies from Greece, so that the famous two cultures are only the latest installment of the issue between a liberal and a practical education, or so that the superiority complex of the theoretical physicist vis-à-vis his experimental colleague still expresses *artes liberales* (for such was geometry) lording it over *artes mechanicae* the which "do not befit the free Hellene" [10]? So pervasive is what he calls "axiology" that Dijksterhuis gives it an importance equivalent to the more familiar teleological point of view as a fundamental and detrimental characteristic of Greek thinking.

For though this is a measured book, it is never bland, never permissive of wishful scholarship. How moderately and yet how firmly Dijksterhuis discounts the fashion, prevalent not so long ago and by no means among Marxists only, which would make science one of the intellectual products of a process rising in the social relations of classes and in the methods of getting a living. According to Borkenau, for example, mechanistic science was a projection upon nature of the practice of standardized procedures in the early factory system. According to Simmel and others, experience with a monetary economy created the prospect of an exact and quantitative treatment of the universe. Alfred von Martin, sociologist rather than economic historian, has held that the ideal of pure theoretical knowledge was a function of the emancipation of the moneyed classes from feudalism and a special case of their pleasure in business for its own sake. "It is doubtful," writes Dijksterhuis mildly, "whether the history of science is enriched by such constructions" [11]. These and similar speculations fail to convince because, on the one hand, they do not demonstrate the causal relation they would establish, and on the other, their authors needs must pretend that no mathematical account of nature had ever been conceived prior to the influence of Renaissance capitalism. Not that Dijksterhuis wishes to neglect the demonstrable role of technology itself as distinct from its supposed social setting, and one may quote as an instance of incisiveness and restraint his paragraph on the practical relation to science of the instrument-makers, the cartographers, and the artist-engineers of the type of Leonardo, Brunelleschi, and Dürer:

> A good deal of the knowledge and skill displayed by these men was still purely empirical, but the constant handling of matter, which is always refractory, could not

fail to stimulate the desire for a causal explanation and to induce efforts to devise a more rational working-method. It thus becomes understandable that the first branch of science in which the revival was to take place was mechanics (at first still in the sense of science concerned with tools and implements). In this case empirical knowledge did not have to be sought deliberately, but arose naturally from the pursuit of technical trades; the waiting was only for theoretical reflection, which, however, was helped by the fact that there is no single department of physics which calls more urgently for mathematical treatment and lends itself to it more naturally than mechanics. The first essential element of classical physics, the mathematical approach, thus came into its own as spontaneously as the second, the empirical foundation [12].

Dijksterhuis is no more indulgent when it is the scientists who habitually abuse the facts. His discussion of Galileo in no way diminishes that extraordinary and provocative man's stature there at the beginning of the battle for classical physics, but he gives the real Galileo, indicating the Aristotelian moorings from which his thought broke loose, the persistence even in *Discourses on Two New Sciences* of fragments from the earlier essays, and the inconsistencies and occasional failures in strict frankness. Nothing is more difficult than to say just what his greatness consisted in, just what his contribution was, and this is because he was so embedded in a past which he vehemently denounced in all his works:

The matter is complicated even further by the fact that besides the genuine, though often one-sided, portraits of Galileo based on the study of his works there exists a spurious picture presented by what may be called the Galileo myth, the current popular representation. This latter is the picture that has been produced and is perpetuated by writers on modern physical science who feel the need of an historical introduction, but who omit to perform the simple duty of exactness, which consists in verifying their statements in the original sources. This picture is totally false, but it is much more vivid than any of the genuine ones, and consequently many readers will be inclined to be content with it. Moreover, it tends to simplify matters greatly: its splendour eclipses all secondary figures. It also furnishes a simple terminology because, whenever a term is needed to characterize the special features of classical science, the adjective Galilean at once suggests itself. It is, therefore, comprehensible that any criticism of this idealized picture tends to cause annoyance . . . [13].

The entire book is a preparation for the chapter on Isaac Newton which concludes the narrative. Dijksterhuis has no thought to question the consensus by which Newton's work is taken to complete the genesis of classical physics and to initiate its career. If any position is secure in the history of science, surely it is that. Nevertheless, this is a remarkable chapter. The felicity with which Dijksterhuis combines the veins of exposition and criticism here finds its worthiest occasion. Whatever one's standpoint in regard to Newtonian physics, neither the historian of science nor the physicist who reads attentively will again say quite the same simple things about Newton's own physics. What, to take only the most notable example, are we to understand in the literal sense of Newton's Laws after Dijksterhuis' dissection of the axiomatization of the *Principia*?

In the *Principia* what are usually taken as enunciations of the classical principle of inertia, of force in terms of mass and acceleration, and of the

equivalence in opposite directions of action and reaction appear as Axioms or Laws of Motion. Of the first of them, the principle of inertia, Dijksterhuis observes that Newton had already stated its content in his Definition III, which reads, "*The* vis insita, *or innate force of matter, is a power of resisting by which every body, as much as in it lies, continues in its present state, whether it be of rest or of moving uniformly forwards in a right line. This force differs nothing from the inactivity of the mass, but in our manner of conceiving it.*" In Dijksterhuis' view (very carefully worked out) this means that Newton did not in fact hold the concept of inertia universally attributed to him, i.e., that rectilinear motion is a persistent state of matter requiring no cause or explanation. What he says is rather Aristotelian, and it is that every motion requires a motor, the *vis inertiae*, identical in function with the "impetus" of the fourteenth-century schoolmen and with what Galileo in his turn had called *Vis Impressa*.

What is to be appreciated is that Newton like every creative scientist wrote in full ignorance of the future of his science and in full knowledge only of its legacy. Even more startling than Dijksterhuis' assertion that he did not think of classical inertia is the further analysis which finds no warrant in the *Principia* for what became the expression of the second law, the relation that equates force to the product of mass into acceleration. Again Dijksterhuis goes back from the axioms to the Definitions, where Newton says what he means by his terms. Axiom II lays down: " '*The change of motion* [i.e., of the quantity of motion or momentum] *is proportional to the motive force impressed, and is made in the direction of the right line in which that force is impressed.*' " Definition VIII says in effect that a force produces what we call momentum in some certain time and that two forces are to each other as the momenta produced in like times.

The question is, do these statements taken together assert the same thing as the relation $\underline{F} = m \times \underline{a}$? And it is Dijksterhuis' view that they do not. He puts it thus, to paraphrase him very closely: begin with Definition VIII, in the case in which a constant force F acts for time t upon a mass-point m starting at rest. Then from

$$F = m \times a \qquad \text{and} \qquad v(t) = a \times t$$

it follows that

$$F \times t = m \times v(t)$$

or that force is proportional to the momentum developed in a given time. But one may not reverse the conclusion. For example, suppose that a constant force F produced an acceleration proportional to time, such that these relations applied

$$F = m \times c \qquad \text{and} \qquad a = c \times t;$$

then in motion beginning from rest, velocity would vary with time thus,

$$v(t) = {}^1/_2\, ct^2$$

so that

$$F \times t^2 = 2 \times mv(t),$$

and again force is proportional to momentum developed in a given time. From this it appears that for Newton's Definition VIII and Axiom II to be valid, the condition that $\underline{F} = m \times \underline{a}$ is sufficient but not necessary. We may not, therefore, take Newton's language to mean what it did not say, i.e., that it is a law of motion that force should equal mass times acceleration [14].

Indeed, Dijksterhuis describes reading the classical force law, $\underline{F} = m \times \underline{a}$, into Newton as a case of the Emperor's clothes. Thus to construe Axiom II one has to suppose that Newton means "rate of change of motion" when what he says is that "change of motion is proportional to the motive force impressed. . . ." Nor need that be imagined if one abandon the notion, for which again there is no warrant in the language, that in speaking here of forces Newton has in mind continuously acting forces, notably gravity. In Dijksterhuis' view he is not at all thinking of gravitational or centrally attractive forces in the second axiom, but only of instantaneous forces and particularly those of impact. This is why there is no reference to time in the statement of the second law. Both the history and the corollaries of the laws of motion and impact bear out the validity of this restriction. It is a very serious one since it means that Newton's own Second Axiom or Law of Motion says nothing about the proportionality of force and acceleration. And if it be urged, as properly it may be, that Newton in all his discussion of gravity knows very well that a constant force creates uniformly accelerated motion, Dijksterhuis answers that like Huygens and indeed Galileo before him, Newton nowhere argues this or provides for it in axiomatization but considers the fact simply as self-evident. Only later when his successors did him the service—or disservice?—of supposing that for change he must have meant rate of change, only then was his Second Axiom taken as asserting their Second Newtonian Law of Motion. Truly, Dijksterhuis observes of the *Principia*, "like other fundamental works in the history of science (one thinks of the writings of Copernicus and Kepler) it lacks the qualities required of a good textbook. If anyone attempted to learn Newtonian mechanics from it, he would be faced with grave difficulties" [15].

This by no means exhausts Dijksterhuis' criticisms either of Newton's axiomatization or of other features of the Bible of classical physics. In the midst of it he seems to remember as it were that he is writing for those unaccustomed to the give and take of scientific dialogue, and

his explanation to them may, perhaps, be taken as an apology for all that realm of discourse which is so foreign to the ways in which men normally exchange their thoughts and in which, nevertheless, it seems likely that a rapidly, perhaps a logarithmically increasing volume of the world's affairs are being and will be carried on:

> This critical discussion of the axiomatic system on which Newton founded classical mechanics may well have appeared a little astonishing to readers without a mathematical training. First Newton is pictured as the universal genius who inaugurated a new era in the history of thought, as the founder of a science which was to lead men to a previously unsuspected insight into nature, and to enable them to achieve technical wonders surpassing the boldest dreams of the past. And then the manner in which he dealt with the principles of the science on which all this is based is severely criticized. If the first statement is true, is not the second devoid of importance? Does it not amount to petty cavilling to point out imperfections in the introductory part of a work every page of which testifies to the brilliant way in which the author handles the new conceptions?
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> The mathematically trained reader, however, will understand that what counts in mathematics is not only the efficacy of the developed methods, but also the exactness of their motivation, and that the unremitting work of strengthening the foundations on which the edifice of mathematics rests is of no less significance than the erection of the superstructure. Such a reader does not merely want to be able to speak and act, but also wants to know what he is saying and on what grounds his actions are based [16].

Nor does Dijksterhuis evade the question he posed in the beginning for this last observation brings out what mechanization has been about. It is in the connotations of the word "mechanics" that the confusion and the disputes arise, and not in the content of the science itself. Like the other words which physics draws from common experience, it must be stripped of its etymological associations if its bearing is to be seized. We have no longer to do with mechanical models nor with ontologies of motion, nor with soul or mind or with their absence: "classical mechanics is mathematical not only in the sense that it makes use of mathematical terms and methods for abbreviating arguments which might, if necessary, also be expressed in the language of everyday speech; it is so also in the much more stringent sense that its basic concepts are mathematical concepts, that mechanics itself is a mathematics. . . . We can then give a positive answer to the question we set ourselves in the introduction to this book. The mechanization of the world picture during the transition from ancient to classical science meant the introduction of a description of nature with the aid of the mathematical concepts of classical mechanics; it marks the beginning of the mathematization of science, which continues at an ever-increasing pace in the twentieth century" [17].

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12. *Ibid.*, p. 243.
13. *Ibid.*, p. 334.
14. This discussion on pp. 466–471.
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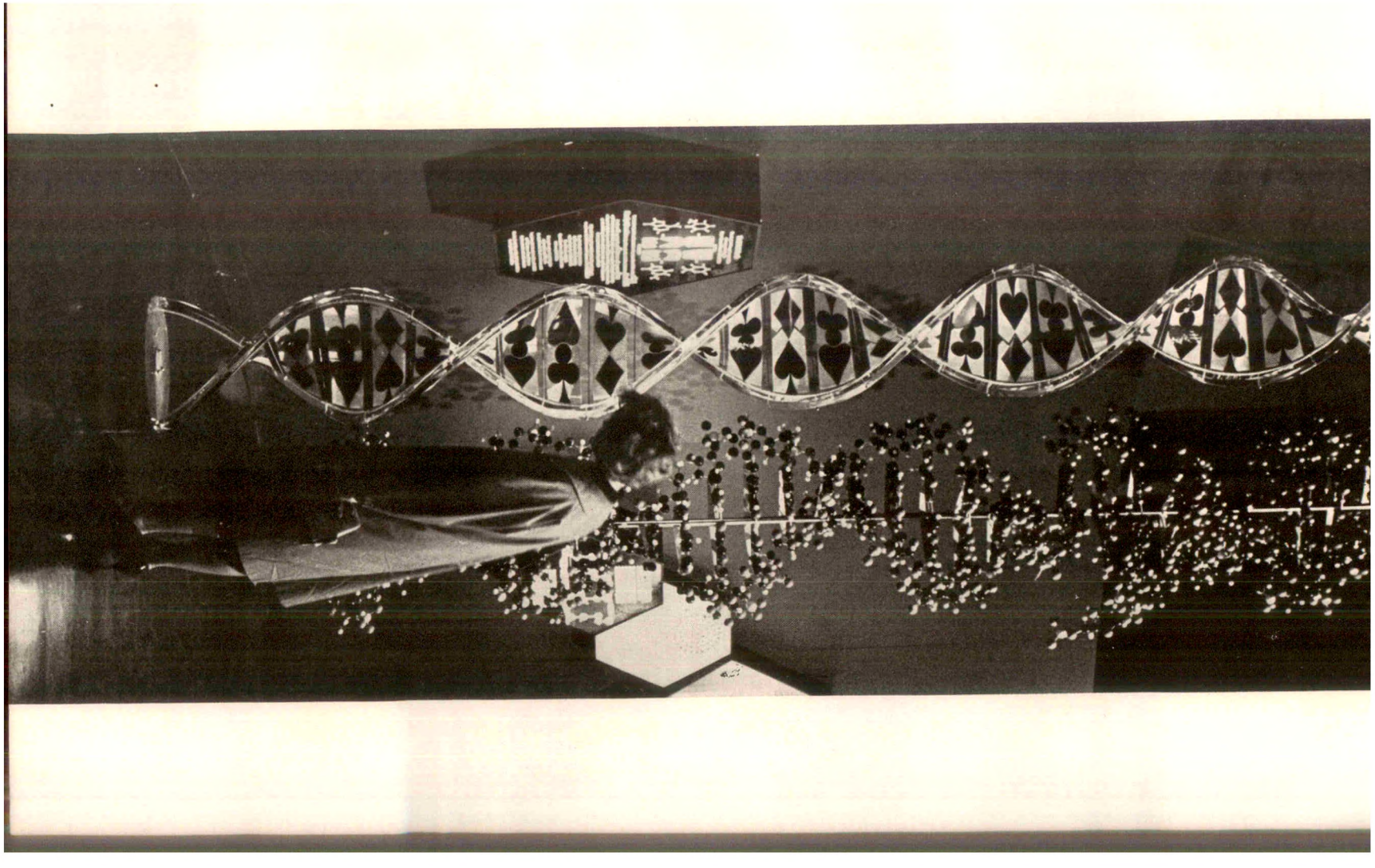
HERE AND THERE

By the Board of Editors and the Membership of The Sigma Xi-RESA Societies

Through the courtesy of the American Association for the Advancement of Science, we present with permission, on the opposite page, a reproduction from Volume 7, No. 2, AAAS Misc. Pub. No. 62-5, June 1962. The Bulletin highlights the Seattle World's Fair.

> The illustration is of a 16-foot high model of a small fraction of a DNA molecule. The model revolves in order to display its spiral structure. It is based on Watson and Crick's concept of a ladder of alternating pairs of chemical bases. The model at the left is a rung-for-rung translation of the molecular model at the right of the illustration. It uses George Gamow's device of visualizing the four bases, adenine, guanine, cytosine, and thymine as four card suits. This makes it possible to describe base-pairing simply: "Diamonds can pair only with spades and hearts only with clubs."
>
> The text attached to the illustration added this further information: "In this model, it is the very infrequent mistakes made in picking up and pairing cards to complete a new chain ladder that accounts for mutations."
>
> The model was at the end of a walk through "Darwin's Forest." The forest corridor led to a sequence tracing contributions to knowledge of heredity by Mendel, Morgan, Müller, Beadle, Tatum, Griffith, Avery, Macleod, McCarty, and ending with the Watson-Crick Model of DNA.
>
> The Nobel Prize in Medicine and Physiology for 1962 has been awarded to Drs. Francis H. C. Crick, James D. Watson and Maurice H. F. Wilkins for their work in connection with the structure of deoxyribonucleic acid, DNA. Dr. Watson of Harvard University and Dr. Crick of Cambridge University worked out the theoretical aspects of the DNA model. Dr. Wilkins of the Medical Research Council biophysics research unit in London, using X-ray diffraction, provided the physical evidence for the double-helix structure illustrated on the opposite page.

The distinguished biologist, author and member of the French Academy, Jean Rostand, has had two of his books combined in an English translation entitled **The Substance of Man,** published by Doubleday ($4.95,



1962). The book is a collection of thought-provoking aphorisms rooted in Rostand's reflections on human life in the light of our present biological knowledge. These are pithily and pungently expressed, and do not dodge controversial issues. Some examples follow.

> A great writer is a man who knows how to surprise by telling us what we have always known (page 119).
>
> Our inherited substance, which makes us ourselves, is composed of thousands of small elements fortuitously matched and more or less fortunately grouped. We are a bric-a-brac construct. If a single one of the elements in the egg from which we came had been in the slightest particular different from what it was, or had merely changed position, the man born from it would have been another. What is this "I," Pascal would have asked, whose identity depends upon the nature or position of an atom! Nothing can be of more moment to us than the rigorous machinery that controls the distribution of the hereditary patrimony. Yet, as I observe with a certain astonishment, most of us beget our children without a moment's qualm over the material connection between them and us. To the biologist, fatherhood presents itself in quite another light than to the layman. It is a game of heads and tails played with the chromosomes of one's forebears; it is a time-extension allowed to half one's being; it is the late gift to oneself of a half-twin. Perhaps I speak out of character professionally, but it seems to me that a clearer awareness of just what our child is in its relation to us might bring a more precise shading to our ways of loving him. The notion of one's own obliteration is less repugnant when half one's chromosomes have been passed on to livelier flesh (page 8).
>
> The man who denies biological determinism probably does not know that certain actions are as clearly denied to certain men as are brown hair or a flat nose, when the appropriate germinal conditions are not present. Does he imagine his actions can change while he remains organically the same? Those who wish to preserve the notion of individual responsibility would, I think, have to believe that one is responsible for one's chromosomes (page 16).
>
> Men of the twentieth century, however clothed, policed, and subtle, are substantially, chromosomally, identical with the stonecutters of the Pleistocene Age. Living anachronisms, their flesh is contemporary with that of the mammoth and the rhinoceros. They can, to be sure, mold themselves to the complex demands of modern society, since it was men of their sort

who set it up; but they are no more inclined toward justice, intelligence, peace, or altruism than their cave-dwelling forebears. "The human species," wrote Bonald, "is reborn with each generation." The infant man who, at this moment, is on the point of opening his eyes on the civilized world is precisely what he would have been had he been born a hundred thousand years ago from a similar grouping of chromosomes. No stage has been skipped for him. In some twenty years he will be expected to have covered the course that took millennia. The child of two individuals, not of two "social entities," he comes from as far off as the first thinking men. His mother rocks a belated Cro-Magnon in her cradle (page 22).

The fact that the flesh of the species does not mature yields an argument for the social usefulness of the aged. Humanity, since it remains eternally young, has all the more need of its old men (page 24).

Will man, without progressing further, be able to go on indefinitely satisfying the requirements of civilization? If the gap between what he is at birth and what he is asked to become goes on increasing, will he not grow weary of having more and more to repress the beast in order to counterfeit the angel? Won't he then eventually rebel against demands that are too lofty for him, and ideals that are too austere? Without doubt the capacities of the human chromosomes have a limit. But nothing yet compels us to think this limit has been reached. One can have confidence in a genetic equipment that has managed to pass, in a few dozen millennia, from the cave age to the age of laboratories (page 24).

From: **The Substance of Man** by JEAN ROSTAND; Copyright © 1962 by Doubleday & Co., Inc.; Reprinted by permission of the publisher.

GRANTS-IN-AID OF RESEARCH

The attention of the membership of the Society of the Sigma Xi is called to the fact that **ALL** correspondence with respect to Grants-in-Aid of Research should be addressed to the Executive Secretary, Professor T. T. Holme, at 51 Prospect Street, New Haven 11, Conn. Inquiries addressed to the Editorial Office of the American Scientist result inevitably in unnecessary delays.

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Kodak reports on:

separating metal ions . . . boron-barium-lanthanum-thorium-strontium glass with neodymium . . . a little sensitivity for science from business

TOPO in Milan

Pliny gets credit for about 1900 years of priority on chromatography. He told how to use papyrus impregnated with gallnut extract for the detection of ferrous ion, although he didn't say it just that way and didn't call it chromatography. Today we need words like that. Otherwise if you are seen watching blots spread on blotting paper, people will question your right to earn a living by such activity.

Paper chromatography has made a leap forward. Don't think of it as a technique on which biochemists have some kind of exclusive contract. Biochemists took to it because it is a powerful and ridiculously simple way of separating closely similar components of a mixture when you don't have much of the mixture. It was known to be useful also for separating metal ions, but you heard less about that. Now you may hear more. TOPO (*Trioctylphosphine Oxide*) did it.

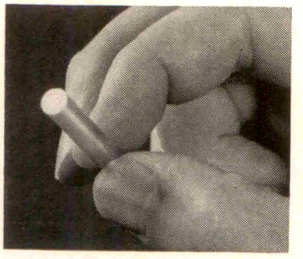
They love our TOPO in Milan and are impregnating filter paper with it (*Journal of Chromatography*, *7*, 112). We had put it on the market in '57 after workers at the Oak Ridge National Laboratory, who had studied the solvent extraction merits of various phosphine oxides for 49 different elements, had picked TOPO for complexing hexavalent uranium.

The Milanese appear to have fixed their attention on the wide and well-spaced *differences* in the affinity that TOPO displays toward various metal ions. For about 50 of them their paper gives curves of R_F against acidity of the eluent. A full set of curves is presented for each of the eluents HCl, HNO_3, and H_2SO_4. Also operating procedures down to fine details.

Bravo Milanesi! Bravo TOPO! Bravo Oak Ridge! Want an abstract? Want any other of some 4000 EASTMAN Organic Chemicals? Request List No. 43 from Distillation Products Industries, Rochester 3, N. Y. (Division of Eastman Kodak Company).*

*The word can be pronounced "EASTMAN 7440" to avoid the embarrassment of shouting in Italian, "Hurray for the mouse!"

Laser logic



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THE SCIENTISTS' BOOKSHELF

By Hugh Taylor, the Associate Editors, and Guest Reviewers

An Introduction to Physical Oceanography by W. S. von Arx; 422 pages; $15.00; Addison-Wesley Publishing Co., 1962.

This book is designed for a one semester course in physical oceanography and is based on the author's experience in teaching such a course at M.I.T. It is the first such textbook to be published in English, and it fulfills a long felt need.

In preparing such a text the author must first decide (*a*) what level of mathematical sophistication should be presumed of the reader and (*b*) what aspects of physical oceanography should be emphasized. The book is nearly free of mathematical derivations and should not prove troublesome to anyone with an introductory calculus course. Considering the heterogeneous background of students in beginning oceanography courses the choice is a reasonable one. von Arx solved the second problem by emphasizing those portions of physical oceanography in which he is most involved. Three chapters (nearly a third of the book) are devoted to methods of measuring currents and to laboratory models of the ocean circulation. These are areas in which the author has made many major contributions in the past fifteen years. However, he has covered the problems of waves and tides in a single chapter of thirty pages and has completely ignored underwater sound. Both are highly developed fields of physical oceanography and it is believed they should be given better treatment in any introductory text.

Accepting these limitations the book provides a fine introduction for those aspects of physical oceanography which it treats. The book is divided into eleven chapters. The first two are concerned with historical and introductory material. The third is on waves and tides. The fourth is on fluid mechanics and includes the best "non-rigorous" explanation of the Coriolis force this reviewer has seen. The fifth discusses some of the physical properties of sea water and their consequences in the ocean. The sixth and seventh are on the advective and convective processes in the ocean. The next three chapters, as noted previously, are on methods of measuring currents and the use of laboratory models. The last chapter is on the "Gulf Stream problem" and, as another reviewer has remarked, the entire book can be considered an introduction for this discussion of our attempts to describe and understand this strongest of all ocean currents.

The book contains several appendices, the most unusual of which is the first: "Some events that have influenced thought in the marine sciences," and includes more than 250 items beginning with Thales concept of the earth floating in the water in 600 B.C. and ending with the invention of the neutrally buoyant float for measuring ocean currents by Swallow in 1955.

The book is amply illustrated, emphasizes current concepts in oceanography and has a list of recent references at the end of each chapter. For those students who find the text material of insufficient challenge there are a set of study questions at the end of each chapter, at least some of which are certain to prove taxing to the professor as well as the student. The ideal introductory text in physical oceanography is yet to be written, but we should all be grateful to von Arx for finally providing us with a good one.—*John A. Knauss*

Advances in Electrochemistry and Electrochemical Engineering, Vol. I: Electrochemistry, edited by P. DELAHAY; 326 pages; $12.00; Interscience Publishers, 1961.

Such a book is welcome because of the rapid expansion of the field. It resembles, even in choice of subject matter, an already existing series of monographs: "Modern Aspects of Electrochemistry," of Academic Press, 1954–59. Articles I, II, IV, and V are on the same advanced level. Article III is on a rather lower level.

(I) *The Structure of the Double Layer*, by R. Parsons. This chapter is concerned with relatively small advances in knowledge of structure made during the last few years, and the considerable progress arising from connecting this knowledge to that of the kinetics of reactions on mercury. (II) *Hydrogen Overvoltage*, by A. Frumkin. This is a masterfully detailed account of double layer effects upon the most studied electrochemical reaction. (III) *Oxygen Overvoltage*, by M. Breiter. This is a brief account. The discussion of coverage effects on Tafel lines is misleading. Certain equations lack recognition of stoichiometry. (IV) *Semiconductor Electrode Reactions*, by H. Gerischer. This is a comprehensive and excellent review of a new field. It is a pity that (pages 150–1 and 157, 158, 160) results are quoted from the earlier work of Green without acknowledgment. (V) *Fast Reactions*, by P. Delahay. This chapter describes instrumental techniques whereby faster and faster reaction rates can be measured.

The editing and English are sometimes rather rough, but detract little from a valuable collection of reviews which no one with electrochemical interests should be without.—*J. O'M. Bockris*

Theoretical Geomorphology by A. E. SCHEIDEGGER; 333 pages; $18.00 text edition; Prentice-Hall, 1962.

It is perhaps customary for books dealing with late advances and new applications in a scientific field to be written by a recognized worker in that field, and to be understandable to the majority of persons doing research in the field. However, when the subject field, geomorphology, is in that critical stage of an art striving to become a science, it is not too surprising to find that a book written by a "rank outsider" contains a great amount of material borrowed from related disciplines that in all probability will neither be accepted nor understood by the majority of geomorphologists. There may be some question as to whether all of it *should* be.

Most of the standard sort of geomorphic exposition is contained in the first thirty-seven pages of the book—Chapter I, "Physical Geomorphology." As a summary explanation of common geomorphic terms it is fairly complete, but the interrelations among various elements of landscapes are not examined adequately.

The remainder of the book deals successively with the general physics of motion and processes in or on slopes, river beds, drainage basins and valleys, large bodies of water, shorelines, glaciers, and air moving over unconsolidated sand. A final chapter deals with "special" features such as caverns, geysers, hoodoos, and nuées ardentes. The usual procedure is to set up a system of differential equations that describe the velocity potential or force fields assuming simple conditions within the body of water, ice, air, or whatnot. A major defect with this approach is the frequent failure to close the gap between the equations based on physics and the properties of the landforms. One is left still wondering why the world's scenery looks as it does. For that reason the title of the book is a misnomer, and might better be *Mechanisms of Transporting Agents*. In too many cases the mechanisms treated have no apparent bearing on land sculpture and their inclusion would seem premature. Further, it must be remembered that landforms have a history that may be fully as important as modern processes; this aspect is not considered.

With its limitations kept in mind, the book may be useful in advanced

courses or seminars in geomorphology as a convenient reference. Errors of oversimplification are numerous. The style is colloquial but acceptable, and it is a decided pleasure to see the ease with which the mathematics are handled. The author draws from an impressive number of foreign works, including many Russian references, and some of the material appears here for the first time in English.—*Mark A. Melton*

The Mathematical Theory of Linear Systems by B. M. BROWN; 267 pages; $8.00; John Wiley & Sons, 1961.

So many books on linear systems have appeared that some justification is in order for another, especially since the greatest attention presently needs to be given to nonlinear methods. This introductory text is sufficiently different to deserve notice. Its author is a mathematician with an obvious interest in automatic control, who writes clearly for the scientist or engineer but emphasizes mathematical generality rather than details of practice. At the same time he avoids rigorous proofs in favor of shorter intuitive reasoning. The result is a rather efficient exposition, including more topics than are found in most elementary texts on linear systems. The books approach is most evident from its use of the operator D throughout and its relative avoidance of transformed variables.

The first half of the text contains the standard elementary topics. In accord with the mathematical emphasis, linearity is first presented in terms of the linear equation's form; only much later the important physical concept of superposition is noted, in connection with a good generalized discussion of linear operators. The unilateral and bilateral Laplace transforms are explained; the bilateral transform is used through most of the book. The second half is written at a much more advanced level and considers statistical methods, system optimization, difference operators, general linear operators, sampling, and interpolation methods. Again illustrating the mathematical approach, the z transform is presented as multiplying by "a function z^{-r}" and summing, in connection with difference operators, and is only applied late in the chapter on sampling. But as with most topics in the book, all the important aspects of the transform are noted.

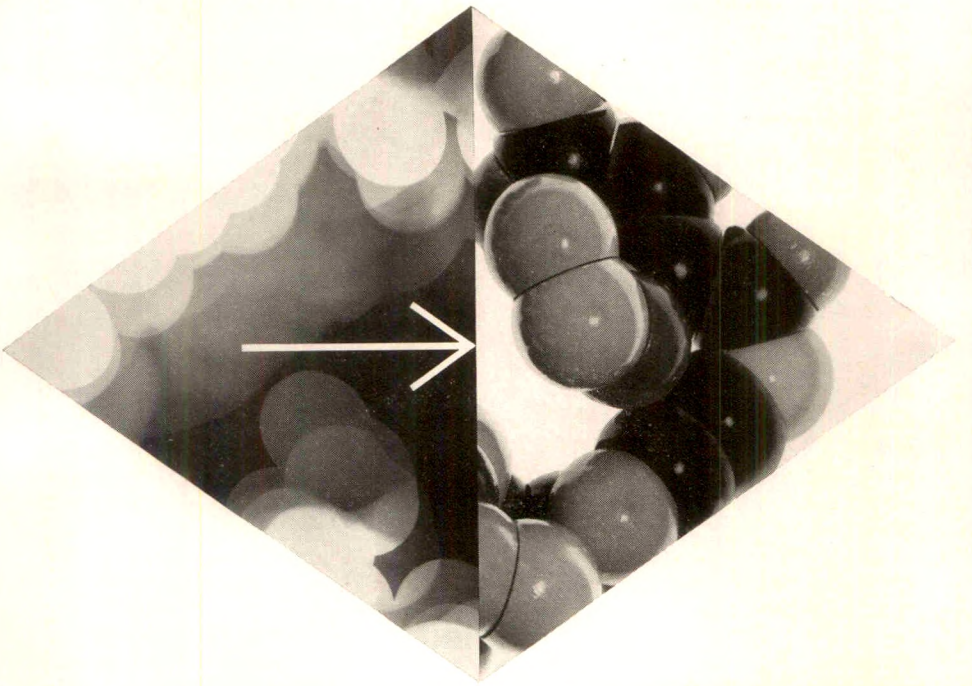
For mathematical rigor, or for engineering practice, other texts should be used. But as a mathematically oriented and very complete introduction to linear systems analysis, this work is an interesting contribution.—*Peter L. Balise*

Stability by Liapunov's Direct Method with Applications by Joseph LASALLE and SOLOMON LEFSCHETZ; 134 pages; $5.50; Academic Press, 1961.

Because no physical phenomenon is entirely linear, and because it is often advantageous purposely to introduce nonlinearities into systems, nonlinear methods of analysis are receiving great impetus. The difficulty of solving nonlinear differential equations has led to the use of various approximations, and no single technique seems dominant in the United States. However, in the Soviet Union the "second method" of Lyapunov (first published in 1892) is pre-eminent for studying the stability of nonlinear control systems.

Lyapunov's "first method," not of concern here, investigates stability via explicit solutions of the differential equation. The powerful second or "direct" method simply states that if a positive definite function V of the system state variables can be found such that dV/dt is negative semidefinite, the system is stable; if dV/dt is negative definite, the system is asymptotically stable. This is intuitively reasonable since if a system's energy continuously decreases, the condition is obviously asymptotically stable; and if the energy is constant, a stable limit cycle has been reached. However, energy is only a special example of a V function; more general and complex functions are usually required. In fact, the problem is to construct a suitable function if it exists, for which only

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Lyapunov's second method is the subject of some papers and parts of books, and at least one Russian book translation. But this monograph is apparently the first American book written to present the method to anyone with moderate mathematical competence. Beginning with a brief review of matrices and other appropriate background, it develops the theory logically, giving attention to physical interpretations and applications. The writing style is attractively informal, with good illustrations. The authors are to be complimented for what they have done to bring wider attention to an important subject.—*Peter L. Balise*

Medical Genetics 1958–1960, An Annotated Review by V. A. McKusick *et al.;* 534 pages; $14.50; The C. V. Mosby Co., 1961.

Recent progress in human genetics has been so rapid, and at the same time so highly diversified, that neither the production of new textbooks nor the revision of outdated ones has been able to keep up with current advances. In order to fill this gap for the time being, McKusick and his co-workers have resorted to a rather ingenious scheme, that of collecting in a single volume abstracts of a great number of separate publications. These reviews had appeared previously in the *Journal of Chronic Diseases* during the most crucial period of modern medical genetics: 1958–1960.

Covering basic as well as clinical data, the reviews range over a much wider territory than might be expected from the title. Within this broad area, they are arranged by years and subdivided according to subjects. There are sections dealing with new books, symposia and the proceedings of important congresses, while others deal with the history and methods of human genetics, with biochemical and cytological genetics, with congenital malformations and special tissue cultures, and with a variety of clinical syndromes. One of the minor imperfections of the volume is the absence of a list of journals covered by the reviewers.

Being primarily concerned with causation, and the causes behind causes, the editors of this "annotated review" have focused their attention on metabolic defects and the different ways in which deficiencies become manifest in the individual's phenotype. They have certainly succeeded in compressing an enormous amount of technical information into a limited space, without losing the reader's interest or overtaxing his power of concentration, and have thus produced a well-illustrated book (with more than 100 figures and tables) that is much more than a compendium of abstracts. Their comments are always relevant and enlivened by a wit which usually is truly humorous and occasionally somewhat caustic.

On the whole, the volume assures clinicians and behavioral scientists interested in human genetics of pleasant and informative reading. It will prove to be valuable as a reference book, and the achievement of this goal should encourage the editors to continue this new series of publications.—*Edward V. Glanville, Franz J. Kallman*

Physics and Astronomy of the Moon, edited by Zdenek Kopal; 538 pages; $16.50; Academic Press, 1961.

The possibility that men will land on the moon before 1970 has stimulated a great deal of interest in the physics and astronomy of the moon. If this possibility did not exist, it is doubtful whether this book would have been written because, in the absence of a space program for lunar exploration, very few people (including astronomers) would have paid $16.50 for a book on the moon. Most of the copies would have been sold to large academic libraries and would have been read only by those few astronomers who were engaged in lunar research. Now, however, things are different, for this book will, undoubtedly, soon be found in the library of nearly every space-oriented company. Moreover, it is likely that a fair number of copies will be sold to space engineers engaged in designing equipment to land on the moon. Indeed, a few astronomers may even buy a copy.

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The thirteen chapters of this book were each written by an expert in a certain area of lunar research. The first three chapters are devoted to the celestial mechanics of the moon. Other chapters devoted to measurements of the moon's radiation summarize the photometric properties of the moon, the polarization of moonlight, lunar temperatures, luminescence of the moon's surface, spectroscopic observations of a suspected volcano, and observations of lunar eclipses. The remaining chapters concern the topography of the moon, the formation of lunar craters, radar studies of the moon, and the origin of the moon.

The structure of the lunar surface is an interesting question which is of great importance in designing vehicles to land and travel on the moon. Evidence is presented in several chapters which points clearly to the conclusion that the moon is completely covered with a layer of dust or powder. Polarization measurements suggest that this powder is very similar to volcanic ash. The rapid change in the surface temperature during an eclipse (measured by the lunar infrared radiation) compared with the absence of any detectable temperature change a foot or so beneath the moon's surface (measured by the radio waves emitted from the moon) corroborates the conclusion that a highly insulating layer of dust covers the moon's surface. The thickness of this layer is uncertain, however, and it is not likely to be definitely settled until instrumented or manned vehicles land on the moon.

This book adequately fills the need for an advanced reference book. That it is authoritative is, of course, insured by the specialists who wrote it. One criticism that may be made is that the reproductions of photographs could have been improved considerably if white plates had been used instead of the slightly tinted paper that the book is printed on. A comparison with the plates in *The Moon* (see the following review), a popular book on the moon written by the editor of this book, illustrates this point. Nevertheless, the *Physics and Astronomy of the Moon* is a very valuable reference book which symbolizes the renaissance of the moon as an exciting area of astronomical research.—*Robert E. Danielson*

The Moon by ZDENEK KOPAL; 131 pages; $4.50; Academic Press, 1961.

This book does not contain any mathematical formulae as it is intended to be a popular book about the moon. Yet the second chapter contains quite a potent dose of numerical facts and figures for the layman who is not at ease with exponents or with the metric system of units. However, for a scientist who is not familiar with the moon, this chapter makes quite pleasant reading. In any case, the lay reader can always skim through the second chapter and proceed to the next chapter without difficulty.

Here the author describes the surface features such as craters, rays, maria, and mountains with the aid of several good photographic plates of the moon. At this point, the inclusion of a small map of the moon would have been quite helpful in identifying features on the photographs. The author goes on to point out a feature which will come as a surprise to most people. Contrary to the impression of a very rugged lunar terrain gained from photographs, the moon's surface is really quite flat. As a matter of fact, there appear to be no slopes anywhere inclined more than ten degrees. What appears to be a sharp pinnacle on the photographs is just a flat hill on the moon. Furthermore, it is pointed out that an observer standing in the middle of Clavius, the largest crater, will not see the rim of the crater because of the large curvature of the moon's surface. Thus it seems that good lunar scenery will more closely resemble "desert area with a few mesas showing in the distance" than rugged terrestrial mountain terrain.

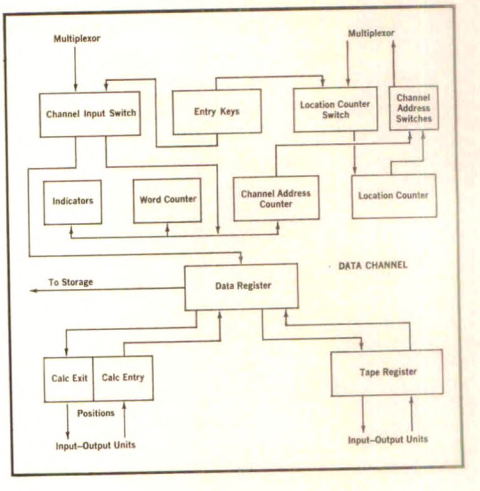
In the remainder of chapter three, the author discusses the origin of lunar craters and straddles a long-existing controversy by concluding that both meteoritic impacts and internal (volcanic) ac-

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                00067   0 76000 0 00006          COM
                00070   0 36100 0 02000          ACL  COMMON
                00071   0 76000 0 00006          COM
                00072  =0 53400 4 00106          LXD  WTAPE+70,4
                00073  =0 10000 0 00077          TNZ  WTAPE+63
                00074  =0 76000 0 00012          RTT
                00075   0 00000 0 00077          HTR  WTAPE+63
                00076   0 02000 4 00004          TRA  4,4
                00077  =0 53400 4 00106          LXD  WTAPE+70,4
TD              00100   3 00000 0 00105          TXH  WTAPE+69
                00101   0 50200 0 00100          CLS  WTAPE+64
                00102   0 76400 0 00302          BST  *
                00103   0 76200 0 00323          RTB  *
                00104   0 02000 0 00040          TRA  WTAPE+32
                00105   0 02000 4 00003          TRA  3,4
                00106   0 00000 0 00001          HTR  1
                00107  =0 50000 0 00100  CTAPE   CAL  WTAPE+64
                00110   0 02000 0 00102          TRA  WTAPE+66
                                02000  COMMON     SYN  1024
A                               00000            END
 0SHARE ASSEMBLER STATISTICS
 0TAPE   TOTAL        1 FAIL        2 FAIL        3 FAIL        4 FAIL
   INP      81             0             0             0             0
   LIB       0             0             0             0             0
   COL      81             0             0             0             0
 0NUMBER OF ON-LINE INPUT RECORDS          0
 0NUMBER OF OFF-LINE PRINT RECORDS        89
 0NUMBER OF SYMBOLS,  DEF     4,DEFOP      0,UNDEF      0
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This type of written input-output program is relatively time-consuming and costly to prepare, particularly when input-output routines are used repeatedly.



Input of routine data with a unit such as this IBM 7090 data channel reduces program writing, speeds up processing, and cuts the cost per answer.

In an effort to increase efficiency, more and more instructions have been built into computers in the form of circuitry. This means fewer written programs are necessary. However, built-in computer instructions that facilitate the solution of a particular type of problem may limit the computer's capacity to handle a variety of problems. Because of this, IBM is studying new ways of organizing data processing systems. The goal is to improve the speed and proficiency of specialized problem solving without sacrificing the flexibility of general-purpose machines.

Computer architects at IBM are attempting to achieve the most efficient relationship of built-in instructions and programming systems to the range of problems to be solved. They are transferring many input-output and programming operations to built-in circuitry. One example is the IBM 7090 data channel shown above. In addition, they are developing common languages which make it possible to use the same program on different machines. At the same time, they are working to increase over-all speed by developing time-sharing and concurrency techniques that make greater use of the entire system. For example, IBM programmers have developed a method by which a large computer can handle a number of problems at once, thus reducing the cost per answer. This involves a supervisory program that monitors the execution of the multiple tasks assigned to the central processor. In addition to these multiple problem-solving techniques, an experimental system permits the computer to handle several different programs simultaneously. From developments such as these will come the advanced architectural techniques necessary for a new generation of computers.

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tivity have formed craters on the moon. In chapter four, the author describes how the temperature of the moon is established followed by a discussion of luminescence of the moon. Chapter five discusses, among other things, the observations and reasoning which leads to the conclusion that the moon's surface is covered with a layer of dust (see the preceding review). Chapter six describes spectroscopic observations of Alphonsus, a suspected volcano.

The final chapter begins with some "crystal ball gazing" about possible commercial and scientific advantages of being on the moon. The author then asks if these possibilities are worth the large amount of money and effort which will be needed to get to the moon. He does not explicitly answer this question. This is probably because the answer is "No;" the immediate commercial and scientific gains which will result from landing men on the moon do not, *by themselves*, warrant the money and effort. However, it cannot be emphasized too strongly that the real value of going to the moon is in the act of getting there. The technology which must be developed for this will certainly have a large and beneficial effect in many areas of research not directly connected with space research. Moreover, the influence that this program will have on the attitudes of people is indicated by the enormous interest in the manned orbital flights. Indeed, it is likely that the beneficial influence on children in having astronauts as television heroes rather than cowboys will, by itself, justify the cost. Also, as the author points out, for a wealthy nation like the United States, which is not working near its capacity, nothing suffers because of such a program.

In this reviewer's opinion, this is a delightful little book which can be read with profit by readers having a fairly wide range of backgrounds. Some readers may be annoyed by a few rather flowery paragraphs here and there, but these are a very minor part of an informative and thought-provoking book on a subject which will, more and more, become a part of our daily lives.—*Robert E. Danielson*

The Biochemistry of Intracellular Parasitism By JAMES W. MOULDER; 172 pages; $6.00; University of Chicago Press, 1962.

The stated purpose of "The Scientist's Library: Biology and Medicine" is, "... to provide authoritative information about the growth and status of various subjects in such a fashion that the ... books may be read with profit not only by the specialist but also by those whose interests lie in other fields." To one whose interests all lie in other fields, it seems that this particular volume admirably fulfills these purposes. There is a great deal of information lucidly and pleasantly presented and the references are extensive and, mostly, pertinent to the main arguments. One obtains a rather clear picture of a somewhat disorganized field of research. The book would seem to be at once an excellent introduction to the biochemistry of intracellular parasitism and a useful review and compendium of present knowledge.

The book concerns itself, after an initial discussion of the notion of parasitism and of the different classes of parasitism, entirely with the restricted group of "obligate" intracellular parasites. These are organisms which have not yet been successfully cultivated (as multiplying populations) outside living host cells and, consequently, the investigation of their biochemistry presents formidable difficulties. The author further restricts himself to a discussion of parasites with "organismal" as opposed to "viral" life cycles, with the exception of a chapter on the pox viruses (on the slender grounds that these undoubted viruses are yet somewhat "intermediate" between organismal parasites and the other viruses).

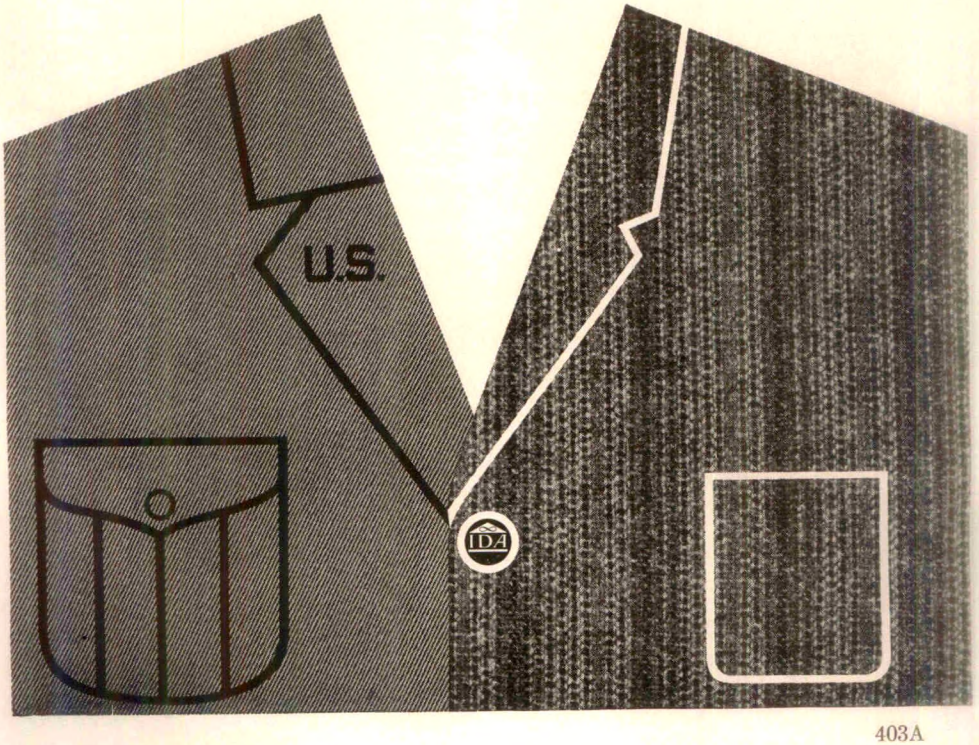
Chapters are devoted to the Malarial parasites, to the Rickettsiae and to the Psittacosis - Lymphogranuloma venereum group. Each chapter describes what is known of the life cycle of the particular group, followed by a presentation of what is known of their biochemistry. This usually consists of a description of gross composition, of carbohydrate metabolism (if any), of nucleic acid and protein metabolism,

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of electron transport and oxidative phosphorylation. The picture obtained is one of increasing dependence of the parasites (from Sporozoa to Pox viruses) on host cell metabolism. However, the author refrains from drawing very many conclusions as to the origins of intracellular parasitism and especially as to the origin of the viral mode of reproduction.

This book then is a clearly written presentation of what is known about the biochemistry of a small group of extremely important organisms (including the causative agents of malaria, typhus, psittacosis, trachoma, smallpox, and various diseases of domestic animals) which are shown to be elegantly adapted to a very interesting biological niche. It will be of general interest to biologists and biochemists as an informative review of the field and of particular importance in pointing out the enormous gaps in our knowledge of the nature of intracellular parasitism.—*W. D. Donachie*

A Biology of Crustacea by J. GREEN; 180 pages; $5.75; Witherby Ltd., 1961 ($5.75; Quadrangle Books, Inc., Chicago).

This book will be welcomed by persons wanting a general introduction to crustacean natural history. It is the first such book since Calman's "The Life of Crustacea," published in 1911 and now virtually unobtainable. The two books are alike in some chapters, but Calman dealt mainly with the Crustacea of different habitats whereas Green follows a more physiological arrangement.

In such a small book much had to be omitted. Green has selected especially certain aspects of biology that can be observed in intact animals. Information is presented on external anatomy, fossil forms, larvae, feeding mechanisms, blood, circulation, pigments, molting, associations with other animals, parasitism, distribution, and relationships with man. Other topics receive less attention, but the great variety among crustaceans is clearly brought out, aided by effective line drawings.

Many will bemoan the omission of favorite items. I personally felt the lack of emphasis on the role of the exoskeleton in determining much of the physiological ecology of crustaceans, and the absence of a discussion of crustacean relationships to annelids and to other arthropods. However, the chief weaknesses of the book all relate to the fact that too much has been attempted in too small a space. No topic is dealt with in enough detail to become really interesting, and much of the book tends to read like a catalog of facts. Positive statements are made even on the most controversial items, with no attempt to discuss contrary evidence. There is little of the intellectual excitement that might come from formulating problems faced by the animals and then discussing evidence as to how crustaceans have met these problems. Unfortunately, such an approach would have required a longer book or the omission of more topics.

There are very few misprints, but a much magnified figure of *Daphnia* is labelled "Actual length of the body," and in an appendix the Cephalocarida are unfortunately included in the Branchiopoda.—*John H. Lochhead*

Introduction to Theoretical Physical Chemistry by SIDNEY GOLDEN; 307 pages; $9.75; Addison-Wesley Publishing Co., 1961.

This book, of 307 pages, covers thermodynamics, statistical mechanics and quantum mechanics, in that order. It has been written for students of physical chemistry primarily, but it should have considerable interest for physicists as well.

The author is an expert in this field. He has given a condensed, unified, authoritative, original and fairly sophisticated treatment of the subject. It is surprising how much has been packed into a book which is really not long enough to cover any one of the three subjects in detail, let alone all three. The level seems appropriate for physical chemistry graduate students with facility in theoretical work.

It is difficult to see how this book can be used as a text in standard graduate

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courses in physical chemistry. Such courses usually include experimental physical chemists, organic chemists, etc. The texts in thermodynamics, statistical mechanics and quantum mechanics would ordinarily have to be much more detailed and elementary, at least at the beginning. The final level might be comparable to this but much more help than the present book will provide will usually be needed in attaining this level.

This text should prove extremely helpful and stimulating for reference purposes, or as the basis of a supplementary, self-study project for graduate students and others who have shown aptitude in theoretical work during their regular graduate courses.—*Terrell L. Hill*

Concepts of Mass in Classical and Modern Physics by MAX JAMMER; 230 pages; $6.00; Harvard University Press, 1961.

The author of the two monographs "Concepts of Space" and "Concepts of Force" has prepared a third, "Concepts of Mass," which received the monograph prize of the American Academy of Arts and Sciences for the year 1960 in the field of Physical and Biological Sciences. This is perhaps the first thorough and critical analytic treatment of this subject.

Professor Jammer follows an historical approach beginning with the etymology of "mass" showing that it originated either from the Greek "maza" (barley-cake or ordinary bread) or possibly from the Hebrew "mazza" (unleavened bread). He traces the rise of the concept "quantitas materiae" in medieval thought and concludes that antiquity did not formulate a concept of mass. The conceptulization of mass is ascribed to the work of Johannes Kepler, Isaac Newton is regarded as having systematized the concept of mass, and Ernst Mach is considered to have given the first acceptable operational definition of mass as a theoretical construct. The remaining portions of the book deal with the electromagnetic theory of mass and its impact on modern field theories and mass and energy in the theory of relativity. The author concludes that—"the notion of mass seems to elude all attempts at a fully comprehensive elucidation and a logically as well as scientifically unobjectionable definition."

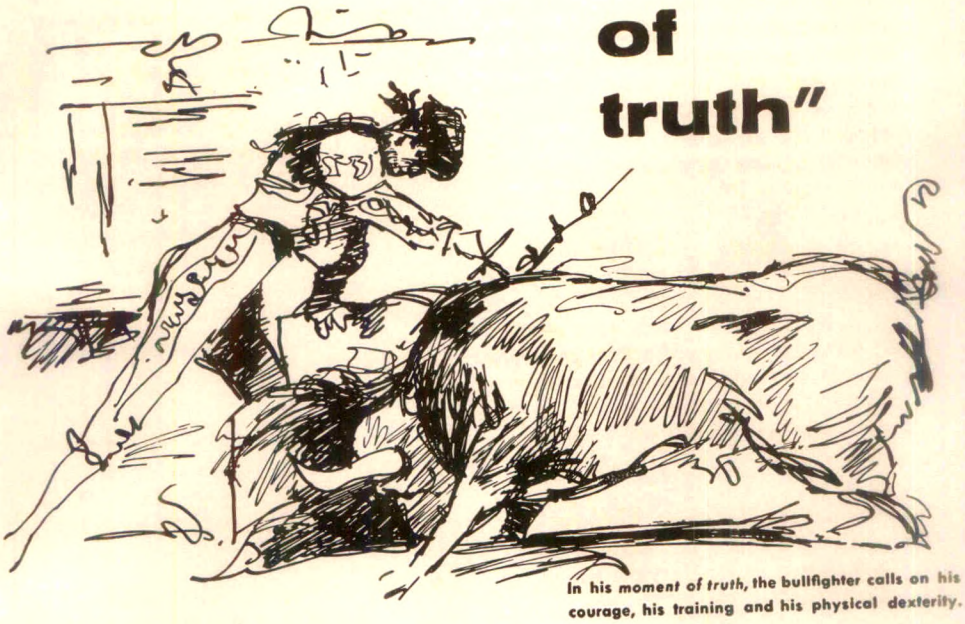
The book is written in a rigorous and concise fashion. It can be recommended to all readers interested in the foundations of physical science. Graduate students in physics should find this book a unique introduction to a very vexing problem in their chosen field, even though it is one of the most highly developed scientific disciplines.—*Floyd Dunn*

The Physical Anthropology of Southern Nigeria by P. A. TALBOT and H. MULHALL; 127 pages; $6.50; New York: Cambridge University Press, 1962.

This book represents a statistical analysis by Mulhall of data gathered by the late P. A. Talbot during the 1920's and 1930's in Southern Nigeria. The data consisted of somatological measurements on "nearly seven thousand persons, representing almost a hundred 'tribes' and lesser ethnic units." The individual measurements were lost, as a consequence of the second world war, and the book contains analyses of only the means and standard deviations which had been assembled by Talbot. Unfortunately, the subjects were not drawn with a view toward representative or systematic sampling of particular populations; instead they represent a series of prisoners, policemen, and soldiers. Classification of the samples is primarily by linguistic groupings, but no attention has been paid to the most recent and widely accepted revision of African linguistic classifications (Greenberg 1951, *et seq.*).

The book is divided into two parts—one a general introduction to statistical methods appropriate for the analysis of a series of somatometric observations, and the other an application of these methods to Talbot's data. Conclusions are reached with respect to the degrees of similarity or difference in a series of measurements of presumably homogeneous groups. Unfortunately there are

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no supporting data with regard to degrees of interbreeding of the supposedly distinct populations, or of the details of the environment within which these populations were found. In view of these limitations there is no way of relating the measurements to anything but the measurements themselves.

References: GREENBERG, J. H. "Studies in African Linguistic Classification," *Southwestern Journal of Anthropology, 5,* 79–100.—*Peter Kunstadter*

On the External Characters of Minerals by A. G. WERNER, translated by ALBERT V. CAROZZI; 118 pages; $4.50; University of Illinois Press, 1962.

Late in the eighteenth century, geology was slowly emerging as a science through the work of a few brilliant men. Among these, Abraham Gottlob Werner was influential mainly through his teaching. He is generally known as the founder of the Neptunist school, soon discredited, although it strongly influenced geologic thought, especially in America. Nevertheless, his basic contributions to mineralogy are sufficient in themselves to bring him lasting honor.

His *Von den äusserlichen Kennzeichen der Fossilien* (1774) was a completely new classification of minerals which initiated modern mineralogy. It was translated into French (1790, 1795) and English (1805). The three translations incorporated material from Werner's lectures that modified and expanded the original work. Werner himself, in spite of printed statements to the contrary, intended to publish a second edition but never did so.

By a magnificent stroke of luck, the University of Illinois Library acquired Werner's copy of the original work, annotated in his own hand, obviously intended to serve as the basis for a second edition. Dr. Carozzi has prepared his translation from this copy, carefully indicating the manuscript changes by Werner and the original pages on which they appear. This second edition, published nearly two hundred years after the first, is a precious document for the history of geology and especially the development of Werner's thoughts on the subject of mineral classification. We need no longer speculate on the authorship of the new material in the translations of the first edition for we now have, in the author's own words, his final thoughts on the subject.

Dr. Carozzi is to be congratulated for his painstaking but readable translation of this invaluable document and the University of Illinois Press for making it available in a beautifully printed and illustrated edition.—*Aurèle La Rocque*

Christianity and Science (Vol. 14 of the 20th Century Encyclopedia of Catholicism) by JEAN ABELÉ, S. J., translated from the French by R. F. TREVETT; 144 pages; $3.50; Hawthorn Books, 1961.

In its original French version the title of this book was: Is Christianity Uninterested in Science? In this edition, however, Fr. Abelé demonstrates the affirmative of this question in a fine introduction, and, leaving to another volume in this series an examination of the classic objections raised against the Christian religion on scientific grounds, proceeds to the more subtle problem of defining the relationships between Christianity as a religion, science as a method of knowing, and both as forms of a way of life.

Part one develops some aspects of Christianity relevant to the discussion of its relationships with science.

Part two is a sketchy historical account of the encounter between Christianity and science. Focus is placed on the (mis)use of the newly developing science for apologetic purposes, and on the ideological entanglements that ensued. Part three discusses modern problems.

From a prior age in which wisdom was one and all studies were interdependent we have progressed to an autonomy of methodologies and a correlative independence of the knowledges attained through them. We seem to have a better understanding today of the reciprocal autonomy of science and the Christian religion, and of the delicate

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care that must be used in relating the conclusions of each to the other. A later chapter is devoted to some of the practical problems that confront the Christian researcher, with the lines of possible solutions.

The book on the whole is well done, though this reviewer would like to have seen its outline appear more explicitly throughout the volume. It is written for the Christian in science, but should be of use to all men of science with either a personal or professional interest in this absorbing question.—*Andrew J. Dufner*

Plant Taxonomy, Methods and Principles by LYMAN BENSON; 494 pages; $11.50; The Ronald Press Co., 1962.

The oldest plant science, taxonomy, has been the subject of many textbooks in the last decade, few of which are distinguished or even different from their predecessors. Benson's book, by contrast, is refreshingly different and a truly valuable contribution for all who wish to understand the "how" and "why" of modern plant taxonomy. The author might have reversed the title thereby emphasizing methods and principles as he does in the text. This book is not a discussion of systems of classification or plant families. The reader will not identify plants with it nor learn the parts of a flower. He will learn the procedures for taxonomic study, the techniques for gathering information, the bases for drawing conclusions, and the forms in which taxonomic research can be presented.

The five parts of Benson's book are logical paths of development: exploration for data, classification, choice of names, description and documentation, and treatises and monographs. Each chapter has a list of suggested readings based on a more comprehensive bibliography at the end of the book. While the discussion of the principles and practices in the text may be succinct, the bibliography offers examples of more comprehensive use. The rules of the International Code of Botanical Nomenclature are discussed with problems as examples and with answers given. The recommendations of methods of presenting data are used superbly in the book itself. The suggestions for securing and caring for loans will warm the heart of every curator and an editor's life will be a happier one if the instructions for preparing manuscript and handling proofs are followed.

While the reader may tire of examples drawn from the genera *Quercus* and *Ranunculus* and the family Cactaceae, or illustrations of the western U.S., these are the plants and places the author knows best. Each is used appropriately in the text. Dr. Benson's exposition of the techniques of taxonomy deserves a warm welcome. His book has met a need.—*Richard A. Howard*

Culture and Behavior (The Collected Essays of Clyde Kluckhohn), edited by RICHARD KLUCKHOHN; 402 pages; $6.75; The Free Press, 1962.

With the death of Clyde Kluckhohn came the close of the career of one of America's best known anthropologists, a career which was marked by research in a wide range of subjects, many of which are included in this book. The book contains an introduction by Clyde Kluckhohn's son, Richard, and twenty-one essays, all of which had been previously published, but which had been modified for this book before Kluckhohn's death. They range in content from an extended discussion of the concept of culture (written with William H. Kelly), to a report of archaeological remains from a small New Mexico pueblo; from some notes on an obscure Navaho ceremony, and case studies of Navaho personality development, to consideration of the national character of the Great Russians, and the value system of modern Americans. Also included is a complete bibliography of Kluckhohn, compiled by Lucy Wales.

Kluckhohn's range is reflected in this book, but not his depth—the book does not do justice to the man. In part this is because he never wrote any detailed technical summary of his theoretical position, and in part it is because much of his effectiveness was as a teacher and an academic administrator.

The book is a curious mixture.

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Perhaps if he had lived to complete the manuscript there would be more continuity between the various essays, and less repetition in some of the sections. As is, it will be of most use to the student of anthropology who is interested in Kluckhohn's miscellaneous works. The general reader will not be interested in an outdated technical discussion of problems of the archaeological classification of an unimportant New Mexico pueblo. This might have been the opportunity for an extended discussion of general and important problems of classification and taxonomy of cultures—but it was not so utilized.

Kluckhohn was perhaps at his best when outlining theoretical problems and summarizing the positions other authors have taken in regard to these problems. But again, the general reader or the anthropological specialist is liable to be frustrated. For example, in an essay on "Conceptions of Death among the Southwestern Indians" we find a brilliant and concise summary of theories purporting to explain customs which surround death and burial. Kluckhohn implies that he will develop a theory to explain the social functions of these practices, but nowhere in his discussion of the customs of Southwestern Indians do we find a concise statement of his own theoretical position, or any indication of how his hypothesis might be tested. Likewise, we would search in vain for Kluckhohn's definition of the concept of *function*, a concept to which he returns repeatedly.

In summary, this book is an interesting sampling of the work of an important scientist, but it does not stand up as an independent work.—*Peter Kunstadter*

Proceedings of the International Conference on Semiconductor Physics, Prague, 1960; 1133 pages; $35.00; Academic Press, 1961.

The remarkable recent advances in our knowledge and understanding of the semiconducting state are epitomized in this volume. The volume sparkles with brilliant contributions, and is enlivened by a few catcalls, especially by Russian scientists, at the "germanian physicists," who persist in ignoring the complex problems of the liquid and incompletely crystalline semiconductors.

Considerable advance is described in the determination of band structures of single crystals. It has been demonstrated that detailed analysis of all many particle effects fully justifies the conventional independent particle method of semiconductor bands, and that in polaron motion one must distinguish a high and low temperature range. In the low temperature range, conventional band theory can be applied, in the high temperature range, the mobility process is characterized by an activation energy and is qualitatively similar to diffusion of classical particles through a crystal (Kohn, Section B-1).

In discussing transport processes, Herring (Section C-1) considered the present state of transport theory, especially on multiphonon scattering, ionized impurity scattering, and the application of quantum transport theory to strong scattering situations and the role of lattice deformations on hopping processes characteristic of low mobility materials. An important question is that of the Hall effect associated with hopping. Herring notes that this can only occur if the probabilities of different jumps are affected by some memory of the preceding jump, and that the Hall mobility can be much larger than the drift mobility. The nature of scattering processes is in general not well understood, especially in cases where mobility is low. Joffe (Section 0-1) reviewed a number of incompletely explained questions on less simple materials, especially as to the temperature dependence of scattering cross sections.

Considerable progress is described in the new field of Esaki diodes. An interesting model for the excess current in tunnel diodes is based upon considerations of the field ionization of impurity levels present (Chynoweth and Logan, Section F-1–4). The study of tunnelling has become an important tool for examination of semiconductors.

Thermal conductivity received attention, although its temperature dependence still apparently remains in-

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completely explained. Those looking for possible means of reducing thermal conductivity will wish to examine the new observations of Slack (Section 0–3) on the influence of phonon-spin interactions on thermal conductivity in some antiferromagnetic materials.

A new technique for gaining information on scattering processes, ultrasonic absorption, is described (Hogarth, Section 0–6).

Aigrain (Section F-9) suggested in a brief theoretical discussion that a new kind of elementary electromagnetic excitation be considered in semiconductors. He coined the name "helicon" for them. These excitations arise as very low frequency plasma oscillations propagated especially among the locally non-neutral regions in the semiconductor. Alfven waves in neutral plasma are regarded as a special case of helicons.

The extensive development of the magneto-optical methods as ably reviewed by Lax (Section J-1) is impressive, as are the uses of direct optical and photoelectric effects.

Considerable attention was given the excitons role in semiconductors. A theory by Haug and Schoenoffer (Section K-5) appears to present a unifying approach to earlier theoretical studies by Pekar, Frenkel, Wannier, and by Heller and Marcus. Experimental evidence is presented by several workers as to the modes and means of migration of excitons, especially from the observations of optical and magneto-optical effects, on this basic property of semiconductors.

A series of papers on surface properties is introduced by an excellent review by Lax (Section M-1). New surface cleaning and ultra high vacuum techniques have permitted important advances, and attempts to measure surface properties at other than room temperature signify advance.

In addition to the papers above which were grouped by physical measurement techniques, a large number of papers are presented grouped by materials. Included are some organic molecular complexes, anthracene, metal oxides, sulfides, diamond, many III-V compounds, Te, and Se. Glassy state semiconductors were reported on by Kolomiec (Section R-10).

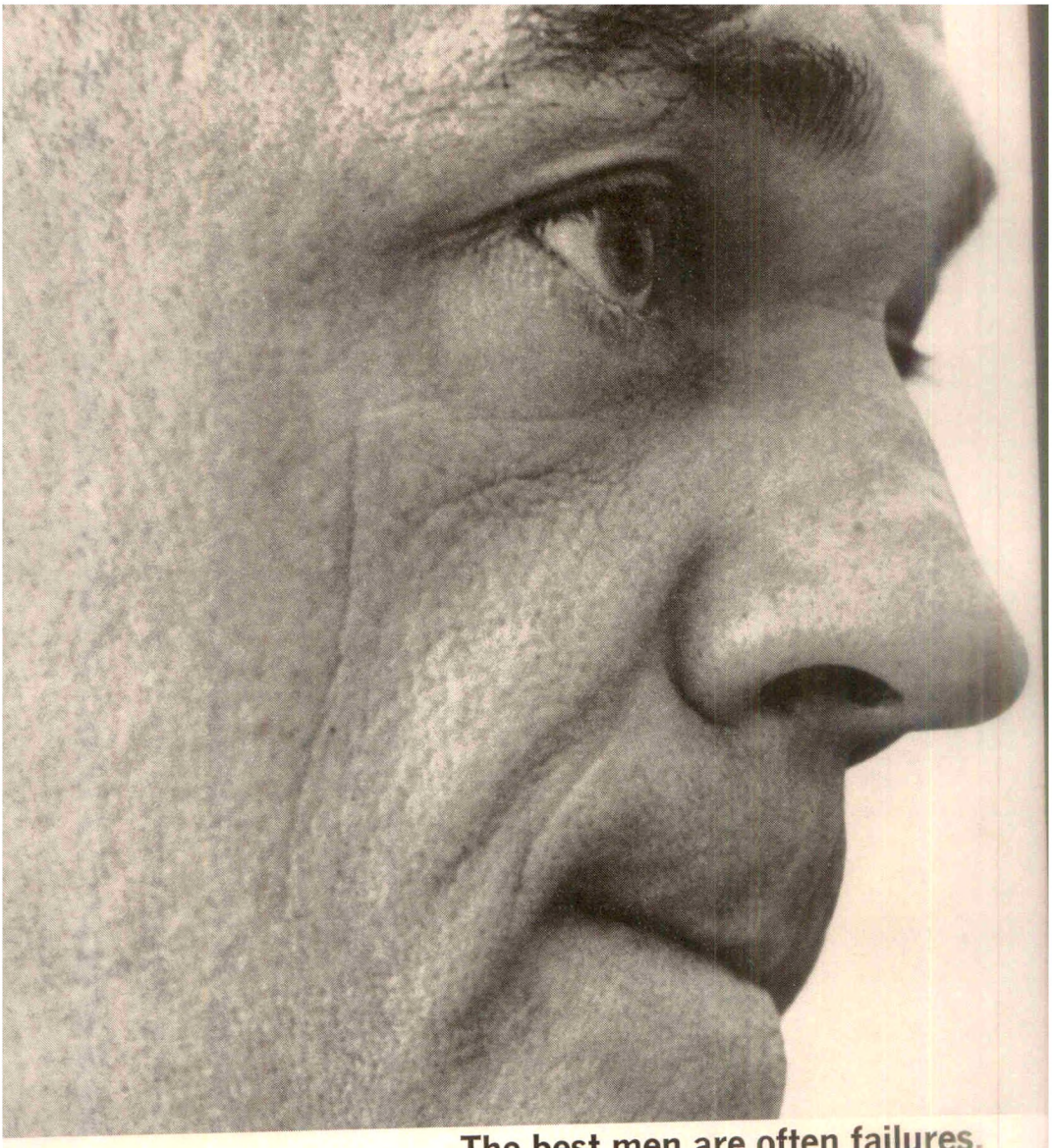
In this reviewer's eyes the present compendium of papers only emphasizes the unfortunate lack of progress in dealing with the semiconducting disordered state. There has been perhaps too little progress in this area since the suggestive studies of Eisenschitz and Dean on band perturbance by order-disorder transitions as calculated for potassium and argon. The fact that the set of all known semiconductors conducts better in the liquid state than in the solid state (compared at temperatures just above and below the melting point) points up the inadequacy of an over-simple band theory approach.

Some American readers will find the use of English, German, French, and Russian throughout a disadvantage, although most articles are in English. The editors are to be commended for succeeding in the difficult job of bringing these many articles of current interest to publication in lucid form in a relatively short period.—*Herbert A. Pohl*

Molecular Biophysics, by R. B. SETLOW and E. C. POLLARD; 545 pages; $11.75; Addison-Wesley Publishing Company, 1962.

Physical Chemistry of Macromolecules, by CHARLES TANFORD; 710 pages; $18.00; John Wiley and Sons, 1961.

In spite of their different titles, these books deal with much the same thing. Whether called molecular biophysics, molecular biology, or physical biochemistry, the field is in fact largely macromolecular chemistry. On the other hand, Tanford's book is, as one might expect from the author's background, heavily oriented toward biological macromolecules. It does pay some attention to synthetic high polymers, but not with the loving attention that one finds in other books by authors more heavily imbued with the synthetic polymer tradition. Setlow and Pollard do differ from Tanford in putting more emphasis on biology and physics and less on chemistry. For example, one



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finds in the index of Setlow and Pollards' book eleven entries under "Viruses" while Tanford has none but does have fourteen under "Virial coefficients," e.g., "for flexible polymer," "for spheres and rods," "of proteins," and so on. Similarly, the former lists "*Drosophila*" and "*E. coli*," the latter "Donnan effect" and "Egg albumin"; on the physical side, the former lists "Carbon 14" and "Ionizing radiation," the latter, "Carboxymethylcellulose" and "Ionic polymerization." But these are selected examples. Both books list "Collagen, X-ray diffraction of," "Deoxyribonucleic acid" (about twenty entries in each case), "Flow birefringence," "Isoelectric point," "Sedimentation constant," and "Zeta potential." Both books are competently written and full of the facts and theories that a graduate student needs to know if he is to do research either in physical biology or in the field of synthetic high polymers.—*B. H. Zimm*

Volcanoes in History, in Theory, in Eruption by FRED M. BULLARD; 441 pages; $7.50; The University of Texas Press; Austin, 1962.

Fred Bullard describes in dramatic and yet scientifically correct fashion the behaviour of the main types of volcanic eruptions. The book is written in a style understandable to nontechnical readers. At the same time, the book contains a great deal of information for the student of geology and petrology.

The book is divided into three parts. In the first part is a brief summary of the history of the science of volcanology, a classification of volcanoes, and a discussion of the kinds of material given off by volcanoes.

In Part 2, Dr. Bullard discusses his five main types of volcanic eruptions—the Peléan, Vulcanian, Strombolian, Hawaiian, and Icelandic—by considering a specific volcano in considerable detail as typical of each main type. This section is well illustrated with photographs, diagrams, and maps of the volcanoes mentioned. The 1902 eruption of Pelée is described from eye-witness accounts and the various phases of this type of eruption are carefully explained. The history of Vesuvius since 79 A.D. is outlined as an example of Vulcanian type eruption. There is also a historical account of the volcano "Vulcano." Stromboli is used as an example of Strombolian eruption.

The recent activity of both Mauna Loa and Kilauea are summarized in the discussion of Hawaiian type eruptions. Fissure eruptions which have produced the great lava plateaus of the world are described as the Icelandic type of eruption. There is a brief summary of some of the volcanoes in Iceland.

Part 3 of the book considers volcanic cycles, the birth of new volcanoes, man's use of geothermal energy, and a discussion of the origin and relation of volcanoes to the major crustal features of the earth. In the discussion on volcanic cycles, Kilauea, Etna, and Vesuvius are used as examples. The chapter on the birth of new volcanoes in historic time includes an extended account of Paricutin Volcano in Mexico.

The utilization of geothermal steam in Italy, New Zealand, Iceland, and in Sonoma, California is summarized.

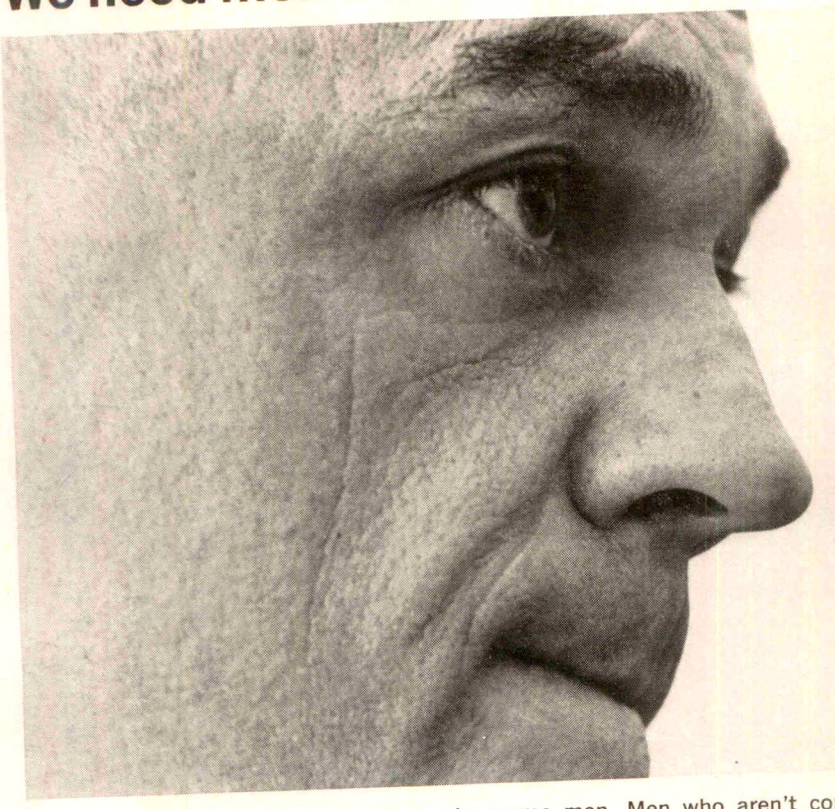
The book closes with a short glossary of terms and a bibliography.—*Willard H. Parsons*

The Ethnobotany of Pre-Columbian Peru by MARGARET A. TOWLE; 180 pages; $6.00; Viking Fund Publications in Anthropology, No. 30, Aldine Publishing Co., 1961.

Ancient Peru was both a center of civilization and one of the hearth's of New World agriculture. It is now possible to trace the beginnings of plant cultivation on the Peruvian coast back to about 2500 B.C. Owing to the extremely dry coastal climate, the yield of perishable plant remains from graves and midden deposits is remarkably rich. For these reasons the study of pre-Hispanic Peruvian plants is especially important for the culture historian and for the botanist concerned with the development of cultivated plants in America.

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published in 1934–35. Since that time new cultural phases have been discovered in Peru, a longer and firmer relative chronology has been established, absolute dates by the radiocarbon method have been obtained, and much new datable plant material has been recovered. Dr. Towle's book is therefore timely and very useful.

This study is based on an analysis of 2200 wild and cultivated plant specimens with clearly defined archaeological contexts. These were excavated from sites in various parts of the Peruvian coast during the past twenty years. Part I is a systematic ethnobotany with pertinent citations of the botanical and archaeological literature, and includes a list of plants according to their uses. Part II is a chronological and regional treatment of plants integrated with useful summaries of the archaeological contexts from which the plants came. This section enables the reader to view the course of plant domestication from the stage of premaize, incipient agriculture to the stage of intensive agriculture when some 50 cultivated plants were utilized. The story of this development is still far from complete, and many problems of origin, including the origin of Peruvian types of maize, remain to be solved. But this study is an important contribution to that story and will be extremely useful to archaeologists and ethnobotanists.—*Donald Collier*

Biological Effects of Freezing and Supercooling by AUDREY U. SMITH; 462 pages; $11.00 (Monograph No. 9 of the Physiological Society); The Williams and Wilkins Co., 1961.

Dr. Smith's monograph is a valuable analysis of the cryobiological literature appearing since Luyet's review of 1940. It gains by having but a single author who writes with admirable clarity and who has covered the zoological and microbiological material thoroughly.

The emphasis of the book is on the preservation of viability, and it is organized according to the degree of cellular complexity. Thus, it begins with the freezing of spermatozoa and red blood cells, moves on to microorganisms, and then dwells extensively on the preservation of multicellular systems, with separate chapters on tissues in culture, ovarian and other mammalian tissues, mammalian organs, poikilothermic animals, and mammals. Because of its approach, the book will be especially useful to those wishing to preserve the aforementioned biological materials by freezing; and it covers in detail the cooling rates, temperatures, suspending media, and warming rates that yield optimal survivals. To those who are not concerned with preservation, or whose interests in cryobiology cut across taxonomic lines, its usefulness is reduced by a rather inadequate index. There are, for example, no general index headings or cross indices for such items as "storage," "storage temperatures," "protective agents or additives," "rate of cooling," "rate of warming or thawing," "polyvinylpyrrolidone," "permeability," or "dehydration." The book will not be informative to those searching for information on freezing in higher plants, for Dr. Smith makes only one reference to the vast literature in this field, and that to answer a criticism. Either the word "Biological" in the title should have been qualified, or, as a minimum alternative, reference should have been made to a few of the pertinent reviews and papers on freezing injury in plants.

The author ascribes most freezing injury to high concentrations of electrolytes, and emphasizes glycerol as the important additive in preventing this injury. Her arguments are so forceful that many readers may consider them unassailable. However, a number of cryobiologists argue that many findings are incompatible with Dr. Smith's interpretations, and that other data are compatible not only with her interpretation but with alternative theories as well. Unfortunately, documentation of this point would require considerably more space than that allotted in these columns.—*Peter Mazur*

Precocious Sexual Development (A Clinical Study of 100 Children) by E. THAMDRUP; translated from the Danish by A. LA COUR; 237 pages; $7.50; Charles C Thomas, 1961.

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The text, which is an English translation of the Danish original, is the first study in which a clinical investigation of all children with premature development of the sex characters has been attempted for an entire population (Denmark). The cases have been classified into six major types; true precocious puberty, adrenogenital syndrome, hormone-producing gonadal tumors, premature pubarch, premature thelarch, and premature sexual development due to exogenous hormone action.

It is only in the first type, true precocious puberty, that reproduction is biologically possible at an early age. While the largest number of cases fall into this category there is no known biological cause in most instances. However, about 20 per cent of the patients with true precocious puberty had brain lesions which usually impinged upon the hypothalamus in some way.

All children in which premature sexual development was found were examined at short intervals so that rates of development could be followed. Measurements were made of height, bone age and body proportions. Hormonal analyses were done for pituitary gonadotrophins, neutral 17-ketosteroids, reducing corticoids, estrogens and androgens. The sella turcica was examined by X ray. Thus it was possible to compare all aspects of development in these children to the normal population figures. At the same time a study was made of the psychological problems which arose due to the advanced sexual development.

This study of 100 cases is most instructive in helping one understand these developmental anomalies which though rather rare can be quite alarming both to the children and parents involved. The incidence in Denmark was approximately 4 girls and 1 boy per year in a population of 4½ million. The total number of cases reported in the literature (1000) suggests a ratio of 3 girls to 1 boy. Each case history here represents a biological experiment on the part of nature whose signs and symptoms must be unraveled if we are to gain a better knowledge of the endocrine glands and especially their role in the development of puberty.—*Robert D. Lisk*

A Textbook of Comparative Endocrinology by AUBREY GORBMAN and HOWARD A. BERN; 468 pages; $12.50; John Wiley & Sons, 1962.

Finally, a textbook of endocrinology has been written which places the rat and man in proper perspective as only two of many species which comprise the vertebrate phylum.

Throughout the text the developmental aspects of the endocrine system as well as the functional specializations of the various endocrine glands are emphasized for all vertebrate classes. This approach provides one with a lucid picture of the evolutionary development of the endocrine system in the vertebrates. In the concluding chapters of the book the integrative mechanisms between the endocrine and nervous systems are stressed. Finally, to help achieve an over-all view of hormonal mechanisms, the hormone systems found in the invertebrates and in plants have been compared with the vertebrate system.

The importance of comparative studies in the field of endocrinology are strikingly illustrated in the chapter on the thyroid. The typical rat endocrinologist is apt to think of the thyroid as the regulator of metabolism. Yet this function for the thyroid has never been unquestionably proven for a cold-blooded vertebrate. However, the thyroid is necessary for schooling behavior in fishes and metamorphosis in amphibia.

The material is well presented throughout, and gives a comprehensive coverage of non-mammalian endocrinology, at the same time integrating this material with the more classical work on the rat and man. If a weak point exists it is a lack of documentation. There are usually no more than a dozen references given at the conclusion of any chapter and these are primarily to review articles. This, the reviewer feels, weakens its function as a text, in that it does not allow the student to track



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down any particular facts which may catch his fancy.—*Robert D. Lisk*

Experimental Nuclear Chemistry by GREGORY R. CHOPPIN; 226 pages; $9.25 trade edition; Prentice-Hall, 1961.

With nuclear phenomena playing so prominent a role in current scientific activity, a book on experimental radiochemistry is a welcome addition to a chemist's library. The jacket of Professor Choppin's book states that the book "describes the basic theory and techniques involved in the use of radioisotopes in scientific research." The volume consists of twelve chapters which attempt to cover all aspects of radiochemistry, viz. nuclear phenomena, techniques for the measurement of radiation from radioactive materials, radiochemical laboratory practice, radioactive indicator applications, radiation chemistry, safety procedures, and licensing regulations. To achieve this end in a limited space, the author has introduced a series of forty experiments, preceded individually or in groups, by explanatory matter.

Fundamentally, "Experimental Nuclear Chemistry" is a laboratory manual either for a course in radiochemistry or as an adjunct to a course in physical or inorganic chemistry. A useful pedagogic feature is several sets of problems chosen to illustrate selected portions of the text. For this, unfortunately, the "basic theory" is not always adequate. "Experience has shown that it is helpful to have at hand throughout the course a text dealing with the basic aspects of nuclear sciences." This direct quote from the preface is indeed true. The basic material, presented mostly in the front half of the book, is incomplete, compressed, and seems rather an annex than an integral part of the book. For example, Statistics of Counting is discussed in four pages of which eight lines are devoted to the Chi-squared test with no explanation of terms, and about a page allotted to optimization of counting times, choice between counters, and tests for the reliability of a counting system.

The book also contains a number of errors, some typographic. The Curie is incorrectly defined, the curves in Figure III-3 are incorrectly labelled, and the expression for counter efficiency is reversed. Furthermore one finds scalar for scaler, Pu^{141}, Ni^{60} of mass 57.9536 and other irregularities which one hopes will be corrected in a subsequent printing.—*Charles Rosenblum*

New Perspectives in Physics (Where Does Physical Theory Stand Today?) by LOUIS DE BROGLIE, translated by A. J. POMERANS; 291 pages; $6.00; Basic Books, 1962.

This book may be read with profit by both scientists and historians of science, for it is written by a Nobel prize winner who has concerned himself not only with the substance of physics but with its historical development.

De Broglie's latest publication is not a technical treatise, but a compilation of many diverse papers and lectures written between 1950 and 1956. The book is organized into four sections. Part I contains six chapters primarily pertaining to the early historical and philosophical aspects of quantum theory, plus a highly readable paper on The Philosophical Meaning and Practical Consequences of Cybernetics.

In Part II the author examines what Einstein once referred to as one of the central questions of quantum physics. The question is "whether the current interpretation with its exclusive use of a purely statistical ψ wave is a *complete* description of reality, in which case indeterminism would be a physical fact and we could never hope to give accurate descriptions of atomic events within the framework of space and time, or whether,—the interpretation is incomplete and, like the statistical theories of classical physics, merely hides a completely determined and ascertainable reality behind variables which elude our experimental techniques." In what amounts to a fascinating intellectual autobiography, De Broglie traces the development of his early concepts on wave mechanics in 1923–1924 to his more recently developed conviction that at least a searching re-examination of this

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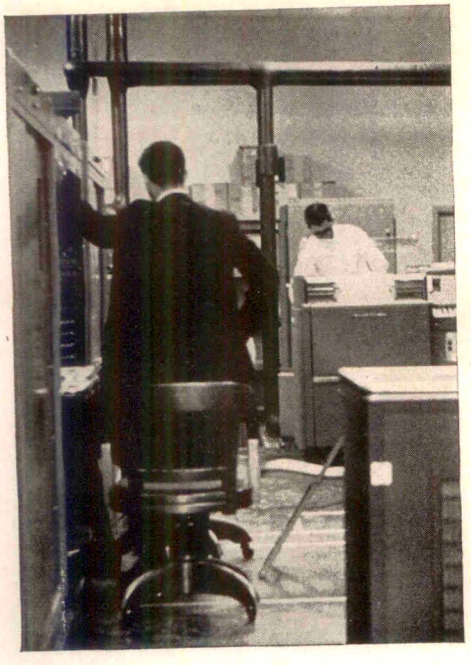
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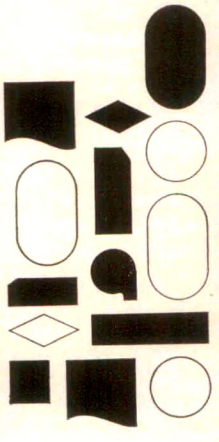
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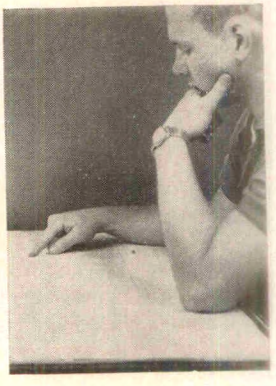
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question is justified. He is only faintly optimistic about supplanting the prevailing statistical theory, but he makes an eloquent appeal to scientists to attempt a "return to the clarity of spatiotemporal representations—if only to determine whether the orthodox view is in fact the only possible one." And he hints that the dilemma of the wave-particle dualism may be impeded not only by formidable technical difficulties but by the role of "fashion" in science.

Parts III and IV contain short chapters pertaining to education, the aims of scientific research, contemporary science, and brief essays relating to the history and philosophy of science. Included also are brief biographical sketches of Pascal, D'Alembert, and Pierre Duhem.

In general the book is a stimulating survey of some of the successes and enigmas of modern physics. The reader who enjoys the reflections of an elder scientist will find this book one of the best.—*Frederick A. White*

The Collected Papers of Enrico Fermi, Vol. I. Italy, 1921–1938, edited by E. AMALDI *et al.;* 1043 pages; $15.00; The University of Chicago Press in cooperation with the Accademia Nazionale dei Lincei of Rome, 1962.

As a most fitting memorial to the late Enrico Fermi, an editorial board composed of E. Amaldi, H. L. Anderson, E. Persico, F. Rasetti, C. S. Smith, and A. Wattenberg, with E. Segrè as chairman, has prepared and annotated selections from his publications in various journals of physics. This first volume shows that the "selection" is more inclusive than exclusive; only those papers which were essentially duplicates, or were excerpts from more detailed accounts have been omitted. The volume opens with an excellent biographical introduction by Segrè. Everyone who knew Fermi bowed before his virtuosity and brilliance in physics, and Segrè's reverence shows clearly in what he has written, together with a wealth of details which must have been gathered in long conversations with Laura Fermi and others who could supply them. At the end of the period covered in this volume, Fermi was in his 37th year, and was leaving Italy for the United States. During these years there had appeared the great contributions of the statistical atom model, the theory of β-ray spectra, the discovery of neutron-induced radioactivities for which he received the Nobel prize, and of thermal neutrons which were the key to the first sustained chain reaction.

In addition to the original papers on these subjects, the reader has available valuable reviews and summaries of other fields, written by Fermi with his translucent clarity. As each paper, or, in some cases, each small group of papers, is presented, an illuminating introductory comment appears, written by a distinguished physicist, sometimes the one who was Fermi's collaborator in the work in question. In addition to members of the editorial board, G. Polvani and B. Rossi have participated in these remarks.

There are several plates, showing, for instance, pages from an entrance examination on "sound" for the Scuola Normale Superiore at Pisa, written at the age of seventeen. Instead of the usual high school theme on pitch, frequency, and resonance, we find the boy beginning with the partial differential equation of a vibrating reed and its solution by Fourier's series. In others, there are reproductions from the celebrated notebooks. The collection is recommended for those desiring an authoritative record of Fermi's physics, which is essentially identical with the progress of the discipline in the second quarter of the twentieth century.—*Samuel K. Allison*

An Introduction to the Physics of Vibrations and Waves by N. FEATHER; 330 pages; $6.00; Aldine Publishing Co., 1962.

This book is intended as a sequel to Professor Feather's *An Introduction to the Physics of Mass, Length and Time* published in 1959. In that work the author provided the beginning undergraduate specialist in physics with a systematic account, without the use of the calculus, of the origins and development of the fundamental measurements

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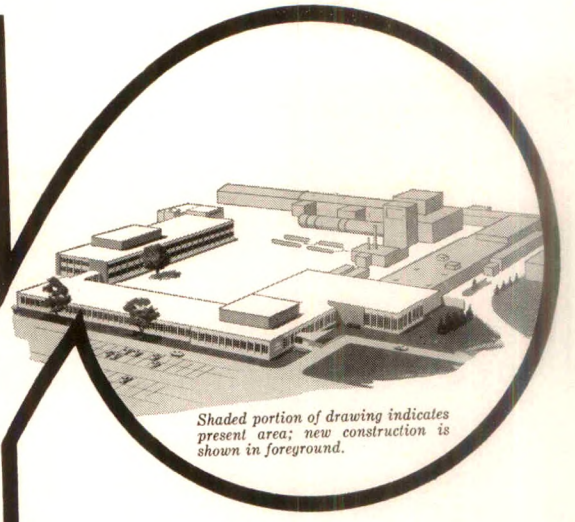
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in physics (mass, length, and time), and the mechanical properties of matter in bulk. Gravitation, elasticity, surface tension, and kinetic theory of gases were treated there as branches of macroscopic physics using the classical concept of particles.

In this work, on vibrations and waves, Professor Feather, now making use of the calculus in a work designed for second year physics students of the University of Edinburgh, elaborates and formulates the classical concept of waves, i.e., waves and the extent to which the characteristic features of wave motion can be elicited from the study of vibrations—the processes in which they originate, and by which they are sustained or ultimately dissipated.

The topics in this volume—stretched strings, coupled vibrations, elastic vibrations of rods and air columns, plane waves in an infinite medium, water waves, the wave-particle duality—are all treated systematically in relation to the whole, while "meeting history on the way." As in the earlier volume, so here, the author makes frequent reference to the historical origins of the concepts involved, but without neglecting recent advances. While classical electricity and magnetism receive some mention in the chapter on the wave-particle duality, the book is much more concerned with mechanical vibrations, elastic waves, sound, and light.

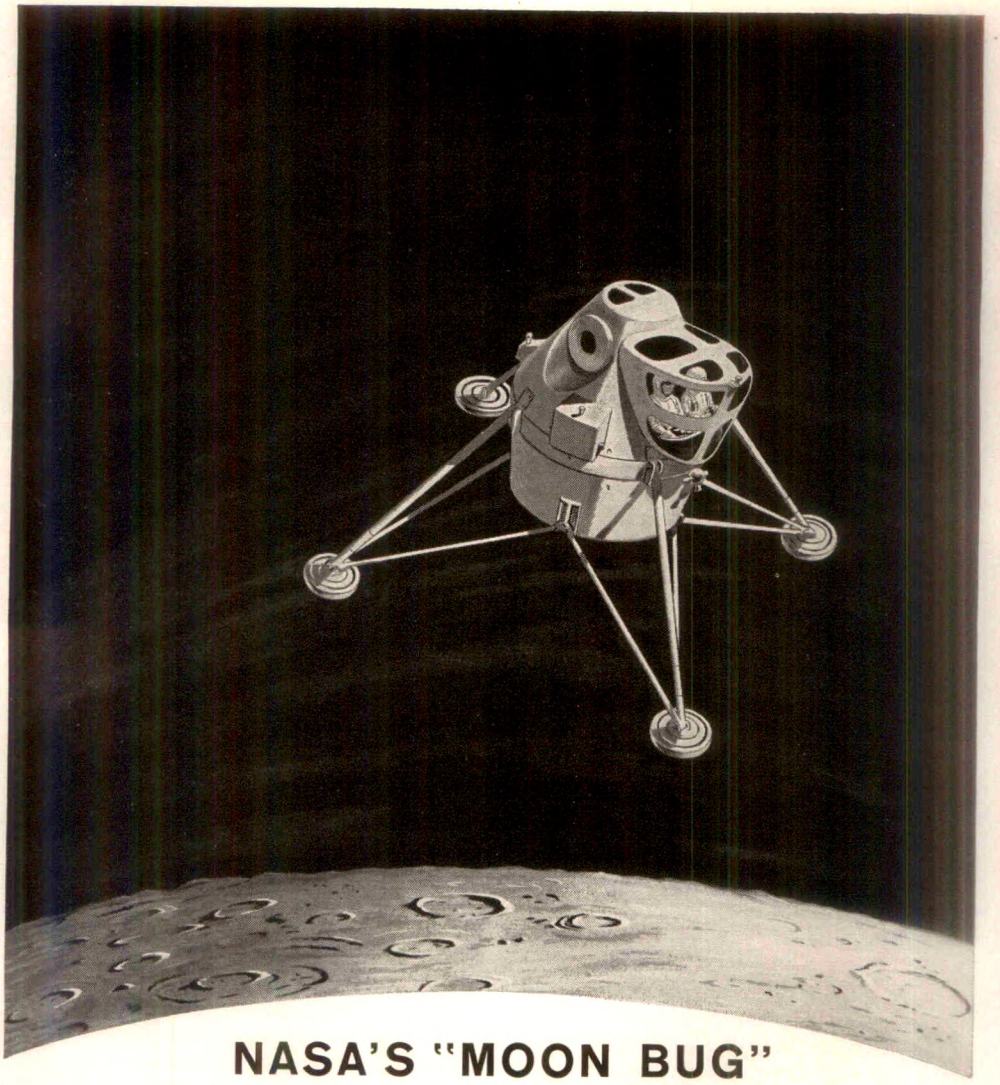
Except for the skimpy treatment of electricity and magnetism, the two volumes in this sequence might together conceivably serve, what its author obviously had in mind, as a general introduction to physics at the undergraduate level. Most physics teachers will probably conclude that in coverage and depth of treatment these works are unsuited for that purpose. Notwithstanding this limitation, here is an unorthodox approach to what physics is all about which will find appreciative readers among young physics students and many others. No substitute for a more rigorous textbook, these volumes are admirably suited to convey some feeling for the historical origins and the unity of the subject. They stress the understanding of basic principles in place of comprehensive coverage of the many separate branches of physics, or fluency in mathematical manipulation—both of which the author recognizes as being desirable in their own right.

The most serious criticism which this reviewer would raise in connection with Professor Feather's brave experiment in teaching physics with the help of this kind of textbook, is that scattered historical remarks throughout the work too conspicuously bear the earmarks of a narrative built up around what most logically might have happened in the history of physics, from the vantage point of the moderns looking back. Unfortunately, the records of what took place, and when and where, are by no means so neat, so unambiguously pinpointed, or so straightforward as this account purports to convey—even ignoring all but the most positive content of the history of the subject.—*Erwin N. Hiebert*

Temperature Measurement in Engineering, Vol. 2 by H. D. Baker *et al.*; 510 pages; $13.00; John Wiley & Sons, 1961.

This book is a fitting companion to Volume I of the same title published by these authors in 1953. In Volume I emphasis was placed upon thermocouple systems. The present volume places major emphasis upon resistance thermometry and radiation pyrometry. The chapters are arranged to cover first the fundamental description of the systems and then to discuss design considerations. It is unfortunate that some erroneous definitions and statements appear in the presentation of the radiation laws (section 4.3). While it is doubtful that these statements will cause more than a slight annoyance to an experienced person, they could be misleading to one of less experience. This appears to be an isolated lapse in an otherwise excellent presentation.

The greater part of the present volume is devoted to a discussion of various applications ranging from the measurement of temperatures near absolute zero to the almost incomprehensible temperatures of stellar regions. Included also are such important applications as surface



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temperatures and transient effects caused by moving sources or by changing boundary conditions. Interspersed within the several chapters are many fine discussions of the fundamental meaning of temperature. This reviewer would have preferred gathering these discussions into a common location. It was, however, very welcome to find this material included in a volume devoted primarily to the measurement of temperature which one might expect to concentrate on techniques rather than theory. The authors have achieved a nice balance in this regard.

Extensive derivations are not offered in the book; rather, more than 1500 references are made to original articles in the field. While the authors do not claim comprehensive coverage, certainly any worker in the field will find that this volume together with its previously published companion offers a very satisfactory introduction to temperature measurement techniques and analysis for laboratory or industrial applications.—*D. E. McFeron*

Hydrodynamic and Hydromagnetic Stability by S. CHANDRASEKHAR; 654 pages; $16.80; Oxford University Press, 1961.

Hydrodynamic and hydromagnetic stability are subjects of great importance in astrophysics. About 1950, the author (a theoretical astrophysicist) began to investigate some basic stability problems which are applicable to astrophysics. This book summarizes the results of nearly ten years of work by him and his collaborators. No astrophysical applications are contained in this book, however. It is written as a physics text in which the application of the stability calculations is limited to laboratory experiments, many of which were performed by colleagues of the author at the University of Chicago.

After an introductory chapter on basic concepts, the next four chapters are devoted to the stability of a plane-parallel layer of fluid in the presence of an adverse temperature gradient. This is followed by a discussion of thermal instability in fluid spheres and spherical shells. Next come three chapters on the stability of couette flows and related flows between coaxial cylinders. Further chapters are devoted to the Rayleigh-Taylor instability, the Kelvin-Helmholtz instability, and the stability of jets and cylinders. The book concludes with a chapter on gravitational stability and a chapter on a general variational principle.

An astrophysical application for the thermal instability in a layer is the convection occurring in the outer layers of the sun. In 1952, the author published a paper on the effect of a uniform magnetic field on the stability of a fluid layer and applied his results to sunspots to try to verify the hypothesis that a sunspot is dark because the sunspot magnetic field inhibits the transport of heat by convection. The result was inconclusive because over-stable oscillations (oscillations of increasing amplitude) were predicted in the sunspot umbra and thus the magnetic field did not fully inhibit convective motions. Although the matter is still not resolved, recent work suggests that the over-stable (standing wave) oscillations are replaced by hydromagnetic traveling waves when more realistic boundary conditions are used. Also, it now seems that convection can take place in the penumbra of sunspots in the form of convection rolls.

The preceding paragraph is an example of some of the progress in solar physics which has resulted from the author's foresight in attacking these linear stability problems. Much remains to be done, however, before we have satisfactory quantitative models of sunspots and granulation. Indeed, as far as this reviewer is aware, there has been no attempt to apply critically the calculations on a rotating convective layer to the sun. A hydromagnetic instability is probably involved in the origin of solar flares. An extension of the calculations of the stability of the pinch (contained in the chapter on jets and cylinders) to include the effects of finite electrical conductivity might yield important results in this area.

Few scientists write a book of this magnitude. It is even more impressive when one realizes that at least half of

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the book represents original research by the author and his collaborators. One cannot escape the conclusion that it is a first class piece of work by one of the most productive scientists of our time. Physicists will find it a most valuable book, both in the author's characteristically elegant mathematical treatment and in the summaries of the relevant laboratory experiments. Astrophysicists interested in cosmical electrodynamics will find this book the necessary theoretical foundation to pursue the subject.—*Robert E. Danielson*

Radio Surface Waves by H. M. BARLOW and J. BROWN; 200 pages; $6.75; Oxford University Press, 1962.

The authors of this volume have made many important contributions to the subject of electromagnetic surface waves in recent years. Therefore, it is quite fitting that they should co-author a monograph on surface waves. Essentially, it is a detailed exposition of their own views on this rather controversial subject. The electromagnetic waves discussed in the monograph are of a non-radiating type. Thus, the subject matter really deals with *evanescent* waves and it is just this evanescent character which is the basis of a working definition of a "surface wave," although, as the authors point out, not all evanescent fields are associated with surface waves. The authors admit that the definition is highly idealistic and realizable only with a smooth and plane interface between homogeneous media. They propose that waves on actual structures, such as when a wave propagates around a smooth cylindrical supporting surface, be regarded as a "perturbed surface wave."

The book discusses, in detail, the conditions for the support of surface waves on various planar and cylindrical structures. Many interesting physical descriptions are put forth such as the significance of a complex Brewster angle and its relation to surface waves. By first leaving aside the question of excitation, the early chapters use only quite elementary methods. Later in the book, the question of launching of surface waves is considered in an elegant fashion. (Unfortunately, however, the reader is never told if the Zenneck surface wave can be excited over a homogeneous conducting half-space.)

Most noteworthy is the material on approximate methods of analysis and design procedures for surface wave antennas. Such problems as discontinuities in surface properties and the resultant radiation are dealt with by several approaches which have various degrees of rigor. Furthermore, the analytical data are checked by careful microwave experiments to a high precision.

In the main, the book is clearly written and is a worthwhile purchase. The quality of the printing and the illustration is excellent.—*James R. Wait*

Abyssal Crustacea by J. LAURENS BARNARD *et al.;* 223 pages; $10.00; Columbia University Press, 1962.

Through the farsighted generosity of G. Unger Vetlesen, former owner of the schooner *Vema*, the results of scientific work done with the aid of this Columbia University research vessel are being published in a series of *Vema Reports*. Judging from the first volume, *Abyssal Crustacea*, we can look forward to a handsome series.

Abyssal Crustacea contains works on abyssal Atlantic amphipods and isopods respectively by two leading American carcinologists, J. L. Barnard and R. J. Menzies, and a paper by the distinguished Rumanian carcinologist, Mihai Bacescu, on species of the cumacean genus *Makrokylindrus* collected on *Vema* cruises. The high proportion of new species and genera emphasizes the paucity of our present knowledge of abyssal crustaceans. In his report on amphipods collected from 15 stations in the South Atlantic, Barnard records 53 species, of which 38 are new. Menzies adds 107 new species of abyssal isopods to the 69 previously known from the North and South Atlantic, and Bacescu describes six new species of *Makrokylindrus* from Atlantic and Pacific *Vema* stations. Barnard and Menzies give diagnoses rather than full descriptions, but this brevity is fully compensated for by the excellent and complete illustrations. Bacescu's descriptions are more

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lengthy and his illustrations are outstanding. Keys are used generously by all authors.

Each author does more than simply report on the *Vema* collections. Barnard has short but interesting sections on abyssal adaptations, the food of abyssal amphipods, and endemism in the deep sea. The scarcity of interbasin species suggests a high degree of endemism in the six basins of the South Atlantic. Barnard also gives several tables summarizing the known abyssal amphipods of the world. Menzies illustrates and gives diagnoses for all known Atlantic abyssal isopods. His studies on the zoogeography, ecology, and phylogeny of abyssal isopods are to be published separately.

Minor flaws in this fine volume are the failure to give derivations for the new names, Bacescu's omission of a designation of a type-species for his new subgenus, and Menzies' use of the categories "tribe" and "subtribe" at the superfamily level. In his report on the isopods of the Swedish Chilean Expedition, Menzies revived Monod's term "Quatuordecempedes" for the non-gnathiidean isopods. This mouthful is replaced without comment in the Vema report by the slightly shorter "Quatuordecapoda," a Latin-Greek bastard combination.

The types of the new species, originally deposited in the Lamont Geological Observatory, have now been transferred to the American Museum of Natural History, an institution much better equipped to carry out curatorial responsibilities.—*Thomas E. Bowman*

Biophysical Science by EUGENE ACKERMAN; 626 pages; $13.50 trade edition; Prentice-Hall, 1962.

An intermediate-level text with such a broad title by a single author is so difficult and ambitious an undertaking that it is remarkable to find the result so successful. Certain biophysical aspects of the classical fields of genetics,

virology, cellular and systemic physiology, and biochemistry have been selected to show how both the concepts and the methods of physics can be fruitfully applied to biological problems. In any effort of this type, at least one field is likely to suffer; in this case it is biology. The treatments, for example, of muscle and nerve physiology, of the cytological and genetic effects of radiation, and of the movement of substances across cell membranes are too dilute to be of much value for a physiology course. On the other hand, the chapters on photosynthesis, macromolecular structure, and enzyme kinetics are more complete and better organized.

The author is clearly on more familiar ground in dealing with thermodynamics, information theory, and specialized physical instrumentation. An exception is a weak chapter on microscopy.

The organization of the book is original and interesting, and many of the chapters are well written. There seems to have been some difficulty, however, in pitching the various chapters at the same general level of difficulty; some chapters (e.g., nerve and muscle) are oversimplified in the extreme, while others are well suited to a graduate student audience. The references for further reading are rather few and not always well chosen. Certainly parts of this book will interest students and scientists in a number of related fields. It will be difficult, however, to use as a complete text, because of the heterogeneity both of quality and of level of complexity in different chapters.—*Robert D. Allen*

An Introduction to the Biochemistry of the Cancer Cell by HARRIS BUSCH; 424 pages; $13.50; Academic Press, 1962.

In the voluminous literature on the cancer problem, one has to wade through innumerable research papers or, in the clinical and therapeutic fields, encyclopedic works. The present concise volume is a welcome addition to the library of not only the research worker but also the student and inquisitive clinician.

One of the outstanding services this book renders is that of emphasizing the crucial importance of the role of the nucleus and nucleolus in the dynamic processes of the economy of the cancer cell. The valuable research contributions of the author make the chapter on "Nuclear Proteins" most rewarding reading. The authoritative handling of the challenge of the nucleus to the investigator is supported by a number of lucidly written chapters which deal with modern concepts of "The Genetic Apparatus," the significance of "Deoxyribonucleic Acid of Neoplastic Cells" and "Ribonucleic Acids," and "Approaches to Cancer Chemotherapy via Inhibition of Nucleic Acid Biosynthesis." Carcinogenesis and enzymology are not discussed as intensively as other topics, probably because these have been dealt with recently in several books and reviews. The chapter on "Toxohormone" throws light on a controversial subject and will be read with interest, as will those on "Plasmacytoma" and "Antigens of Neoplastic Cells" and especially the concluding evaluation of "The Cancer Cell as a Parasite."

The extensive author and subject indices greatly add to the value of this work. The illustrations are excellent and the simplified metabolic schemes and charts are a valuable assistance in the comprehension of difficult problems. Since the book on the *Biochemistry of Cancer* by the late J. P. Greenstein a number of years pregnant with many thousands of research papers and symposia have gone by. The rapid progress has necessitated a new look at the subject and the present stimulating volume will undoubtedly contribute significantly to new advances in cancer research.—*George Weber*

Soil Management for Conservation and Production by R. L. COOK; 527 pages; $9.95; John Wiley & Sons, 1962.

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America, long known for its waste of natural resources, including the most important one—its soil, was, on the other hand, blessed with pioneers and leaders in soil science, during the last hundred years. When the time became ripe in the Thirties, the visible sins of the past, the knowledge of agricultural leaders about good and bad practices in this country and abroad, and the spirit of science and practical enterprise, all combined to give birth in this country to the development of a theory and practice of soil and land management that has been uniquely successful and in its totality is unparalleled anywhere in the world.

The book by Cook presents in simple and concise language the principles and practices of soil management for conservation and production utilizing the results of his own research and experience in addition to the body of knowledge accumulated and organized by his predecessors and contemporaries. The principles of soil management as related to physical and chemical soil properties and affecting conservation measures, appropriate tillage methods, treatment of soil acidity, and other features are treated in Chapters 1 to 13. Chapters 13 to 19 treat the cropping systems, management and fertility practices of the larger soil and climatic regions of our country, showing their functional interrelation. The last four chapters deal with the special topics of management of organic soils, garden and greenhouse soils, turf and forest soils.

This book contains a wealth of information presented in a direct and easily understood manner. It is warmly recommended as a college text for agronomy and soils students and should be good and fruitful reading for everybody interested in soils for professional or other reasons.—*Hans F. Winterkorn*

Rock-Forming Minerals, Vol. I: Ortho- and Ring Silicates by W. A. DEER *et al.;* 333 pages; $15.50; John Wiley & Sons, 1962.

As announced in the preface the Rock Forming Minerals are to be covered in

five volumes, the four still to be published being Chain Silicates (Vol. 2), Sheet Silicates (Vol. 3), Framework Silicates (Vol. 4), and Non-Silicates (Vol. 5).

In undertaking a task of this magnitude, the authors have aimed "to provide a work of reference useful to advanced students and research workers in the geological sciences." As is evident from the contents of this first volume, they have succeeded admirably.

A systematic order of presentation for each mineral or mineral group consists of a tabulation of the essential chemical, crystallographic, X-ray, optical and physical properties, together with a summary statement as to petrological occurrence. These bare details are then elaborated in subsections entitled: Structure, Chemistry, Optical and Physical Properties, Distinguishing Features and Paragenesis. The references are described by the authors as "comprehensive to 1959, but later additions extend the coverage for some sections to 1961." The work is therefore remarkably up-to-date.

The sections on Structure include a description and illustration of atomic structure and, where applicable, X-ray parameters and determinative methods. The Chemistry sections are characterized by tables of representative analyses with calculated structural formulae. Relevant experimental data on synthesis and phase equilibria are included here. The "Optical and Physical Properties" are listed and considered in relation to structure and Chemistry. Aids to identification are provided under "Distinguishing Features."

In this series of volumes, the title: "Rock-Forming Minerals" is used primarily to restrict coverage to those species "which by their presence or absence serve to determine or modify the name of a rock." The "Rock-Forming" part of the title does not necessarily imply a consideration of the petrologic significance of the minerals. There are sections on Paragenesis but they may be disappointing to some readers, as in many cases (in this volume at least) they are little more than a summary of published descriptions of mineral occurrences and associations. This, however, is a reflection on how little is yet known of the petrogenetic significance of individual minerals, rather than any serious omission on the part of the authors.

As an up-to-date and concise review of all available and pertinent data on Rock-Forming minerals, the series will constitute an exceptional reference for all earth scientists. It will also provide an excellent guide and foundation for future research. It is to be regretted that the price of the volumes will prohibit purchase by many individuals who could benefit from them.—*R. B. Hargraves*

Psychology: A Study of a Science, Study II: Empirical Substructure and Relations with other Sciences; Vol. IV Biologically Oriented Fields: Their Place in Psychology and in Biological Science, edited by SIGMUND KOCH; 731 pages; $12.50; McGraw-Hill Book Co., 1962.

"Monumental" is a term which "preface-writers" and "book-reviewers" are fond of using when considering publications which they enthusiastically welcome. In the case of *Psychology: A Study of a Science*, the adjective is truly justified. This series of seven large volumes (three remain to be published) will stand as a useful monument to psychology's first 100 years.

The series is divided into two "Studies" of three volumes each, with a final "Postscript to the Study" (Volume 7) to be written by the editor, Sigmund Koch. Study I, recently published, is concerned with the conceptual and systematic aspects of psychology. Study II is titled "Empirical Substructure and Relations with other Sciences."

Volume 4, dealing with interrelations between psychology and biology, is the first volume of Study II. It contains a 40-page introduction to Study II and 14 papers written by geneticists, physiologists, zoologists, and neurologists, as well as psychologists. The presentations are, for the most part, unified statements by a single scientist. Throughout Study II, an effort has been made to include chapters by younger investigators, although the editor makes "no claim to 'New Frontier' standards"

The chapters are wide in scope, and consequently difficult to classify as to subject matter. In general, however, the contents are as follows: Five chapters are on the nervous system and neurophysiology, one deals with interrelations between psychology and genetics; four review sensory processes in relation to behavior, physics, and physiology; one long chapter, called "Biological Psychology," considers "issues in biological inquiry that concern learning"; and three chapters are devoted to perception.

Although there is an explicit disclaimer to complete coverage of the area, specialists will inevitably question the inclusion of certain topics and the exclusion of others. For example, "Transactional Psychology," the topic of the last chapter, hardly leaps to mind when one considers biologically oriented fields of psychology. Conversely, a chapter dealing explicitly with hormones and behavior, or one concerned with the biochemistry of behavior, might have been welcomed.

Each chapter is a major endeavor and each should prove valuable as reference and instructional material for many years. Since the design of the series places conceptual formulations and empirical substructure in different volumes, most biologically-oriented psychologists (and behaviorally-oriented biologists) will find it desirable to purchase Volume 1, as well as Volume 4, in order to have available the material relating to their specialty.—*Thomas E. McGill*

The Psychology of Jung: A Critical Interpretation by Avis M. Dry; 329 pages; $6.00; John Wiley & Sons, 1961.

This evaluative survey written "with a view to rendering the psychology of Jung more understandable to interested, though uncommitted, readers," treats his work of the early, middle, and late periods, its appeal, historical and social background, his religious contributions, and Jungian therapy. Each of these is capably and honestly evaluated as is Jung's critique of Freudian theory. The influence of Jung on psychologists and analysts of all sorts is painstakingly

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delineated. The work is well written with frequent summaries which enable the reader to keep the thread.

This work reaches essentially the same conclusions as those of Edward Glover in his *Freud or Jung*, with meticulous documentation of every point. Dry presents evidence of the "baffling quality" of Jung's later work and his tendency "to abandon in his work that habit of orderly and argued exposition which forms an accepted part of the rational approach." She calls our attention to his "blurring of historical facts." She notes how his symbolic method leads to a "failure to consider the outer world" and that his interpretations, particularly of religious symbols, are often "unnecessarily complicated." Jung's tendency to ready generalization and his resultant inconsistencies are documented and Dry attempts to make interpretive interpolations. In her evaluations, she points out his blindness toward sex and obvious unfamiliarity with Freud's later ego psychology formulations, which are consistent with his "oversimplified picture."

Dry attempts to account for Jung's changes by hypothesizing that he was highly suggestible (noting his almost simultaneous support and rejection of Nazism as an example). The suggestible Jung was influenced by the borderlines and psychotics who were his patients. As early as 1914, he had left Bleuler and Freud and resigned his university post. To this and to his being withdrawn in quiet Switzerland, she attributes his system's inwardness and lack of connectedness or rather the blanket connectedness of everything with everything else. This may also account for his appeal. Those who seek easy mystical answers—and the self-help market is very big—will go to Jung. Dr. Dry also points out that much of his influence was personal. He was a man of immense vitality and his manner engendered instant respect.—*Richard O. Rouse*

Psychological Practices with the Physically Disabled, edited by J. F. GARRETT and E. S. LEVINE; 463 pages; $8.75; Columbia University Press, 1962.

Modern rehabilitation philosophy recognizes that the psychological aspects of physical disabilities are at least as important to the total person as his medical needs. Nevertheless, surprisingly little of a systematic nature is known about the relationship between normal or pathological somatic conditions and global psychological functioning. Any conscientious effort to add to the store of knowledge on this subject is therefore to be warmly welcomed.

The editors of this book have brought together the work of twelve experts, each writing on the subject with which he is most familiar. Every chapter is devoted to the discussion of a different physical disability or disability-type; and, with minor variations, all chapters follow the same topical outline, giving the volume a coherence that is often lacking in collections of this general type. At the same time, the book loses a certain necessary continuity by the very nature of its organization; for if there is one thing certain it is that the psychological problems of adjustment to disability are not a simple function of either type or degree of somatic impairment. This, the reader will doubtless conclude for himself as he examines the contents of the several chapters.

In a new field of study, the best work is often that which admits the most ignorance and recognizes the need to begin at the beginning, with the formulation of meaningful, testable propositions. Much of the psychological material in this book must be treated on the level of the "educated guess"; and the psychologically sophisticated reader will detect in it more than an occasional hint of stereotyped thinking and hasty generalization. But rehabilitation practice demands action, so opinions often serve as premises where solid theory is lacking. This volume presents the best opinions psychologists currently have to offer.—*F. C. Shontz*

The World of Ice by JAMES L. DYSON; 305 pages; $6.95; Alfred A. Knopf, 1962.

Only occasionally appears a book on a scientific subject that manages to get its story across to the non-scientific

public so successfully that the general reader not only understands and is intrigued by the story but also grasps something of the thrill inherent in scientific research and the reward of scientific accomplishment. "The World of Ice" is one of these. It is an extraordinarily fine book.

One wonders, in going through the volume, what it is that makes it effective. It seems to add up to a thoughtfully planned, painstaking effort of a competent craftsman. The author not only knows his subject technically but clearly has thought about it carefully in terms of people and human history. To this is added a flair for description and a feel for the appropriate anecdote or analogy. Finally the book is helped by a pleasing appearance and an abundance of carefully selected and well reproduced photographs.

A useful device that the author employs to the limit, perhaps even a little too much, is to tie the World of Ice to well-known historic incidents that are known to almost everyone. The selected incidents range widely, many relate obviously to the subject. Others are less directly related. Nevertheless in most cases the author makes his point with them. Some of the items selected include the infamous Donner party incident, the sinking of the Titanic, the well-publicized visions of the damming of the Strait of Gibraltar and the Bering Strait, the tragic loss of the Hans Hedtoft, and even the Loch Ness monster, the abominable snowman, and the cooling of explorers' high balls with glacier ice thousands of years old.

Professor Dyson employs a similar technique in interpreting geologic events or other natural phenomena in terms of people and the modern world. There follows just one example—a quote from page 34 where the striking advance of the Black Rapids Glacier in Alaska is being described.

"This spurt of the glacier might have gone unnoticed but for the fact that the Rapids Roadhouse, a hunting lodge on the Richardson Highway near the Black Rapids Glacier and directly in its path, was occupied during the winter of 1936–1937 by the H. E. Revell family. For

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several weeks during the early part of the winter the Revells heard rumbling sounds coming from the direction of the glacier. Since the glacier was not visible from the lodge and because the sounds were accompanied by tremors, they were attributed to earthquakes. But on the third of December Mrs. Revell, while looking up the glacier valley with field glasses, was startled to see a huge jumbled mass of ice blocks about six miles distant. During the next few days as the Revells watched this wall of ice grow higher, they realized that the glacier was rapidly bearing down on them. They could see that the blocks of ice were jostled about and that many were gradually tilted until they toppled over and fell in a cloud of snow and small fragments, followed by a roar and by a jar that rattled the windows of the roadhouse. It was then that this obscure glacier, through a report sent by Mrs. Revell to the U.S. Weather Bureau Station in Fairbanks, made the headlines and became known to millions of people."

One detects in the author a somewhat cynical attitude toward political problems and toward the efforts of men to live together on our earthly planet. These do not add much to "The World of Ice" and would not be missed if omitted. For example, on page 109, in speaking of Antarctica—"Night clubs may arrive before we know it; golf courses will pose a greater problem. Already a planeload of sightseeing U.S. Congressmen has made a trip to Antarctica." And on page 249—"Man is a doer of big things. He has probed the depths of the Earth with his seismograph and has learned the secrets of outer space. Of course, he has not yet learned to live with his fellow man, but he can turn missiles loose to do his bidding miles and miles away . . . ." Or on page 270—"Man doesn't know what his fate will be fifty years hence. Through the best leaders he can put in office now, he is hardly capable of determining his future from one year to the next."

Occasionally Professor Dyson uses an apt analogy to make a point. In speculating on the possibility of mineral deposits under the Antarctic ice, he says—"Would the fabulous iron ores of the Lake Superior region have been found if the exploration of North America had been limited to a marginal zone less than fifty miles wide?"

The book is well worth reading. Anyone who does so will have a real appreciation of the relation of man to the World of Ice. As the author says in his introduction—"It is vital that we learn more about these things—and they make a fascinating story. That is why I have written this book."—*John C. Reed*

Fundamentals of Mid-Tertiary Stratigraphical Correlation by F. E. EAMES *et al.;* 163 pages; $9.50; New York: Cambridge University Press, 1962.

The authors of this stimulating but controversial volume have taken a mass of anomalous distributional data for fossils, and relying extensively upon planktonic foraminifera, have proposed a new set of nearly world-wide Mid-Tertiary stratigraphical correlations. As a result, the marine Oligocene does not appear to be well represented in many parts of the world and notably so in the Caribbean and Tethyan areas. As a corollary, most of the strata previously assigned to the Oligocene in these parts of the world are referred to the Aquitanian (accepted as lower Miocene). To accomplish this striking result "reworking" is appealed to innumerable times to explain fossil associations not in accord with their ideas. Many of the cited instances of "reworking" are incontestable but the multitudinous appeals to it stretch the bounds of credulity.

In one of the few references to the well-studied foraminiferal sequence of the Pacific Coast, the lower Relizian of California is correlated with the *Globigerina ciperoensis ciperoensis* zone of the Caribbean and Sicily. Inasmuch as unequivocably older Pacific Coast faunas (lower Saucesian-upper Zemorrian) are commonly correlated with Caribbean faunas several zones younger than the *G. ciperoensis ciperoensis* zone, this correlation is rather amazing.

In Part II the authors (F. T. Banner and W. H. Blow) of the systematic

study of the Globigerinaceae admit that their taxonomy is biased toward stratigraphic utilization and does not necessarily reflect evolutionary relationships. In a similar vein the restriction of *Pliolepidina*, to the lower Miocene is accomplished only by adopting a narrow concept for the genus and appealing to reworking to explain the numerous occurrences of undeniable Eocene larger foraminifera often associated with it.

The proposed correlations will undoubtedly form the basis for many, and probably at times heated, discussions, but in order for the proposals to be adequately evaluated and tested, it will be necessary for many paleontologists and stratigraphers to improve their techniques and sharpen their methods.—*J. Wyatt Durham*

Plants—An Introduction to Modern Botany by VICTOR A. GREULACH and J. EDISON ADAMS; 557 pages; $7.50; John Wiley & Sons, 1962.

Although this is intended as a text for a one-semester course, it is a sizable volume. It departs from the traditional to a considerable extent, although it is not wholly revolutionary—and probably is the better for it.

The book is divided into four sections. In the first, Man and the World of Plants, the economic significance of plants is considered. This is followed by a relatively short outline—40 pages—of the plant kingdom—briefer than in most texts. Section two, Levels of Plant Organization, starts with molecules—26 pages—including DNA, and goes on to cells, tissues and organs, organisms, and communities. Section three, Plants in Action, deals with such topics as "Molecular Traffic," transpiration, absorption, water transport, energy transformations, photosynthesis, respiration, assimilation, and digestion. Growth, development, and morphogenesis are given considerable attention. This section closes with a discussion of "The Biological Environment." Section four, From Generation to Generation, treats of asexual and sexual reproduction; heredity, with emphasis on the chromosomes and their structure; the fossil record; and closes with a modern presentation of the origin of life. There is also an appendix—Some Basic Chemistry for Botany Students. Each of the 19 chapters concludes with questions, and references which are mostly recent citations.

The illustrations are effective, the diction is clear, and the book is interestingly written.

There is no doubt that the marked advances in the physical sciences during the last several decades are changing the emphasis and the direction of biology. This does not mean that all that is classical must be discarded. Rather it suggests that a fuller understanding of the phenomena of life is being achieved, and that the outlook for the future is both rich and almost limitless. Happily, this is reflected in the present thoughtfully prepared volume.—*Edwin B. Matzke*

The Physiology of Flowering by WILLIAM S. HILLMAN; 164 pages; $4.50; Holt, Rinehart & Winston, 1962.

The physiology of flowering offers much that is intriguing. We are dealing with one of the fundamental phenomena of biology—that of morphogenetic redirection of growth. A stem which has produced nothing but leaves and more of itself may quite suddenly change its direction of growth so that flowers are formed. Often this occurs in response to changes in environmental factors such as temperature or day-length. It is impressive to realize that species such as the cocklebur or the Japanese Morning Glory will remain vegetative so long as they receive dark periods less than some maximum length, but if only one dark period exceeds this length, sometimes by only as much as a few minutes, the plant will become reproductive.

William Hillman has told this fascinating story very well indeed. His rather informal, free-wheeling way of writing is basically easy to follow and often provides pleasant reading. His coverage of the topic is broad, and his insight into the problems is acute. He provides much more than a simple review of literature, although the volume is excellent in this respect. There is much interpretation;

often most ingenious. His statement of the facts seems to be generally accurate (although he completely missed a pet point relating to one of the reviewer's projects!). If the book does nothing more, it at least presents a true picture of present research on the flowering process as a field which is only on the threshold of real discovery and generally quite confused.

The subject is so confused, and in latter chapters this becomes so apparent, that it might be quite difficult for the average reader to keep his attention riveted on the topic at hand. Conclusions can seldom be applied to the field as a whole, although ideas about physiological mechanisms may sometimes approach the truth as it relates to one kind of plant. Thus, even though the book attempts a broad coverage, its size limitation prevents it from being truly comprehensive.

Hillman discusses this problem at some length in his introduction. He decided to try for the true picture of the whole field. Yet, this decision may have cost him much of his audience, and the size of the book prevented it from being realized anyway. An alternative might have been to restrict his coverage somewhat and attempt to intensify his discussion of one relatively coherent aspect of the topic.

The book is essential for the worker in the field. This would be true even if the worker is already familiar with the literature, since Hillman's interpretations and original ideas are often quite valuable. I would also recommend the book for anyone else who might be interested and willing to separate the kernels of wisdom from the chaff of confusion in this short and pithy little volume.—*Frank B. Salisbury*

The Physiological and Behavioral Aspects of Taste, edited by M. R. KARE and B. P. HALPERN; 149 pages; $6.00; The University of Chicago Press, 1961.

The book is a collection of ten papers on various aspects of taste that were delivered at an interdisciplinary symposium on taste at Cornell University in June 1960. It is prefaced with an excellent discourse on taste by Pfaffmann, who unfortunately did not attend the symposium. The book spans such topics as electrophysiology of taste at the level of the receptors and the thalmus, glucose appetite regulation, comparative aspects of taste, metabolic factors, preference testing, and taste interactions.

Both comparative taste preference and electrophysiological studies indicate that each species lives in its own taste world, sometimes quite different from that of closely related species. Even though recent and careful experimentation by Moulton disproved the myth that albino rats have a poorer olfactory ability than those pigmented, the myth is still considered by some researchers.

The discussion by Frommer, Benjamin, Kitchell, and Halpern following the chapter on the taste responses recorded in the thalmus is very enlightening and resolves some of the apparent differences in previously published material. The relative magnitude of responses of the taste areas of the thalmus to various chemical stimuli are very similar to those recorded from the chorda tympani nerve. Little overlap of taste and touch occurs in the thalmus, according to Benjamin.

The importance of studies of human taste behavior as well as that of other animals is stressed throughout the book. Of particular importance is the work of Pilgrim on interactions between two suprathreshold taste stimuli presented simultaneously.

Are there but four taste qualities; sour, bitter, sweet, and salty? How well do the electrophysiological responses from the periphery indicate probable behavioral responses of the animal? These two questions are discussed several times throughout the book but with little resulting agreement on the part of the participants.

Much of the material used in the book was published previously. The discussions following each paper were very helpful and show the need for more symposia like those sponsored by the Macy Foundation, where any weak or erroneous concept proposed by a participant is immediately challenged and

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differences in nomenclature reconciled. —*Lloyd M. Beidler*

Forces and Fields by MARY B. HESSE; 318 pages; $10.00; Philosophical Library, 1962.

Since at least the seventeenth century the problem of action at a distance—can matter act where it is not?—has perplexed both scientists and the philosophers who have studied and criticized their works. For the strict mechanists, who played so large a role in the origin of modern science, causal action could occur only through contact. That was a principal thesis in their rejection of Aristotelian and scholastic science, and until the mid-eighteenth century it led many of them to reject the *Principia* as well. In the nineteenth century, residual discontent with the notion of instantaneous action across empty space was one source for the concept of space-filling physical fields. The success of field theories soon challenged the magistral Newtonian model which had long been extended from mechanics to the whole of physics, and some of the resulting problems were central to the emergence of relativity and quantum mechanics. The twentieth century revolution in physics has only transformed not eliminated them. Reconciling the field, as continuum, with the atomic and subatomic, as discrete, is only one element in a problem-complex which, inherited from earlier ages, is today more acute and more central to physics than before.

The subtitle of Mary Hesse's *Forces and Fields* is *A Study of Action at a Distance in the History of Physics,* and her book's first purpose is to trace the evolution of ideas about physical causal action from primitive science to our own day. Three chapters carry the narrative from pre-history to the end of the Middle Ages; three more consider the seventeenth and eighteenth centuries; and the last three (excepting a conclusion) treat nineteenth century field theories, the theory of relativity, and quantum mechanics including electrodynamics to the Wheeler-Feynman theory of the mid-1940's. The ground covered is richer than a brief review can even hint, and the last third of it has scarcely been seriously treated before. Since Dr. Hesse's description and analysis is always careful, usually clear, and often both perceptive and incisive, her book should be widely studied by all those concerned with the evolution of modern physics. One hopes that a paperback edition will soon make it more generally and economically available.

Dr. Hesse's purpose is not, however, simply historical. She, herself, is perhaps even more a philosopher than a historian of science. As a result she sees the history of action at a distance as a case study which can illuminate the nature and status of physical theories and disclose the line, if any, which separates such theories from metaphysical speculation. In addition, particularly in her last chapters, she is concerned to discover what the most recent physical theories have to say about the existence of action at a distance itself. On all these topics she has wise and important things to say. Her opening chapter combines an elegant summary-criticism of logical positivism with some essential suggestions for reform, and she returns occasionally to these topics during the course of her historical narrative.

The explicitly philosophical dimension of Dr. Hesse's book is for me at least as rewarding as the explicitly historical, but the attempt to combine the two is a source of the book's main weakness. History and philosophy of science can do much to illuminate and stimulate each other. Dr. Hesse's book is a noteworthy attempt to prove the point. Yet the writing of history differs from that of philosophy in its main concerns, values, and principles of organization, and Dr. Hesse has not always succeeded in reconciling the two. During much of the book her concern with evolution of ideas leads her to provide more data, and sometimes different data, than her philosophical concerns require. Too often her essential philosophical and analytic remarks seem asides, insufficiently attached to the material that has preceded and insufficiently developed to convince the sceptic. Elsewhere, philosophical concerns dominate the presentation of apparently historical materials, and the reader's sense of ideas

in development is then sometimes lost. Since *Forces and Fields* is, in any case, physically too short a book for the immense range of topics at which Dr. Hesse aims, the inevitable result is frequent unevenness and occasional incompleteness of treatment. Nevertheless, these are only imperfections in a generally stimulating and informative book, one that will be widely and profitably used for a very long time.—*Thomas S. Kuhn*

Stereochemistry of Carbon Compounds by E. L. ELIEL; 486 pages; $15.00; McGraw-Hill Book Co., 1962.

This welcome entry represents, to my knowledge, the first textbook on organic stereochemistry to be published in this country. As a consequence of the increasing importance and pervasiveness of three-dimensional views of compounds and reactions, it is sure to find a receptive audience. Professor Eliel has not permitted his "squatter's rights" in the field to lower his standards, either; he has written a lucid, accurate, thorough, and up-to-date account of stereochemistry, which is a pleasure to read.

Designed for advanced undergraduates and beginning graduate students (but certain to be useful to nearly all organic chemists, no matter how long past their student days), the book covers the classical aspects of optical and geometrical isomerism, racemization, and resolution, relative and absolute configuration, and the special features of ring compounds, then continues with an especially valuable and thorough discussion of conformational analysis in both cyclic and acyclic systems. Three final chapters treat the stereochemistry of unstable intermediates, optical rotatory dispersion, recent theories of the relation between structure and optical activity, stereoselective syntheses and stereoregular polymers.

The entire work is characterized by precision in definition and discussion, thorough documentation, and an unusual appreciation of thermodynamics and kinetics in stereochemical problems which is particularly worth while for students. Only a few errors were detected, none of which would seriously mislead the reader.

This book is likely to become quickly a standard text for organic chemists, and can be recommended highly.—*Richard K. Hill*

Plasticity and Creep of Metals by J. D. LUBAHN and R. P. FELGAR; 608 pages; $16.75; John Wiley & Sons, 1961.

As parts of structures and in the processes in which they are deformed mechanically, metals are usually subjected to complex stresses which may vary markedly with time. Long term service is often expected of structural members. On the other hand, testing is usually done on specimens of simple shape subjected to simple stress patterns. The testing period is seldom comparable in length to the desired service life.

Lubahn and Felgar give detailed and critical analyses of the empirical bases of the common mechanical testing procedures, a phenomenological account of effects observed with metals and alloys over wide ranges of stress level, strain rate and temperature, and formal mathematical descriptions of the macroscopic properties. It is a considerable feat to write a book of this length primarily on metallic plasticity with no mention of dislocations. Although the authors are to be admired for steadfast adherence to their phenomenological approach, the penalties appear in the frequent use of vague phrases like "metallurgical changes" and "metallurgical reactions causing softening" which seem to imply a much greater difference between processes that involve dislocation motion and rearrangement than there is. The rather wordy, repetitious and loose style could have been tightened to yield space for a forthright description of at least some atomic scale mechanisms.

The use of test data to estimate the behavior of alloys in forming processes or under complex service conditions is described in great detail with many valuable examples set forth explicitly. The complications are introduced progressively, so that this book should be

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useful for training future designers or as a constant companion for those engaged in design work. Numerous problems are also included. In view of the nearly complete absence of discussion of fracture and of the crystallography of deformation processes, together with only cursory mention of the development of preferred orientations, the prospective reader should be prepared to study these matters further before placing too much confidence in his designs.

Although there would seem to be no real restriction of the methods outlined in this book to metallic materials, the ranges of the variables are likely to be different for nonmetallic materials and brittle fracture a relatively more pressing problem.—*C. E. Birchenall*

Immunochemical Approaches to Problems in Microbiology, edited by M. HEIDELBERGER and O. J. PLESCIA; 402 pages; $6.00; Rutgers University Press, 1961.

The symposium published in this book covers a broad range of topics which are pertinent to many fields of biological interest. The papers are mainly summaries of past work and several are very good, detailed reviews. People took the stances that might be expected and there were no spectacular conversions despite active proselytizing. In general, there is broad coverage of immunochemical techniques and their applications to structural studies plus much speculation about the significance of the results. The clear necessity to view immunochemical reactions in two different ways emerges from the symposium: (1) as analytical tools which must be calibrated by other methods or used in a strictly relative sense and (2) as manifestations of highly specific biochemical reactions whose mechanisms still present many problems.

The symposium was organized in five panels each of which was followed by discussion. Panel 1 (Techniques) gives a good coverage of immunochemical techniques, especially precipitin reactions in gel, and the uses of radioisotopes. Panel 2 (Bacterial Antigens) discusses the composition of the bacterial cell wall with emphasis on the teichoic acids and mucopeptides containing muramic acid. The ultimate aims of such studies are the knowledge of the mechanisms by which the cell wall is synthesized and an extensive bacterial taxonomy based on the antigenic microstructure of the cell wall. Panel 3 (Viruses) illustrates the uses of immunochemical methods in the study of the virus coat and in following the synthesis of internal protein by the T-even coliphages. The work in this area is more recent than in the comparable bacterial studies and hence many concepts are defined in quite operational terms with the attendant difficulty of clearly defining the materials under study. Panel 4 (Non-Specific Factors) presents very good summaries of the complement and properdin systems and emphasizes that much has yet to be done before clear chemical definition of the respective factors involved will settle some of the long-standing controversies. Panel 5 (Biosynthesis) discusses studies on the biochemical synthesis of the pneumococcal polysaccharide, including the metabolic basis of some transformation reactions, and the various theories of antibody production. The paucity of experimental evidence concerning the latter process is set out in bold relief by a background of uninhibited speculation.—*Thomas J. Gill, III*

Irradiation of Mice and Men by JOHN F. LOUTIT; 154 pages; $5.00; The University of Chicago Press, 1962.

The avowed purpose of Dr. Loutit's series of lectures in the fall of 1961 at the University of Chicago, upon which the present volume was based, was to discuss the problems of the biological hazards of nuclear energy. The University audience was quite heterogeneous; I suspect that the reading audience might be as well. However, I am at a loss to know just which individuals should be encouraged to read this book. The difficulty arises from the fact that some sections have been written for the average reader and some for the general biologist, while other sections can be of interest only to the specialist. And none are easy reading.

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American Scientists and Nuclear Weapons Policy

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This well documented book will be of lasting value to both scientists and public administrators, and it will be of vital interest to all concerned with current problems of the nuclear era. The author contends that the emergence of the scientist into the mainstream of American political life is one of the great events of our history. As he assays the situation, he emphasizes the growing need for effective measures for integrating scientist-advisers into national policy-making processes. "In this study of the political behavior of American scientists since 1944, Dr. Gilpin has made a significant contribution to understanding of the processes of decision-making in vital areas of our national life."—*Hugh C. Wolf.* *364 pages. $6.95*

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By P. Ya. Polubarinova-Kochina, Translated from the Russian by J. M. Roger De Wiest

This important work, originally published in Moscow in 1952, is considered to be a classic in the field. It not only gives valuable insight into the status of ground water hydrology in the U.S.S.R., but it also fills a need for further mathematical treatment of the subject. Mrs. P. Ya. Polubarinova-Kochina is at present with the Institute of Hydrodynamics, Novosibirsk.
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Perhaps the greatest value to the non-biologist is the opening section on "Radiation and the Facts of Life." The last sentence, however, left me somewhat apprehensive of what I would find next. "Furthermore there are natural defenses against infecting organisms . . . but we have as yet little knowledge of what natural allies we have against the agents which cause the shades of the mortuary to close around the growing boy."

This tone, fortunately, does not prevail, and a great deal of useful information and sound discussion is included in Chapter Two on "Cancer, Leukemia, and Longevity." The last chapter, "Strontium 90," however, was a disappointment because it makes no effort to evaluate the hazard of current or predicted levels of contamination. On the other hand, readers interested in bone metabolism, particularly in reference to calcium and strontium, will find this section very worth while. This area of research has occupied much of Dr. Loutit's time in recent years, and he has provided an excellent account in about fifty pages.—*Miriam P. Finkel*

Secretory Mechanisms of the Gastro-Intestinal Tract by R. A. GREGORY; 240 pages; $8.00; The Williams & Wilkins Co., 1962.

This book is concerned with the regulation of delivery of gastric, pancreatic and intestinal secretions to the lumen of the gastrointestinal tract in response to food. Accordingly, physiological rather than biochemical mechanisms are considered. The author has been a significant contributor to this field for the past twenty years. He has limited his critical review to the experimental findings which have been obtained on non-anesthetized dogs from the time of Pavlov (1900) to the present.

The known physiological mechanisms are regulation by a nerve reflex, by a non-specific circulating agent, and by a specific hormone. The author clarifies the evidences for the existence and the significance of these mechanisms for fluid, electrolyte and enzyme content of digestive secretions. As examples of the complexity of this field, gastric and pancreatic secretion are under both neural and hormonal control, but these mechanisms are not necessarily independent. Also, the gastric secretory response to a hormone "gastrin" is similar to that of a non-specific agent histamine. The pancreatic response to a hormone "pancreozymin" is almost duplicated by nerve stimulation. Finally, the existence of hormones proposed for regulation of intestinal secretion is questioned.

Where quantitative evidence sufficient for statistical evaluation is lacking in the author's logical development of his discussion, he presents typical findings and unpublished observations. Thus, many areas wherein further work is needed are evident to the interested reader. The book admirably furnishes a foundation for experimentation in the physiology of gastrointestinal secretions. —*John H. Annegers*

The Fishes by URL LANHAM; 116 pages; 24 plates; $5.00; Columbia University Press, 1962.

This is an excellent little book on the general zoology of fishes. It is clear and well-written and would make good reading for the student of elementary biology or the fisherman who wants to know more about his prey. The only pity is that, considering its audience, it should be so expensive. Let us hope that soon it will appear as a paperback where it is even more likely to find its audience. —*John Tyler Bonner*

A Naturalist in Alaska by ADOLPH MURIE; 302 pages; $6.50; The Devin-Adair Co., 1961.

A few years ago, by some luck, I ran into a government pamphlet on "The Wolves of Mount McKinley" by Adolph Murie and I was spellbound by an extraordinary account of the habits of wolves in the wild. In a painstaking series of observations (and I imagine frigid ones as well), Murie was able to observe family activity and, even more remarkable, the hunting activities of single wolves and of wolf packs. It is indeed a fortunate thing that the heart of this pamphlet has now appeared in a few chapters of "A Naturalist in

Alaska." But we are even more fortunate in that, besides the wolf study, we also find similar studies on the lynx, the grizzly bear, the caribou, the Dall sheep, the wolverine, the arctic fox, and even a delightful chapter on haymaking mice.

This is a wonderful book that is bound to please everybody. The mammalogist and the ethnologist will find it of considerable scientific importance in their fields, the general biologist, both student and teacher, cannot help but benefit and enjoy reading it. Even without these professional qualifications, the layman will be equally rewarded and fascinated (I know for I have tried it out on some friends). This is a work in the fine tradition of Ernest Thompson Seton and is definitely recommended to one and all.—*John Tyler Bonner*

Atomic Spectra by H. G. KUHN; 436 pages; $13.00; Academic Press, 1962.

For more than a quarter of a century the standard works on atomic spectra have been the detailed textbook by White and the theoretical monograph by Condon and Shortley. Both these works are still indispensable to the student of atomic spectra, whether physicist, chemist, or astronomer, but a need has long been felt either for revised editions of these books or for similar treatments bringing the story up-to-date. The work of Condon and Shortley was recently supplemented (but not replaced) by the two excellent volumes by Slater, yet no one can deny the timeliness of the appearance of the more elementary book under review.

Despite certain shortcomings, Kuhn's book is a welcome addition to the literature on atomic spectra and can be recommended as an introduction to the field. It is written somewhat on the theoretical side, but without rigor, thus tending to be purely descriptive. The elements of the matrix methods that are used in the study of atomic spectra are introduced early in the text and invoked throughout. The treatment of nearly all the topics covered is standard, and a college senior or graduate student should have no difficulty in following the text. Its value to research workers is, however, limited because of insufficient detail.

The book is divided into seven chapters, the first being a short introduction. The remaining six chapters treat in succession: II, theoretical methods, including the Schrödinger and matrix approaches; III, simple spectra (hydrogen, helium, alkalies, etc.); IV, x-ray spectra, the periodic table and the "Aufbauprinzip"; V, complex spectra; VI, hyperfine structure; and VII, the shape and width of spectral lines.

The chapter on hyperfine structure, a field in which Professor Kuhn has himself made many significant contributions, is especially worth reading, as is Chapter V on complex spectra. Chapter V also introduces the Coulomb and exchange integrals and gives energy expressions for a few simple configurations in terms of them. The text is amply documented by references to 467 individual papers that appeared from 1883 to 1959.

Regrettably, the material on the known energy levels of various spectra appears to have been taken directly from the tabular data presented in the well-known volumes *Atomic Energy Levels* (A.E.L.), compiled by Mrs. Moore-Sitterly. Some of the data in Kuhn's book suffers therefore from being only as up-to-date as the A.E.L., the first volume of which appeared in 1949. The revised and corrective material found at the end of the second and third volumes of the A.E.L. was apparently overlooked by Kuhn. Thus Kuhn's statement (p. 174) that the connection between the singlet and triplet term systems of Be has not been found is in error (N.B.S. Jour. Res., *50*, 131 (1953)) and an important term ($1s^2 2s 2p 3s\ ^4P^0$) of B is now known (J.O.S.A., *46*, 100 (1956)), upsetting the statement (p. 303) that the B Grotrian diagram (p. 194) contains all known terms. More serious is the case of fluorine, whose term diagram given on p. 314, stated to contain all known terms, is completely invalidated by the beautiful analysis of this spectrum by Lidén (Ark. f. Fysik, *1*, 229 (1949)).

A further inaccuracy is found on page 8, where, just after a statement that wavenumbers are always given in reciprocal centimeters, Edlén's expres-

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sion is given for the refractive index of air. This expression is useless to the reader unless he knows that the wavenumbers occurring in it are to be expressed in reciprocal *microns*.

There are two particularly annoying features of this book: (*a*) The absence of ruled lines to enclose tables and figures causes tables to fuse disturbingly with the text (p. 40) and tables to fuse with figures (p. 93); and (*b*) The absence of chapter and section numbers at the top of each page forces the reader of this well cross-referenced text to go to the Table of Contents for page numbers each time he wishes to follow up the cross references.

This is a well-produced book, set in large, clear type and written with clarity. In spite of our criticisms, the book can be studied with great profit by a large class of readers.—*Jack L. Tech*

Elements of Infrared Technology: Generation, Transmission, and Detection by P. W. KRUSE *et al.;* 448 pages; $10.75; John Wiley & Sons, 1962.

This book was written for advanced students, scientists, and engineers who are using or developing components for infrared applications. After a short, interesting history of the discovery of infrared radiation, the subject is separated into the three main divisions covering the generation, transmission, and detection of infrared radiation. Mathematical derivations are given for the principal physical theories accompanied by numerous diagrams. A second volume to follow will treat the integration of infrared components into systems and their commercial and military applications.

The most important types of sources are discussed, including diagrams of the spectral distribution of radiation from black bodies and the emissivities of the materials in common use. Masers are only briefly discussed. The section on the optical properties of media includes a table giving the pertinent physical and chemical properties of infrared transmitters. This is supplemented by a listing of the properties of optical materials and some of their commercial sources. A whole chapter is devoted to the optical properties of the atmosphere. Because of their applications in detectors there is a chapter on the physics of semiconductors. Detection systems are thoroughly covered and the most recent technical developments are described. The sources of noise in components and from the radiation background is investigated and there is a mathematical analysis of the photon noise limit. The properties of elemental detectors are compared. The book ends with appendices containing useful engineering approximations relating to detectivity. At the end of each chapter are lists of references for further study.

The section on vibration-rotation spectra is too short to be of great value, but this subject has already been well reviewed. Likewise, no attempt has been made to give a complete treatment of statistical mechanics.

The authors have made a worthy contribution to the literature on infrared technology. This volume should prove of value both as a text and a handbook of data on components and materials.—*Millard Maienthal*

Superfluid Physics by CECIL T. LANE; 226 pages; $9.50; McGraw-Hill Book Co., 1962.

Almost every scientist remembers with pleasure having attended a specialized course of lectures, which was by [illegible] ns an essential part of the "basic no mea[illegible]m" and which could [illegible] curriculu[illegible] dispensed wi[illegible] have been [illegible], but which, because of the enthusiasm of the lecturer and his flair for exposition, created a deeper impression than more essential courses. Cecil T. Lane has been giving such a course to first year graduate students at Yale for the past ten years. It starts with a brief account of the techniques used to obtain low temperatures and ends with a brief, but lucid, account of superconductivity. The heart of the matter, however, is Professor Lane's own first love, the superfluidity of liquid helium. It is doubtful whether the flavor of a course like this can ever be transferred to a book, and the book under review, although admirable in many ways, is curiously disappointing. There must be very few universities

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where such a specialized course can be fitted into the crowded first year of graduate studies, and the book is presumably intended for a wider audience. Certainly, any reader who asks only for the pleasure of learning about interesting things, will derive much excitement from Professor Lane's lucid accounts of such unusual phenomena as second sound, or quantized vortex lines. He will wonder, though, if the first, rather technical, chapter on Production and Handling of Liquefied Gases was really necessary. He might also wonder why, if superconductivity was to be included at all, it should have been given only one chapter of 43 pages, as compared with the seven chapters occupying 151 pages devoted to liquid helium. The serious reader, engaged in research on this or some closely related subject, will also find the book inadequate. It is limited in scope, dealing only with those aspects of the subject which the author finds most interesting and at a level understandable by first year graduate students. One or two errors have been allowed to creep in. For example, in the discussion of the scattering of neutrons by liquid helium, it is stated that a neutron collides with a roton initially at rest, whereas in fact the neutron *creates* a roton. A particularly annoying feature of the book is the relegation of important matters to footnotes, which occur with irritating frequency. One of the valuable features of the book is an excellent discussion of rotating helium and quantized vortex lines, yet the physical arguments for quantizing a vortex line are given in a footnote of 140 words spreading over two pages. Nevertheless, much of the excitement of the original lectures is there, and the tolerant reader will find his tolerance rewarded by much pleasure.—*K. R. Atkins*

Medieval Technology and Social Change by LYNN WHITE, JR.; 194 pages; $6.00; Oxford University Press, 1962.

We who have felt the impact on American life of the automobile, electricity, the airplane, T. V., nuclear power, rocketry, and the zipper need little rhetoric to be persuaded that vast social changes may come from inventions. Many however may get a fresh view of the Middle Ages from Professor White's interpretative study, which should dispel any lingering prejudices about what was once condemned as a static era.

The author finds three "revolutions" in the millenium following the barbarian invasions. These may be described briefly and baldly as follows. First, a military revolution which began when the Franks under Charles Martel adapted the foot-stirrup (presumably of Chinese origin) to the use of a heavy-armed lanceman whose shock power dominated military tactics for centuries; the need to finance his expensive gear gave the final impulse to nascent feudal institutions and set the temper of the chivalric class for centuries. Second, an agricultural revolution which stemmed from three disparate factors: (1) the diffusion among Germanic peoples of the heavy (sometimes wheeled) plow, whose colter, share and moldboard opened up the rich, heavy soils of the North and influenced the development of the strip system, cooperative farming and the nucleated village, all fundamental to manorial economy and social structure; (2) the adoption of the nailed horseshoe and "modern" harness, which made the horse more efficient than the ox in plowing and by speeding up wagon transportation, promoted agriculture, commerce and communications generally; (3) the substitution from the 8th century on of the 3-field (or -course) system for the alternate grain-fallow 2-field system, which reduced labor, encouraged assorting, increased production, and fortified standard diets with more plentiful legumes. The agricultural revolution sharply affected demographic patterns and the reorientation of European leadership from the Mediterranean northward. Third, the late medieval search for labor-saving gadgets which influenced incipient capitalism as the other revolutions had feudalism and manorialism. This search entailed experiments with the natural sources of power (water, wind, heat, gas, gravity, torsion, springs, magnetism) and the harnessing of power by machine designs

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This volume discussed the relationships between structure and the chemical and biochemical composition of cells. It includes a thorough presentation of the biochemical and morphological bases of modern advances in knowledge of the cell. The book is printed entirely on glossy paper to ensure fine reproduction of the photomicrographs, many covering full pages. (88 illustrations, including 62 photographs.) 1962, 248 pp., $2.95

Time, Cells, and Aging

By BERNARD L. STREHLER

Present information on aging is summarized and major areas for theoretical and experimental exploration are indicated in this work. Physiologists, gerontologists, biologists, and medical research workers will find this volume of major interest. The text concludes with a twenty-page, international bibliography. Glossy paper is used throughout to enhance the photomicrographs. (71 illustrations, including 38 photographs.) 1962, 270 pp., $2.95 (Cloth bound edition, $5.00)

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utilizing such devices as the crank, cam, flywheel, governor, and the like. Some experiments produced practical inventions: mills (grain, fulling, saw, forging), the treadle, the trebuchet, firearms, and fantastically elaborate mechanical clocks. Equally important for the future was the rise of a concept of power technology involving a conscious effort to apply means to a determined end; this underlay early modern science and the modern western world.

Professor White's wide erudition illustrates graphically two features of the history of early inventions: the basic importance of nonliterary sources and the utter complexity of cultural origins, diffusion, and assimilation. The Christian inventor, like the Christian Father, could despoil the pagan and in borrowing his ideas stamp them with his own genius. This is a book the layman can read with pleasure and profit; for the scholar the longer notes at the end (pp. 135–77) alone are worth the price of admission.—*James Lea Cate*

Physical Chemistry by MARTIN A. PAUL; 460 pages; $8.25; D. C. Heath & Co., 1962.

Physical Chemistry by ERIC HUTCHINSON; 647 pages; $10.00; W. B. Saunders Co., 1962.

After the succession of new textbooks in physical chemistry that have appeared during the past few years, in which the content and rigor of treatment have continuously increased, it is refreshing to read books that face realistically the fact that they are all designed for junior undergraduates receiving their first formal introduction to the subject. A comparison of these two texts, however, can only reflect the lack of uniformity of standards in colleges across the country. If, as is frequently the case, undergraduates have only one course in physical chemistry, teachers of the first-year graduate course will hope that their students have used Hutchinson rather than Paul.

To a considerable extent, the Paul text is "classical." It stresses thermodynamics in application, though the foundation leaves much to be desired. The same is largely true of the kinetic theory; the reader is left to accept on faith all but the more elementary relations. The absence of any treatment of quantum theory, quantum and statistical mechanics, yet their use in accounting for the heat capacities of gases and solids, and for the calculation of equilibrium constants, can only tax the memories of beginning students quite unnecessarily and perhaps render a disservice thereby to promising chemists. The author believes that the book content is all that can be covered adequately in a year course. If this be accepted, it is obvious that two years of undergraduate physical chemistry are necessary as a reasonable preparation for graduate study. The text is well written and numerical problems taken from the literature, many with answers, will tax most students. There are occasional slips due undoubtedly to the condensation in the writing. For example, the Einstein heat capacity equation for solids fails at low as well as at intermediate temperatures and it is the Debye relation that satisfies Nernst's T^3 law.

The Hutchinson text is intentionally a partial reversal to the classical treatment from earlier overemphasis on quantum and statistical mechanics. The author, however, is obviously appealing to the graduate as well as the undergraduate student. Anyone with experience in teaching the junior course will realize that over 600 pages cannot adequately be covered in one year. Two years of undergraduate physical chemistry seem inevitable, certainly for those who will ultimately major in this branch. For such students the Hutchinson text could be admirable but, as the author admits by inference in the preface, so also would other texts. The topics covered are essentially the usual ones though Hutchinson feels he has covered colloid and surface chemistry more than others have. The treatments of the topics, on the whole, are not new and novelty appears chiefly in the order of presentation. Hutchinson has deferred wave mechanics and statistics to the end of the book and, resultantly, the solid state theory is also deferred. One could have hoped that the importance of the

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Geology and Engineering (Second Edition) by ROBERT F. LEGGET; 884 pages; $12.50; McGraw-Hill Book Co., 1962.

The long awaited second edition of this important volume fills a gap in the engineering-geological literature as no other book. All geologists who advise and offer consulting services to the civil engineer will find it invaluable in supplementing their experience and training. All teachers offering courses in geology applied to civil engineering projects will find it essential in opening the eyes of students to the scope of geological disciplines in construction. These same conclusions apply to the civil engineer and his appreciation and utilization of geologists and their services.

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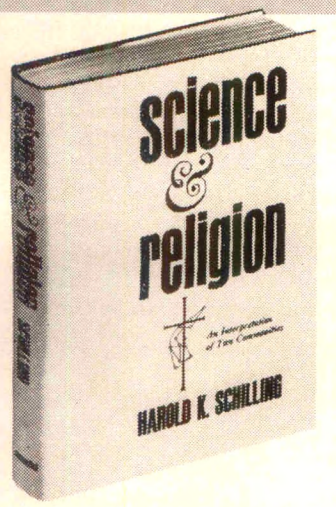
As the author states in the preface, "this book deals only with the interrelationship of geology and civil engineering—it does not deal with the principles or methods of geology." It does not, therefore, replace other fundamental geological books, but does supplement them in this most important field of application.—*Arthur B. Cleaves*

Life: Its Nature, Origin and Development by A. I. OPARIN, translated from the Russian by A. SYNGE; 207 pages; $4.50; Academic Press, 1962.

This essay has two objects. One is to define life and the other is to give a summary account of the ideas of the author on the origin of life. Most biologists will find the chapter on the definition of life just the kind of philosophical muddle they have been trying to avoid for some time. It has an old-fashioned argumentativeness that gives one the distinct feeling of getting nowhere at an alarmingly rapid pace. With all its dialectical materialism (mechanists, it turns out, are as evil as vitalists) it progresses no further (and is definitely less graceful) than the emergent evolution of Lloyd Morgan and the holism of Jan Christian Smuts.

In the section on the origin, Professor Oparin has brought his old views in line with all the modern advances of biochemistry. This section of the book is of considerable interest and of value. It is only marred by an unseemly aggressive assertiveness. Those people in the "current literature" are always making some ridiculous claim, while the author, who has been worrying about the problem longer than most, knows it must inevitably have happened in such-and-such a way. The sophisticated reader will know that it is all guesswork and, therefore, some of the fools may be nearer the truth than the wise men.

The final short section on the further evolution of life should have been eliminated entirely. The sections on development are particularly lacking in insight and can only be described as turgid. In fact, it is a pity that the author did not simply concentrate on his discussion of the origin of life. This



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is an area where he definitely has some worthwhile things to say.—*John Tyler Bonner*

The Statistical Processes of Evolutionary Theory by P. A. P. MORAN; 200 pages; $6.10; Oxford University Press, 1962.

The scope of this monograph is described as follows by the author:

"The treatment is mathematical and there is no extensive discussion of the theory in practice or of its significance for evolutionary theory." There is indeed more rigorous treatment than usual of certain mathematical problems that have arisen in its field but others of equal or greater importance are touched on only slightly if at all. A valuable feature is the abundance of references to earlier work, but this is not so complete that new contributions can always be distinguished from earlier work presented without citation.

The most serious inadequacy seems to be in selection theory. The rate of change in systems of multiple alleles with constant genotypic selective values is developed (without citation) and a few special cases are given in which these values are not constant; but the general theory for multiple interacting loci with genotypic selective values that may be functions of gene frequencies is not treated. Thus there is no treatment of the situation that must prevail in nature in which the surface of selective values has multiple peaks because of intermediate optima and pleiotropy.

There is also no discussion of mating systems or of isolation by distance. A treatment of the survival of single mutations is restricted to ones lacking dominance. The most serious misstatement noticed is that "with pairs of autosomal genes which are not linked, a stationary state is obtained in one generation." Actually, as shown by Robbins in 1918, this state is approached at a rate equal to the mean recombination percentage in the parents, which is 50 per cent in the case referred to.

On the other hand, extensive discussion of the long neglected topic of overlapping generations is welcome, though, as the author recognizes, his models have certain shortcomings. Among other valuable points are the presentation of the theory of the distribution of gene frequencies in finite populations, of the diffusion of favorable mutations through a species, and development of the consequences of migration between local populations in which selection acts in opposite directions. It should be a useful book on the topics which it includes.—*Sewall Wright*

Fluorescent Protein Tracing, edited by R. C. NAIRN; 280 pages; $9.00; Edinburgh and London: E. & S. Livingstone Ltd. (The Williams & Wilkins Co.), 1962.

To those who have used fluorescent protein tracer techniques or who anticipate using them, the problems of getting started are painfully apparent. What optical system is best? What light source is needed? What filters are required and where should they be placed? Which fluorochromes are best applicable to the problem? How are they conjugated to the protein? What are the problems associated with the technique? What are the advantages and disadvantages of the technique? Answers to all of these questions can now be found in one book.

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After the introduction by Dr. Nairn, the practical details of the preparation and purification of fluorescent conjugates, and a very brief theoretical consideration of fluorescence are given in Chapter 2. Chapter 3 deals with the properties of the protein conjugates and Chapter 4 describes the details of microscopy and photomicrography of fluorescent material. The rest of the book deals with its use. In Chapter 5 the use of direct tracing of fluorescent protein conjugates is described. Chap-

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ters 6 through 9, which make up over half of the book, deal with the immunological methods of fluorescent tracing. In Chapter 6 the general considerations of fluorescent antibody tracing are discussed. This is followed in Chapters 7 through 9 by description and critical evaluation of specific applications: bacteria, protozoa, helminths, fungi, viruses, rickettsiae, tissue antigens, and antibodies. Techniques of preparation, fixation, and staining are discussed.

This very readable book is highly recommended both as an introduction to fluorescent microscopy and as a handbook for the use of fluorescent tracing techniques.—*Frank A. Pepe*

A History of Chemistry, Vol. II by J. R. PARTINGTON; 795 pages; $20.00; St Martin's Press, 1961.

For the third time in his versatile life, J. R. Partington has undertaken the survey of an entire intellectual discipline. His big *Text-Book of Inorganic Chemistry* and five-volume *Advanced Treatise on Physical Chemistry* are both established reference works, and his present *History of Chemistry*, when completed, will constitute the most comprehensive review of that subject ever published. Four volumes are contemplated. Volume III "is in the press," while Volumes I and IV are "in a fairly advanced stage of preparation."

The main theme of Volume II (from Leonardo to Boerhaave) must necessarily be the waning appeal of Aristotelian explanations and alchemical objectives and the concomitant emergence of chemistry as a discipline in its own right, incorporating a fast growing quantity of precise empirical observations and leaning preferentially toward corpuscularian explanations. Yet nowhere does Partington explain why or how these momentous changes came about. Interpretation is left to the reader who is supplied with an encyclopedic review of the period's recorded chemical thought. The organization is largely biographical. A life sketch introduces each figure. All his known chemical writings are listed and selectively summarized with accompanying quotes, judiciously chosen, and massive footnoting.

Some one hundred figures thus pass in review. The titans (among them Paracelcus, VanHelmont, Stahl) stand alone in separate chapters or surrounded by a cluster of disciples. The lesser figures are grouped (not always convincingly) into chapters according to common interests. It is by this grouping, and even more by the space devoted to each man, that Partington imparts some degree of interpretation to his material. The book reaches its climax with Boyle, to whom sixty pages are alloted, while Galileo, whose views on matter were not without influence, gets only scattered asides. Some individuals, as Georg von Löhneyss, Oswald Croll, J. F. Henckel, formerly relegated to obscurity, have been raised to new levels of prominence.

This is not so much *A History of Chemistry* as a review of chemical literature unparalleled in breadth and precision. Henceforth the road to the sources lies through Partington.—*John J. Beer*

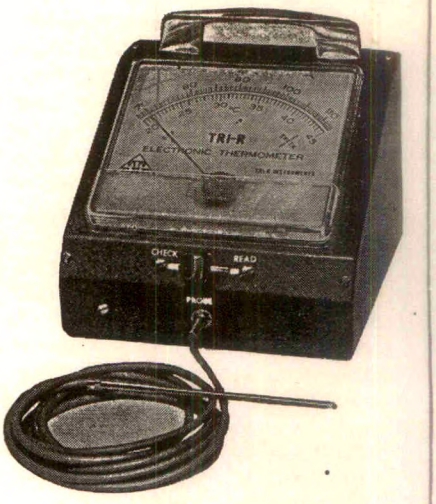
Physicist and Christian—A Dialogue Between the Communities by WILLIAM G. POLLARD; 178 pages; $4.25; The Seabury Press, 1961.

Science and religion are usually compared in terms of the methods used for searching for truth but these are confined to subject matter or to truth about reality which each represents. Dr. Pollard, in this book, shows that science, with special reference to physics, and and religion, with special reference to Christianity, are in essence communities and can be compared as to the characteristic kind of life a person is required to lead in order to survive in each community.

The first two chapters are devoted to the nature and function of these two communities and many comparisons showing the similarity of science and religion are made. In one such postulate, Dr. Pollard states that it is generally supposed that a person learns all about science or religion and then on the basis of such knowledge decides whether he

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wishes to become a scientist or a Christian. This is probably incorrect because more likely, a person gains real insight, understanding, and knowledge in a particular branch of science or in Christianity only after the decision has been made. Very few scientists today knew all about science before they decided to become scientists. Likewise, an exhaustive study of religion and theology does not usually precede one's decision to be a Christian.

In the last four chapters which deal with The Reality of Spirit, Nature and Supernature, Knowledge, and The Problem of Revelation, comparisons are made to illustrate how members of the communities of science and religion acquire knowledge of reality which the particular communities possess. Although the illustrations may not be infallible, they are presented in a logical manner, deserve careful meditation and are difficult to refute.

If there is a conflict between science and religion, this book does not resolve it. On the other hand, this book endeavors to prove that the knowledge of the truth of Christianity that a person can gain is on as firm and convincing ground as the knowledge a person can have of reality through science.—*Clark E. Bricker*

Penguin Summer, An Adventure With the Birds of the Falkland Islands by ELEANOR RICE PETTINGILL; 198 pages; 52 photographs; $5.00; Clarkson N. Potter, Inc., 1960.

The Whispering Land by GERALD DURRELL; 235 pages; $3.95; The Viking Press, 1962.

Each book describes an expedition by husband and wife to America's far south, with natural history the main interest. The Pettingills' was to take wildlife movies for Walt Disney, with collection of museum specimens incidental. The Durrells sought specimens for the author's private zoo on the island of Jersey, with incidental filming. Mrs. Pettingill was under contract to assist her husband and was invaluable

throughout, while Mrs. Durrell apparently went along for the ride and was invalided home following an accident. Durrell's "indefatigable secretary, Sophie," bore the burden of tending the flood of animals he kept bringing to Buenos Aires. The parties were in the Falklands five months, in Argentina eight months, respectively.

The Pettingills circumnavigated the islands, worked from two huts not far from Stanley, one on little Kidney Island, and lived five weeks ("most rewarding and exhilarating weeks of my life") with a family on New Island, a sheep ranch off the far coast. Though their Falkland stay was in late spring and summer, the weather was ordinarily abominable, high winds continual, storms of cold rain, and of snow frequent, sudden. The book tells interestingly of the three types of penguins and the other birds, mainly water birds—albatrosses, giant fulmar and other petrels, shags, flightless loggerhead and other ducks, skua, gulls—with but few small land birds. The Falkland thrush is closely related to our robin, and several birds (as turkey vulture and short-eared owl) are our own species. Most interesting, too, is the account of the people and their lives on that small remote British outpost. Unpleasant adventures were breaking Sewall's arm, and being marooned for a week on Kidney Island on starvation diet. The photographs are excellent illustrations of birds and scenes, rarely of the photogenic Pettingills.

The Durrells went far south, often over barely passable roads or none, across plains almost empty of visible animal life, to the Patagonian town of Puerto Deseado, seemingly dead but with a colony of a million penguins accessible, then to Peninsula Valdes swarming with wildlife—guanacos, rheas, armadillos, tinamous, foxes—and possessing colonies of fur seals and elephant seals, and crying to be made a sanctuary before it is too late, then far north to Jujuy, tropic with a correspondingly different fauna. The book is written humorously and is at times hilarious, contrasting with Eleanor Pettingill's brand of quiet humor. There is much of interest concerning the ways of wild creatures, though it is too bad to do no more than mention such characteristic South American birds as the seriama, and the red ovenbird and its unique nest. There are pen portraits of many interesting, often amusing people. It is doubtful that incidents or portraits lose anything in the telling. Ralph Thompson's sketches are illustrative and lively, usually excellent (though it would seem that he had never seen ovenbird or nest).

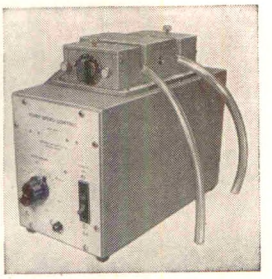
Naturalists and many others can acquire knowledge and entertainment from these volumes. Oddly, though Durrell quotes Darwin frequently, he never mentions Hudson.—*Charles H. Rogers*

Mark Catesby, The Colonial Audubon by GEORGE FREDERICK FRICK and RAYMOND PHINEAS STEARNS; 137 pages; 16 plates; $5.00; University of Illinois Press, 1961.

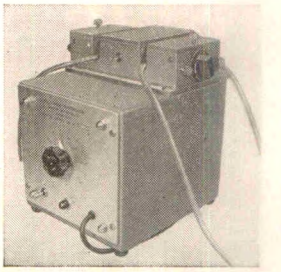
This book is long overdue. A century before Audubon and Wilson, the Englishman Mark Catesby spent seven years in Virginia, four years in South Carolina and the Bahamas, collecting and painting specimens of the flora and fauna and sending large quantities of specimens, mainly of the flora, to his sponsors in England. Back in England, he spent two decades at work which resulted in the publication of *The Natural History of Carolina, Florida, and the Bahama Islands* in two sumptuous volumes and an appendix, then the finest work on the New World's natural history.

While Catesby's work was used as a source book for subsequent work by Linnaeus and others, and his paintings, often crude by modern standards, were of a new style which may well have served as a model for Audubon, the man himself has been neglected and is little known. The present book remedies this. The authors did a vast amount of research on both sides of the Atlantic and have given us probably as full an account as the still existing material would permit of Catesby, his antecedents and background, his life, his friends and

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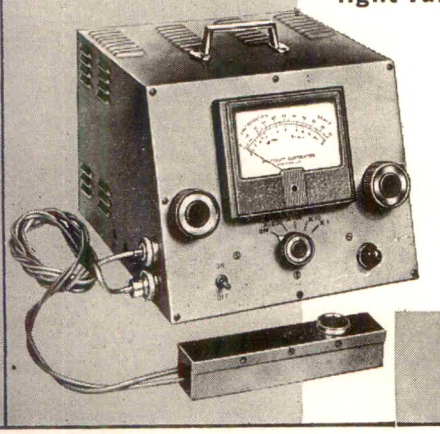
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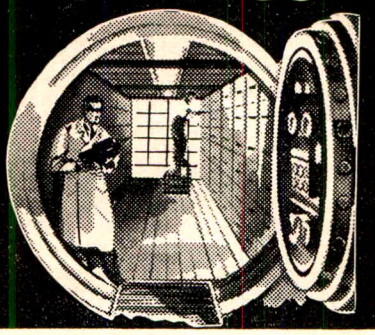
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sponsors, his travels and his work. This is followed by a thoughtful appraisal of "Catesby as a Naturalist" and his accomplishments in ornithology, botany, cartography, geology, anthropology and zoology. There is "A Selected Biography" and an appendix on "Thomas More and His Expedition to New England." The whole volume is copiously documented. The plates are reproductions of Catesby's paintings.—*Charles H. Rogers*

Volcanoes and Their Activity by A. RITTMANN, translated from the Second German Edition by E. A. VINCENT; 305 pages; $11.95; John Wiley & Sons, 1962.

The author of this book quite appropriately states that it is not a textbook in which all explanations that have been proposed are objectively presented. Rather it is, as the translator states, largely a synthesis of Professor Rittmann's own experience and ideas gained during many years of field research. The book does, however, present a generalized survey of the fundamental descriptive data necessary for the interpretations which constitute the major emphasis of the book.

The first eight chapters deal with subjects which might be anticipated from the title of the book but in the last two chapters one is unexpectedly, though still quite logically, confronted with the author's own speculative schematic picture of the processes that have led to such "grand" earth features as deep sea trenches, the folded rocks and batholiths of mountain structure, volcanic island arcs, mid-ocean ridges, the origin of continents and ocean basins and finally to cosmogony and the origin and composition of the earth as a whole. There is also a brief discussion of the origin of meteorites and of the craters of the moon. (The author believes they are volcanic). The hypotheses presented are, in view of inadequate data, necessarily controversial but they are stimulating and worthy of consideration.

There are over 500 active and tens of thousands of extinct volcanoes in the world. The reviewer notes that about 10 per cent of the active volcanoes are in

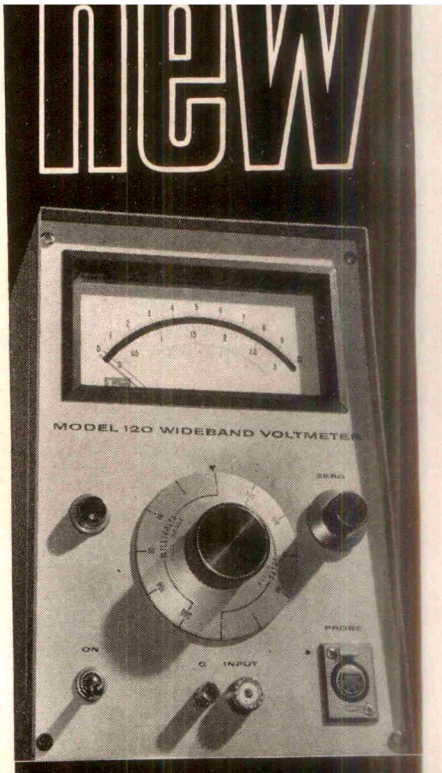
the 50 states of the U.S.A. but that he can count on the fingers of one hand the number of U.S. geologists who have systematically devoted themselves to a lifetime study of volcanoes. A careful reading of this book would surely serve to stimulate the interest in volcanoes needed amongst U.S. scientists from many fields.—*A. F. Buddington*

Viruses by KENNETH M. SMITH; 134 pages; $3.75 cloth bound; $1.95 paper bound; New York: Cambridge University Press, 1962.

In his little book on viruses, Dr. Smith brings the experience and perspective of a pioneer virologist to bear on such aspects of virology as the history and symptomology of virus diseases of plants, animals, bacteria, and arthropods, the methods employed in modern virology, and the spread and control of viral maladies. Such topics as latency, with examples illustrating the sinister potentialities of occult viruses, and the possible relevance of viruses to cancer are also treated.

There are sixteen pages of plates illustrating virus diseases, virus crystals, and especially pictures of viruses, in and out of cells, taken with the electron microscope.

Throughout the book, the material is presented in an engaging manner, and the author's wide acquaintance with the various fields of virology and virologists working therein is apparent. As stated in the preface, the author endeavored to deal with his subject with the minimum of technical description. This objective, together with the brevity of the treatment, results in an inevitable superficiality in a number of respects. More regrettably, numerous factual errors appear throughout the book. For example, it is wrongly stated that the nucleic acid of bacterial viruses is exclusively DNA, and that live poliovirus vaccine is not yet in general use. Several values given for the detailed composition and structure of tobacco mosaic virus are either wrong or obsolete, and a chemical composition is ascribed to vaccinia virus which seems rather to be that of influenza virus. Also the chronology of the discoveries of in-



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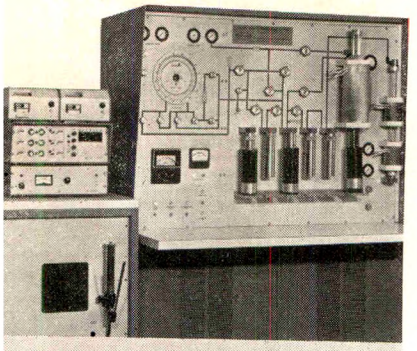
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fectious ribonucleic acid and reconstitution of tobacco mosaic virus are reversed. Nevertheless, the book is a good sketch of the modern field of virology, and should serve to whet the appetites of uninitiated readers (for whom a list of references applicable to each of the 12 chapters is provided), and remind specialists of half-forgotten principles or of promising but neglected areas of research.—*C. Arthur Knight*

Thermodynamics of Solids by R. A. SWALIN; 343 pages; $12.50; John Wiley & Sons, 1962.

This general treatment of the thermodynamics of solids will be of particular interest to chemical metallurgists, physical chemists, and persons concerned with solid state physics and the science of materials.

The first half of the work is devoted to a discussion of the basic concepts and tools of classical and statistical thermodynamics necessary for the treatment of various properties of pure solids and solid solutions. This includes the chemical energetics of forming and breaking bonds assumed to exist between atoms in solution and in certain order-disorder reactions. The advantage of this approach is that such quasichemical combinations may be regarded as *bona fide* chemical species subject to the law of mass action.

More important is the author's use of phase diagrams (which are plentiful) as a source of quantitative thermodynamic data (which are much more scarce). In connection with his treatment of the free energy of binary systems and their composition fluctuations in solutions, the author emphasizes the thermodynamic instability of any pure phase with respect to absorbing impurities from its environment. This has important ramifications for the production of hyper-pure materials. Entropy simply works against persons concerned with purification processes.

The second half of the work is devoted to some general aspects of the properties of interfacial boundaries and various questions connected with boundary impurity segregation and crystalline disorder. Properties like mechanical

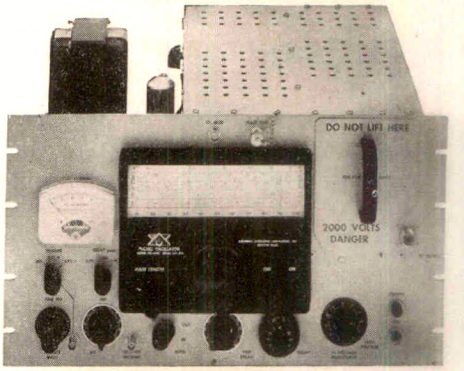
strength, kinetics of phase transformation, electrical conductivity of semiconductors and ionic crystals, luminescence phenomena, photoconductivity, and color, are all dependent upon the types and concentrations of various defects in the crystal. The author, therefore, takes up in some detail the statistical-thermodynamical aspects of atomic disorders such as lattice vacancies, interstitials, and foreign atoms (impurities). It turns out, for example, that point defects, like atomic vacancies, are thermodynamically stable at any temperature above absolute zero, whereas a line defect, like a dislocation, is thermodynamically unstable. It is nevertheless possible to control a given type of point defect by varying the concentration of other types of defect, i.e., by shifting the equilibrium according to the law of mass action.

For semiconductors the concentration of defects varies over much wider limits than for metals. Electronic defects (excess electrons in the conduction bands or holes in the valence band), and their interaction with atomic defects, also become of vital importance in the study of semiconductors and insulators. Again, the effective use of the law of mass action is illustrated in dealing with the problem of equilibrium between defects by treating electrons (e^-) and holes (e^+) as being analogous to the chemical constituents of a crystal. Similarly, good use is made of the notion of the chemical potential of electrons.

The last chapter is given over to the thermodynamic treatment of defects in compounds of both classic ionic and purely covalent nature. The emphasis here falls upon the compound semiconductors which have recently gained so much importance in technological applications of the maser and optical maser. In the discussion on non-stoichiometric compounds it is shown under what circumstances crystal defects, of the type found in the metallic halides, oxides and sulfides, are thermodynamically more stable than regular stoichiometric ionic compounds.

The subject matter of Professor Swalin's volume, though often considered in the European literature, has

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not been widely treated in this country. This summary of solid state thermodynamics will therefore be most welcome. Beyond this, its chief merit seems to lie in its approach to interpreting thermodynamic quantities according to atomistic models—wherever that is possible. Thus, the author skillfully combines classical thermodynamics, with its exclusive concern, at the level of gross phenomenological properties, in questions such as the chemical reactivity of one species for another, or the extent to which a given reaction will proceed toward completion, with the statistical mechanical approach which probes, at another level, into the detailed fine scale properties relating atoms and defects in crystals.

From both a fundamental and technological standpoint this work is an outstanding contribution to the scientific literature—most notably in the area of metallurgical thermochemistry. Only two criticisms will be made here: (1) The work contains too many misprints and mathematical errors of an elementary nature; (2) Since the book was intended for persons exposed to a formal course in thermodynamics, the author might have omitted many of the simpler mathematical derivations or posed them as exercises for the interested reader.—*Erwin N. Hiebert*

Radiation Protection in Mammals by JOHN F. THOMSON; 212 pages; $7.75; Reinhold Publishing Corp., 1962.

This rather specialized book presents a concise survey of major accomplishments resulting from attempts to prevent or ameliorate the effect of acute total body irradiation in mammals. As such, it should prove to be a valuable source of information for beginning students, experienced investigators, and physicians.

The nature of ionizing radiation, its effect on biological systems, and possible modes of action are discussed in brief introductory chapters. There is an excellent chapter on experimental design and criteria for evaluating the protective action of chemical compounds. Brief consideration is given to physical shielding and the oxygen effect, and four chapters are devoted to intensive discussion of various categories of compounds which have been tested for protective action against radiation. Primary attention is given to thiols since the best protectors belong to this class. The author stresses the pharmacologic effects produced by the various compounds and their possible relevance to the phenomenon of protection. Principal hypotheses concerning the mechanism of chemical protection are discussed critically, followed by a brief survey of acceptable methods for post-irradiation treatment of radiation injury in experimental animals and man.

The book has an index, is illustrated with tables of structural formulae, graphs of selected experimental data, and has few typographical errors. References to books, review articles, and the more significant original papers are listed at the end of each chapter, and will serve to introduce the reader to the extensive literature.

The author emphasizes that none of the well-known protective agents can be regarded as suitable for administration to humans because of toxicity and other complications. In this context the book concludes with a quotation from an earlier work by Dr. Thomson: ". . . total body shielding is the only protective measure which this reviewer would trust implicitly for himself."—*Benjamin D. Stinson*

Bird-Song (The Biology of Vocal Communication and Expression in Birds) by W. H. THORPE; 143 pages; $3.75; Cambridge Monographs in Experimental Biology, 12; New York: Cambridge University Press, 1961.

With the modern development of spectrographic analysis of sound and of high quality tape recordings, new pathways have been opened for the study of animal vocalisations, and it is mainly with these new experimental approaches and their results that this book is concerned. The rapid recent advances in this field are perhaps best illustrated by the fact that more than half of the papers cited were published during the last ten years.

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The author, a noted ethologist well known from his book on "Learning and Instinct in Animals," is uniquely qualified for his task. He has been a pioneer in the field, and it is not due to biased selection that the content of this volume is to a large extent based on his own work and that of his school. The book also profits tremendously from Dr. Thorpe's knowledge of music.

The volume starts out with a discussion of the questions whether bird song, or at least the song of some birds, can be considered song in a musical sense, and whether the vocalisations of birds are to be regarded as a form of communication, a sort of "language." Both questions are answered in the affirmative. The following chapters discuss form and function of call-notes, full song and subsong, the development of song in the individual from innate and learned components, specific and subspecific differences in vocalisation (dialects), and the importance of song as an isolating factor in speciation. A final chapter deals with the anatomy and physiology of the organs involved in sound production and perception. One criticism should be mentioned: the use of warning calls to scare away birds has only been mentioned in passing, and in view of its great practical importance this point might have received a somewhat fuller treatment.

As is to be expected in such a new field, many of the ideas put forward are based on spotty evidence only, and phrases

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like "may be due to" or "it appears that" turn up quite often. This is perhaps one of the most stimulating aspects of this book, and it is to be hoped that it will be instrumental in stimulating further research to fill the gaps of our knowledge, and to clarify many of the points tentatively suggested.

This authoritative account is indeed a welcome addition to the famous series of Cambridge Monographs. It will be indispensable for ethologists and ornithologists, and for anyone interested in communication or vocalisation in animals.—*Klaus Hoffmann*

Atoll Environment and Ecology by HEROLD J. WIENS; 532 pages; $15.00; Yale University Press, 1962.

The unusual features of atolls aroused the interest of early Pacific explorers and, shortly thereafter, these reefs gained recognition in the scientific world when Charles Darwin fitted them neatly into his brilliant theory for the origin of coral reefs. It was not, however, until after the Second World War that atolls became objects of intensive scientific study. In his volume, Mr. Wiens attempts to assemble and synthesize both old and new data. In this difficult task he has achieved considerable success. His deep interest in atolls is contagious and anyone, whether or not he has visited an atoll, will find this book intriguing.

The diverse data are arranged in 19 chapters, each with numerous subdivisions. Slightly more than half the volume is devoted to the physical setting—topography, geology, weather, currents, fresh water, and soils. The balance deals with marine and terrestrial fauna and flora; the final chapter describes man's place in the atoll environment.

The author's field work on atolls was done as a member of research teams sent out by the Pacific Science Board of the National Research Council. In supplementing his own observations he has drawn heavily on the results obtained by other team members and published in the *Atoll Research Bulletin*. The Board's work has shown clearly

that atolls which may look similar superficially actually may differ greatly in many ways. Broad generalizations are possible, but exceptions are the rule. The author recognizes that, in spite of all the work that has been done, atolls remain a fertile field for intensive biological and geological research.

Much of the physical setting of atolls is described and illustrated in detail in the first five chapters. The material might be more easily followed if a different arrangement had been used. Some of the data cited appear not to have been fully digested. Generalizations on drilling results are so broad as to be misleading. The chapters dealing with atoll life include a useful one on predatory and venomous species, a discussion of bird life and guano deposits, and a summary of the origins and distributions of plants.

The text, copiously illustrated with 93 figures, is followed by appendices dealing with wind frequencies, rainfall, typhoon effects, and terrestrial ecological communities. There is a bibliography of some 400 titles. Eighty-eight photographs are lumped at the end of the volume, an economical if inconvenient arrangement from the reader's standpoint. Included are excellent air views of reefs, close-up views of reef life, and many fine views of typical vegetation. Photographs of typhoon damage are impressive. Most photographs lack indications of scale and orientation, though in some these matters are covered in the captions.

The volume was prepared as a reference work for students and for administrative and navy personnel in the Pacific islands. It will certainly be useful to these groups and to all others who deal with atolls.—*Harry S. Ladd*

Progress in Polarography, edited by P. ZUMAN with I. M. KOLTHOFF; Vol. I, 355 pages; $12.00; Vol. II, 450 pages; $15.00; John Wiley & Sons (Interscience Division), 1962.

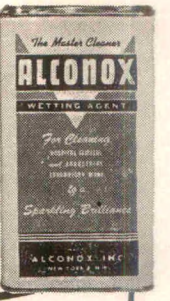
Publication of this work was originally planned to coincide with the seventieth birthday of Professor Jaroslav Heyrovský, who was awarded the Nobel

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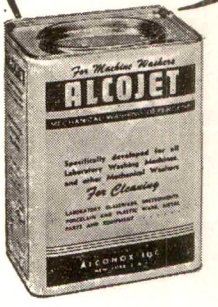


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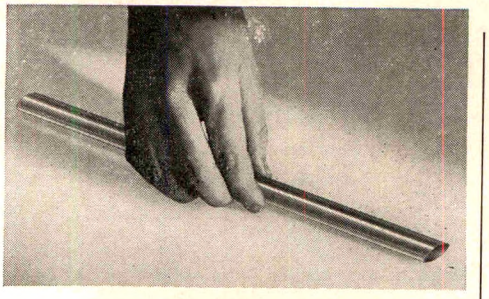
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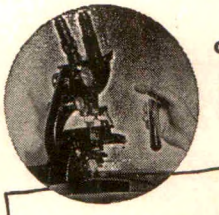
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Prize in Chemistry in 1959 for his discovery of polarography. The complications attending the collection of material for thirty-eight chapters from forty-three contributors in ten countries led, unfortunately, to a delay of over a year. As a consequence, readers acquainted with recent developments will observe that some chapters are already significantly out-of-date. Nevertheless, the work as a whole represents an important contribution to the polarographic literature.

These volumes are intended to provide authoritative treatments by specialists in various aspects of theoretical and applied polarography, with special emphasis on progress made during the past ten years. In several chapters, especially those of a theoretical nature, this aim is admirably achieved, with a logical presentation of refinements in theory or experiment beyond the limits of "classical" polarography. Unfortunately, however, some of the chapters amount to little more than a recitation of generalities and a listing of references with a decided bias towards the work of the author himself.

Most of the material dealing with fundamental theory and mechanisms of electrode reactions appears in Volume I, which contains several outstandingly good chapters of lasting value. Volume II, for the most part, deals with practical applications of polarographic analysis in various fields, and with equipment and instrumentation. Various "non-classical" techniques, such as square wave and pulse polarography, A.C. polarography, and linear voltage sweep methods, and various types of electrodes such as the rotated dropping mercury electrode, the hanging drop and solid electrodes are discussed in several chapters. In general, the treatment is essentially of review character, serving to collect together material otherwise scattered throughout the literature. Chapters on "Chromatopolarography" and on "Continuous Polarographic Analyzers" deserve special mention. Several chapters devoted to applications of polarographic techniques in various fields of science and industry proved disappointing, at least to a non-specialist.

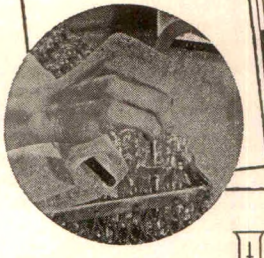
Unfortunately, the two volumes are indexed together. Otherwise, Volume I would be recommended to workers primarily interested in fundamental research, and Volume II to those interested primarily in analytical applications. As it stands, this work deserves a place in libraries of all institutions in which polarography is being used, and on the private bookshelves of serious investigators in the field.—*H. A. Laitinen*



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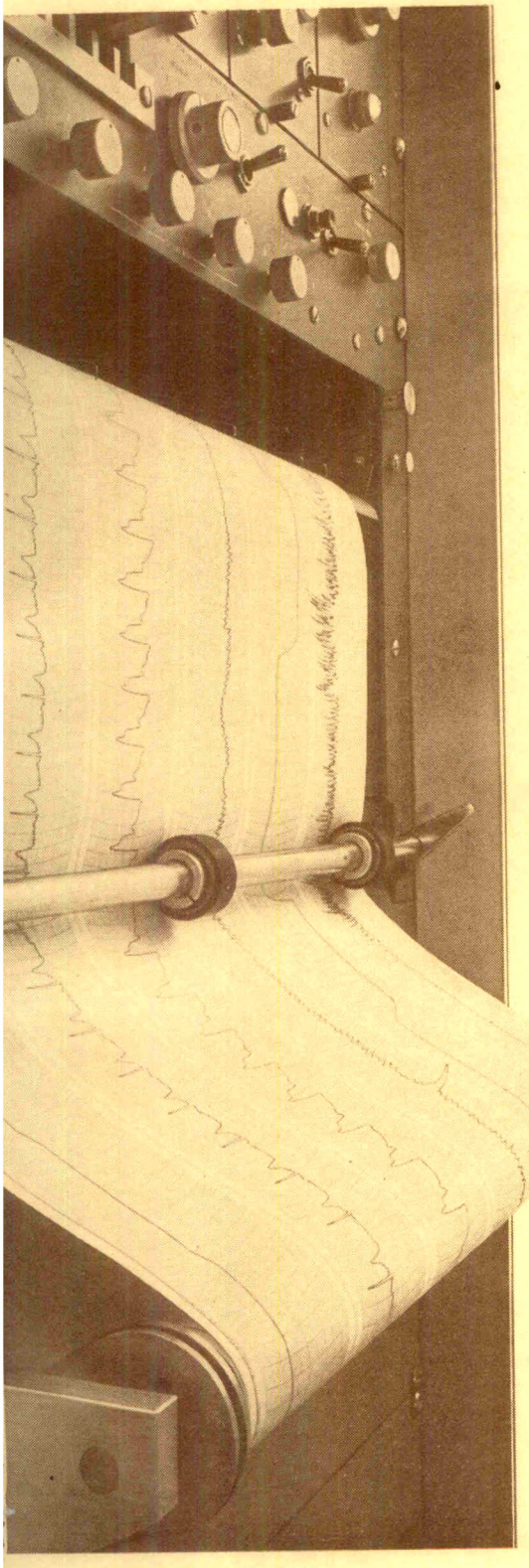
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